AUDIT REVIEW OF

PRELIMINARY TOTAL-SYSTEM ANALYSIS OF A POTENTIAL HIGH-LEVEL NUCLEAR WASTE REPOSITORY AT YUCCA MOUNTAIN

P.W. Eslinger, et al., 1993
Preliminary Total-System Analysis of a Potential High-Level Nuclear Waste Repository at Yucca Mountain
PNL-8444, UC-814. Richland, WA: Pacific Northwest Laboratory

Prepared for

Nuclear Regulatory Commission
Contract NRC-02-88-005

Intermediate Milestone No. 20-5702-061-330-030

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August 1993

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ACKNOWLEDGMENTS

This report was prepared to document work performed by the Center for Nuclear Waste Regulatory Analyses (CNWRA) for the U.S. Nuclear Regulatory Commission (NRC) under Contract No. NRC-02-88-005. The activities reported here were performed on behalf of the NRC Office of Nuclear Material Safety and Safeguards (NMSS), Division of High-Level Waste Management (DHLWM). The report is an independent product of the CNWRA and does not necessarily reflect the views or regulatory position of the NRC.

1 INTRODUCTION

The U.S. Department of Energy (DOE), through its contractors, is conducting Iterative Performance Assessment (IPA) exercises to demonstrate and test its methodology for total-system performance assessments (TSPA) of the proposed repository at the Yucca Mountain site, Nevada. These TSPA are conducted on an 18 to 24 month cycle, and the associated technical reports are submitted to the U.S. NRC. Earlier this calendar year, the DOE formally transmitted the TSPA report to the NRC for review and comment. The report, prepared by Battelle-Pacific Northwest Laboratory (PNL), is entitled "Preliminary Total-System Analysis of a Potential High-Level Nuclear Waste Repository at Yucca Mountain," PNL-8444, UC-814 (Eslinger et al., 1993).

In the letter from R.B. Neel to R.G. Baca of April 19, 1993, the NRC directed the CNWRA Performance Assessment and Hydrologic Transport Element (PA&HT) to conduct an audit of the PNL report. A multidisciplinary technical team was organized to conduct a technical review of various aspects of the PNL TSPA. The scope of the technical review was limited to a reconnaissance level examination of the methods, models, assumptions, and data used in the TSPA. No independent analyses were conducted to check the numerical results presented in the report.

As described in the report, the primary purpose of the PNL TSPA report was to develop mathematical models that provided sufficiently reasonable representations of isolation performance and were also amenable to probabilistic modeling. The second purpose of the TSPA was to make a preliminary evaluation of the impact of parameter selection on the total-system performance measure (i.e., the cumulative mass release of radionuclides to the accessible environment) and to compare the latter to the limits established by the U.S. Environmental Protection Agency (EPA) in 40 CFR Part 191 (EPA, 1985).

Eight categories of analyses were investigated by PNL: (i) Source Term; (ii) Unsaturated Zone Hydrology and Gas Transport; (iii) Saturated Zone Hydrology and Transport; (iv) Volcanic Intrusion Model; (v) Tectonic Disruption Model; (vi) Human-Intrusion Model; (vii) Cumulative Releases for the Total System; and (viii) Dose Model Description and Results.

The structure of the PNL report is: (i) Chapter 1.0 Introduction; (ii) Chapter 2.0 Overview of Modeling Approach; (iii) Chapter 3.0 Source-Term Analysis; (iv) Chapter 4.0 Unsaturated Zone Hydrology and Transport; (v) Chapter 5.0 Saturated Zone Hydrology and Transport; (vi) Chapter 6.0 Volcanic Intrusion Model; (vii) Chapter 7.0 Tectonic Disruption Model; (viii) Chapter 8.0 Human-Intrusion Model; (ix) Chapter 9.0 Cumulative Releases for the Total System; (x) Chapter 10.0 Dose Modeling Description and Results; (xi) Chapter 11.0 Summary; (xii) Chapter 12.0 References; (xiii) Appendix A - Distributions of Hydrogeologic Parameters for the TSA Problem; (xiv) Appendix B - Additional K_d Information; and (xv) Appendix C - TSA Problem Outline Consensus.

Technical review comments on the various chapters of the TSPA report follow. The comments are organized into four categories: (i) general comments; (ii) major problems; (iii) areas of possible controversy; and (iv) areas to be examined in greater detail. The authors of this review have suggested using a conservative approach to estimating TSPA of the repository.

1.1 REFERENCES

Eslinger, P.W., L.A. Doremus, D.W. Engel, T.B. Miley, M.T. Murphy, W.E. Nichols, M.D. White, D.W. Langford, and S.J. Ouderkirk. 1993. *Preliminary Total-System Analysis of a Potential High-Level Nuclear Waste Repository at Yucca Mountain*. PNL-8444, UC-814. Richland, WA: Pacific Northwest Laboratory.

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2 REVIEW OF CHAPTER 3.0 — SOURCE-TERM ANALYSIS

This chapter reports the engineered-barrier source-term release rates for a total of 10 nuclides (^{14}C, ^{79}Se, ^{99}Te, ^{126}Sn, ^{129}I, ^{135}Cs, ^{234}U, ^{237}Np, ^{239}Pu, and ^{243}Am). These were selected to represent a range of physical and chemical properties, inventories, and dose conversion factors (Eslinger et al., 1993, Section 2.3). The analysis was performed using the Analytical Repository Source-Term (AREST) code (Liebetrau et al., 1987; Engel et al., 1989), which could estimate release rates from the engineered barrier system (EBS) of an underground geologic repository for either spent-fuel or glass-waste forms.

2.1 GENERAL COMMENTS

This modeling exercise goes much beyond earlier attempts, that is, it includes release of ^{14}C both in the gaseous and soluble form and glass-waste forms. However, the authors state that the work reported does not provide a sufficient basis to establish a baseline performance assessment (PA). There are a number of other disclaimers provided in the main report, for example Section 1.6, page 1.3, paragraph 2, "Computer codes used must be subjected to formal Quality Assurance (QA) requirements." It is further stated that "...current models were used while still in the development and documentation stages." and that formal testing has not been completed. With these disclaimers in mind, the following comments are provided.

2.2 MAJOR PROBLEMS

The calculations shown in Section 3.1.1 for release in unsaturated zones do not appear to account for the possibility of extended exposure of the spent fuel to high temperature (250 °C+ depending on the age of the fuel and the areal loading of the repository) leading to a higher oxidation state of uranium dioxide. The environmental variables and the exposure time will determine the extent of spent fuel physical and chemical alteration. Qualitatively, the consequences will be a substantial increase in the surface area of the fuel (a factor of 2 to 3 orders of magnitude is not unreasonable) (Manaktala et al., 1991) and altered leaching characteristics of the resultant powdered spent fuel. Under such a scenario, which can result from failure of the container followed by an extended period of no water intrusion or by episodic (cyclic) events of water intrusion followed by drying-up of the repository, dissolution rates and solubility limits of altered spent fuel need to be used for calculating releases or it should be shown that the case considered is conservative. This does not appear to be the case in this report, which uses solubility values of individual elements as the controlling factors rather than those of the compounds.

The analysis uses an assumed container failure distribution (uniform from 2,000 to 5,000 yr) rather then attempt to model container corrosion. The simplistic assumption is understandable given the difficulty of modeling corrosion processes, but is clearly inadequate. If container lifetime is not predicted, no performance credit should be taken for container lifetime. It is not clear how the gap inventory is treated in the release analysis. No provision for gap inventory is made in the governing equations. Perhaps this is merely an omission in the documentation.

Equations (3.1) to (3.5) are poorly documented and appear to have mistakes. Equation (3.3) is said to apply when $t < 1/f_a$. This implies that spent fuel alteration begins at time=0, prior to container failure. Equation (3.4) is defined with $V(t_2)$ as the volume of the container when full. In the flow through release model, the appropriate value for $V(t_2)$ is a small number, much less than the volume of the container. This leads to a false dependence of release rate upon flow for alteration limited radionuclides (e.g, *Tc*) as seen in Figures 3.3 to 3.5. This discrepancy should be discussed with PNL. The estimates of radionuclide solubility and spent fuel alteration do not adequately account for the chemical environment and spatial variability inside the waste package. The waste package environment is likely to be highly variable in space and time. In contrast, solubility and spent fuel alteration rate are assumed to be constants applicable to dilute solutions. The values used thus lack technical basis and cannot be used to draw conclusions concerning system performance.

In Section 3.1.3, the rate term for glass dissolution should be more clearly explained, especially the relation between Eqs. (3.10) and (3.12) and the conditions under which each applies. Presumably, Eq. (3.12) is to be used close to equilibrium where Eq. (3.10) could yield a zero rate or even precipitation of glass, which would be a nonphysical representation of the phenomena under these circumstances.

In Section 3.3, paragraph 2, it is stated that the solubilities calculated from EQ3/6 tended to differ from values used in other analyses. Examples provided are: the solubility of ^{237}Np at 44 °C used in the analyses is 3,160 times greater than the solubility used for ^{237}Np in the preliminary site-suitability study referred to in Lee et al. (1991). Why was this particular factor used? In the same section it is stated, "On the other hand, the Pu solubilities were four orders of magnitude lower than those used by Wilson (1991)." It is also stated that "The difference in solubilities lead to similar differences in the calculated release rates." Although pointing out these differences is helpful to the reader, no explanation for using these widely different values is provided. Without a detailed clarification, the reader is unable to make any sense out of quantitative release values computed by the code.

2.3 AREAS OF POSSIBLE CONTROVERSY

The treatment of container corrosion is an area of concern. Both the PNL work and the Sandia National Laboratories (SNL) TSPA used assumed-container-failure profiles. While this may be appropriate in initial work, the recommendations for future work do not contain suggested improvements in the corrosion analysis. The clear implication is that the DOE intends to take credit for several thousand years of container lifetime in the absence of quantitative analysis of corrosion rates. This issue needs to be discussed with the DOE. Studies of waste package chemistry suggest an aqueous environment consisting of water in films and small pores with a composition ranging between dilute solutions and concentrated brines with strong spatial and temporal variability within and between waste packages. In this context, it is not clear how defensible estimates of solubility and release rate can be obtained. Availability and applicability of values for solubility and alteration rate is a fundamental postulate in the mass transport approach to release rate analysis as developed in the AREST code (Apted et al., 1989). Rather than proceed with ever increasing sophistication in this direction, perhaps a step back should be taken and the technical defensibility of the mass transport approach for Yucca Mountain conditions be carefully examined.

The report refers to Working Draft 4 of the revisions to 40 CFR Part 191 (EPA, 1985). This presumably is the last available revision/proposed revision. According to this revision, it is stated that EPA is considering a population dose criteria as an alternative to the cumulative release criteria. In addition, the individual protection criteria is extended to 10,000 yr after repository closure. These criteria require dose modeling for both individuals and populations. (The revisions being considered for 40 CFR 191, appear to make the regulation more stringent.) However, the report states that the doses were estimated using models and scenarios that were designed to estimate cumulative release of radionuclides rather than dose (Section 2.4, page 2.5, paragraph 2).

2.4 AREAS TO BE EXAMINED IN GREATER DETAIL

Estimates of container corrosion rates are absent. Technically defensible approaches to a container lifetime prediction (or bounding) should be investigated. One example of a promising approach is the use of the corrosion potential as the unifying concept for corrosion estimates, a concept currently being developed at the CNWRA (Sridhar et al., 1991).

The report considers releases from glass wasteform and spent fuel separately. What will be the effect of co-mingling the two in the repository? Such a disposal scenario is quite possible for the high-level waste (HLW) repository.

The waste package design is based on the Site Characterization Plan (SCP) conceptual design (DOE, 1988). Calculations similar to those shown in this report, that is, complementary cumulative distribution functions (CCDF), need to be recalculated based on the advanced conceptual design (ACD) of the waste package (Stahl, 1992). It is possible that SCP conceptual design will be officially replaced by the ACD as early as Fall 1993.

The glass inventories used for the calculations are based on SRL-202 glass. Is this glass representative of the majority of the vitrified waste that is likely to go into the repository? It should be noted that glass from West Valley Savannah River and Hanford Reservation may have different glass composition. It is not clear why only J-13 well water was used which may not be representative of the Yucca Mountain vadose zone. Would the use of de-ionized (DI) water provide higher releases for the same scenarios? If so, how much higher? Will release of radionuclides in DI water provide bounding estimates for the Yucca Mountain site? If so, why are they not used? If not, how can the upper bound of the release be calculated?

Although the code calculates release for both glass wasteform and spent fuel, there is no consideration of groundwater geochemistry alteration due to releases from one type of HLW or the other. For example, short-term releases from glass wasteform will increase the pH of the groundwater substantially. How do the solubilities of radionuclide species released from spent fuel in such a modified groundwater compare with J-13 Well water or DI water? The code, which uses release rate models which are controlled by solubilities of the radionuclides, will surely be influenced by the possible scenario described above.

In Section 3.4, it appears that the report only addresses ^{14}C releases from the fuel. Almost 50 percent of ^{14}C inventory in the spent fuel assemblies is activated crud and corrosion products on the cladding (Manaktala, 1993). Release of ^{14}C from the cladding as a function of time needs to be incorporated into the model, in order to make the ^{14}C releases more accurate and the analyses useful for comparison with the regulatory requirements.

Releases from glass wasteform and spent fuel in colloidal form have not been addressed by the code. Such releases are not bound by the solubility limit of the radionuclides in solution in the groundwater and have the potential of being a concern (DOE, 1993; Manaktala, 1992) in demonstrating compliance with release rate limits specified in the NRC regulations.

Comparing Figures 3.5 and 3.6, releases calculated by the "Wet Continuous Model" appear higher than those calculated by "Flow-Through Model" for releases from spent fuel (although the water infiltration rates are different). This is especially true for releases in the 10,000+ yr time frame. Why are the total releases (area under the curve) different for the two models? It appears that cumulative releases of Tc, Sn, Cs, I, Se, etc., are very much higher as calculated by "Wet-Continuous Model" as compared to the "Flow-Through Model." What's the reason? The total amount released at time approaching infinity should equal the total inventory of the wasteform/spent fuel (ignoring decay, which is a reasonable assumption for long-lived radionuclides). Comparison of Figures 3.9 and 3.10 shows the calculated release of Pu to be very much higher for the "Wet Continuous Model" as compared to the "Flow-Through Model." Such a dramatic increase in release of Pu is not exhibited by the spent fuel. How can this be explained? Can this be verified using existing experimental data or via additional lab experiments?

Release of only a limited number of radionuclides has been calculated. Analyses of other radionuclides need to be performed.

Alternatives to the mass transport approach to release rate of soluble radionuclides should also be examined. Two potential aspects are: (i) 10,000 yr waste package; and (ii) extended dry-out of the repository horizon. At the same time, the assumptions that form the critical basis for the mass transport approach to release rate should be examined. Alternate approaches to TSPA, with less emphasis on calculational strategies and production of CCDF, must also be considered. It would be desirable to consider the advantages and disadvantages of: (i) an extended dry period; (ii) long lived waste packages; and (iii) the traditional mass transport approach to release. Additionally, an attempt to address the importance of disruptive events (e.g., seismic, volcanic) relative to the undisturbed case should be pursued further.

The report does not compare the calculated results with regulatory requirements (NRC, 1992). Although some passing reference has been made to this subject (see Summary section on page iii), it is stated that the dose results presented here indicate that the potential repository may be able to meet both individual and population dose criteria. A more rigorous comparison is required.

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3 REVIEW OF CHAPTER 4.0 — UNSATURATED ZONE HYDROLOGY AND TRANSPORT

This chapter reports two deterministic based approaches for modeling a two-dimensional vertical cross section of the unsaturated zone at Yucca Mountain, that included the Ghost Dance fault (DOE, 1988). For the liquid-phase approach, (i.e., Section 4.1 — 4.2.4) the unsaturated zone was modeled as an isothermal, liquid-phase hydrogeologic system for the purpose of simulating radionuclide transport from the potential repository horizon to the regional water table. Analysis of groundwater particle pathlines and travel-times from the repository to the water table were based on near-steady-state flow conditions. For the vapor-phase approach (i.e., Section 4.3), the unsaturated zone was modeled as a nonisothermal, multiphase system while simulating transport of a single radionuclide (^{14}C), from the potential repository to both the regional water table (liquid phase) and to the earth's surface (vapor phase). The same hydrogeologic data was used in both approaches

3.1 REVIEW OF SECTION 4.1 — LIQUID PHASE

3.1.1 General Comments

Although the results of the reported PA investigations related to the hydrology of the unsaturated zone at Yucca Mountain are intended to be preliminary, the mathematical approach is deemed to be superficial.

3.1.2 Major Problems

In the attempt to address the impact of a number of surface recharge related scenarios on groundwater travel time (GWTT), the investigators have selected SUMO, Eslinger et al. 1990, a two-dimensional (2D) numerical model, to perform the steady-state groundwater flow analyses. The erratic performance exhibited by SUMO appears to have somehow prevented the investigators from achieving their basic objectives.

Numerical solutions of the adopted conceptual flow model are likely to give rise to convergence problems, due to a large extent to the highly nonlinear nature of the five lithologic units' unsaturated material properties. SUMO's solution algorithm seems to have difficulty providing tenable solutions, and this state of affairs is highlighted by the authors' diagnostic emphasizing the occurrence of convergence problems with increasing rate of recharge. Out of the five selected recharge rates, SUMO seems to have yielded a convergent solution only once (i.e., for a recharge rate corresponding to 0.01 mm/yr), whereas approximate solutions were obtained for recharge rates not exceeding 0.1 mm/yr. Note that the specific meaning of the term "approximate solution" is not clearly characterized in the report.

This state of affairs suggests that, in the model verification process, test cases involving infiltration in stratified materials have not been adequately investigated, which then places in question the reliability of the model.

No major effort seems to have been expended on the part of the investigators to mitigate such a deficiency (e.g., mesh refinement, alteration of the lower boundary condition, etc.), in order to perform a comprehensive hydrologic study of the site, except for a peripheral one-dimensional (1D) flow analysis using the MSTS model. It would have been not only desirable but compelling to see, for example, the multi-dimensional model MSTS, which seems to have satisfactorily solved the 1D form of the governing groundwater flow equation, should be used as an alternative to SUMO.

The sole conclusion reached by the investigators is summed up in a brief statement underlining the decrease in travel time registered by a set of nine particles released from the repository horizon as a result of a 100-m water table rise from the baseline case.

As far as the radionuclide transport investigations are concerned, the authors have simply relied on the pathlines and GWTT obtained from the steady-state flow field to estimate radionuclide travel time. The section on radionuclide transport sums up to a single paragraph (Section 4.2.3, page 4.15). There is no mention of the nature of the radionuclide to which they refer; it can only be hypothesized that it must be a nondecaying and nonreacting species. They conclude that for recharge rates not exceeding 0.1 mm/yr, "no radionuclide mass reached the saturated zone within 10,000 years."

A major benefit registered from these investigations was the discovery of SUMO's reported numerical deficiencies, which might assist its authors in their revision process. As reported in Section 2.4.2, two mathematical models, that is, SUMO and MSTS, were used in the course of these investigations. Although a reference to SUMO is cited, the one related to MSTS is missing. Moreover, an extended summary of these codes would have proven beneficial to the reader.

The equation designed to predict the composite hydraulic conductivity-suction head relationship given by Eq. (4-2) (with some duplicate printing), which was first proposed by Klavetter and Peters (1986) is not referenced. The reader should have been warned that this equation is unproven and does not account for either the fractures' orientation or length, not to mention the respective density functions of the four major parameters characterizing a fracture network. Consequently, short of a formal derivation, the validity of such a composite hydraulic conductivity-suction head relationship will remain questionable.

The cross derivative terms in Eq. (4.8) have been ignored. Justification for neglecting the off diagonal terms of the hydraulic conductivity tensor, based on the assumption that the principal directions of the hydraulic conductivity tensor are aligned with the coordinate axes, is not warranted under the circumstances, particularly when the fractured medium has been assimilated to a single equivalent-continuum model. This approach assumes, on the one hand, an ideal homogeneous soil, and above all, an *a priori* knowledge of the exact directions of the principal permeabilities on the other (Long, 1983).

Symbol S_s in Eq. (4-10) must be defined as specific water capacity, whereas parameter ψ is redundant and could have been substituted by H, since the latter corresponds to the suction head (i.e., $H < 0$). Equations (4-9) and (4-10) are not conditional upon the value of the volumetric moisture content as reported, but rather on the sign of the dependent variable, that is, pressure head.

3.1.3 Areas of Possible Controversy

Fluid density, ρ, appearing in Eq. (4.6) gets converted to R in Eq. (4.8), which is now defined as the ratio of fluid density. This is rather confusing, particularly when single phase fluid flow, under isothermal conditions has been investigated.

In Figure 4.7, the value of the pressure for the baseline case at the water table elevation (730.0 m) is greater than zero. Justification for assuming a positive value must be given. Moreover, a verification of the reported MSTS 1D results against the analytical solutions given by Childs (1967) and Childs and Bybordi (1969) for recharge rates not exceeding 0.1 mm/yr would be desirable.

In all the reported flow investigations, the lower boundary of the flow domain has been assumed to correspond to the water table, where the pressure head is zero. From first principles, it is known that when the region above the water table becomes progressively saturated, such a boundary condition will be in violation with the physics of the problem, and hence the analysis is no longer valid. It is an irreconcilable fact that the investigators have overlooked this important issue, particularly for recharge rates in excess of 0.1 mm/yr, by not attempting to run the 2D version of SUMO using an extended flow domain, which would have encompassed both saturated and unsaturated zones of the modeled region.

3.1.4 Areas to be Examined in Greater Detail

First, and foremost, the SUMO code must be revised and improved should the investigators decide to use it in future TSPA investigations. For high recharge rates the saturated portion of the aquifer should become an intrinsic part of the modeled flow domain. In addition to GWTT, it is recommended that two additional information needs, cumulative water storage and cumulative mass flux be included in future PA investigations. The first will provide first hand information regarding the impact of infiltration on the transient water storage at the repository horizon level, and the second will provide estimates of the transient radionuclide mass transfer past some observation points or compliance boundaries in the flow domain considered.

The pair of investigated scenarios, restricted to variations in the upper and lower boundary conditions of the flow domain, should be broadened so as to encompass the numerous other parameters of the conceptual model.

When dealing with such a critical issue as groundwater pathline and travel time in the unsaturated zone of the future HLW repository, it becomes mandatory that Monte Carlo or other stochastic simulations be performed, particularly given the sparsity of the data to be analyzed and the uncertain nature of the available data. A deterministic or preferably a probabilistic sensitivity analysis of the selected performance measure with respect to the system parameters should also be performed.

3.1.5 References

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3.2 REVIEW OF SECTION 4.3 — GAS PHASE

3.2.1 General Comments

Section 4.3 has been reviewed and, in general, found to be internally consistent and consistent with the main body of the document. Detailed comments are included in the following sections.

3.2.2 Major Problems

No major problems have been identified with the technical approach taken by PNL in Section 4.3 other than the observation that their approach is viewed as not going far enough. This has been interpreted to be an area of possible controversy and appropriate comments have been included in that section.

3.2.3 Areas of Possible Controversy

The gas phase analysis can be divided into two major parts: (i) the hydrologic portion which includes the analysis of heat and water flow; and (ii) the geochemical portion which includes the analysis of the transport of ^{14}C through the system. The two groups will be commented upon separately in this section.

The hydrologic analysis consisted of a series of numerical simulations that were designed to predict the temperature and liquid saturation for different surface boundary conditions. In the first case, no recharge was assumed, while a recharge rate of 0.01 mm/yr was assumed for the second case. All other properties and assumptions were held constant between the two cases. The transport of ^{14}C is predicated on the thermal and hydrologic regimes predicted using these models.

Other analyses (Ross, 1988; Green et al., 1992), however, indicate that the flow of heat by convection and the movement of water as either a liquid or a gas are highly dependent upon the properties of the medium, in particular, the permeability values assigned to the subsurface. The mean hydrologic parameters listed in Table 4.1 were used without exception in all calculations. However, as demonstrated in these other analyses, alteration of the hydrologic properties can result in substantially different predictions of the liquid saturation and thermal subsurface regimes, which, in turn, would provide substantially different predictions of the transport of the radionuclides. Since the transport of radionuclides is highly dependent upon the hydrologic properties assigned to the subsurface, it is advised that future assessments of solute transport evaluate the effect of different, reasonable values for the hydrologic properties.

The assumption of a constant temperature boundary at the water table may not be valid. Recent analyses by Buscheck and Nitao (1993) indicate that the thermal impact of the emplaced waste could be manifested several hundred meters into the saturated zone. A different boundary temperature could consequently affect the liquid saturation and thermal regimes in the overlying unsaturated zone.

The repository was modeled with a uniform areal power density (APD) of 76 kW/acre. Recently, a concept referred to as an extended-dry repository with an APD as high as 114 kW/acre has been proposed as a technique to maintain the repository at low saturations for longer periods after waste emplacement (Buscheck and Nitao, 1992; 1993). Because these higher power loads have a significant impact upon the liquid saturation and thermal regimes (and subsequently radionuclide transport), the model simulations should take them under consideration.

Equilibrium of chemical species is assumed to exist between the solid, liquid and gaseous phases. A retardation factor [Eq. (4-25)] was introduced to accommodate the delay in transport resulting from liquid-gas equilibrium. This approach was taken from Amter et al. (1988). However, neither the general approach of incorporating a retardation factor into the transport equation nor the data upon which the retardation factors were calculated are documented in either the subject document or the referenced (Amter et al., 1988) document. Additional support for this approach is needed since the transport of carbon in the gas phase is highly dependent upon the chemical nature of the system.

The assumption of chemical equilibrium is probably valid in systems that do not exhibit large liquid or gas flow velocities. In general, however, the assumption of chemical equilibrium is not conservative. Therefore, additional quantitative information supporting this assumption should be included or made available.

3.2.4 Areas to be Examined in Greater Detail

Effects of variations in media properties on the thermal and liquid saturation regimes should be examined more thoroughly. It is possible that analyses in addition to these (i.e., additional property changes, boundary conditions, etc.) will be required in the analysis of thermal buoyancy and gas phase flow.

A more complete inspection of gas phase chemical kinetics might permit relaxation of the assumption of thermodynamic equilibrium assumed in the Amter et al. (1988) analysis.

3.2.5 References

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4 REVIEW OF CHAPTER 5.0 — SATURATED ZONE HYDROLOGY AND TRANSPORT

This chapter reports the modeling results for water movement and solute transport in the saturated zone below the potential Yucca Mountain Repository. Groundwater flow was assumed to be horizontal, under isothermal, and steady-state conditions. Stochastic and deterministic simulations of the flow field were used, to predict radionuclide migration associated to human-intrusion release-cases scenarios.

4.1 GENERAL COMMENTS

While recognizing that this document is intended to be a very preliminary TSPA, there is still a need for the authors to provide a more extensive background description of current conceptual models of the regional hydrogeologic regime at Yucca Mountain. A brief review of portions of the seminal work by Winograd and Thordarson (1975) would greatly aid in establishing a clear picture of the hydrogeologic setting, and perhaps strengthen the basis of the assumptions regarding the hydraulic regime of both the tuff and upper carbonate aquifers.

There is distinction made between so-called "stochastic" runs and "deterministic" runs of the aquifers in Chapter 5. The basis for this distinction is not clearly described in this chapter which makes the continued use these terms extremely confusing.

On page 5.1, fourth paragraph, the assertion that water moves downward from the tuff aquifer to the carbonate aquifer is neither supported by citation nor by a description of the physical or geochemical basis for this assumption. Moreover, throughout this chapter, the two aquifers are treated individually suggesting the absence of a significant hydraulic connection.

In the first paragraph on page 5.2, twelve simulations quite simply will not provide "an adequate statistical description of the probability of radionuclide transport." Rather than imply that these repeated realizations of the aquifers constitute a Monte Carlo study from which statistics can be generated, the authors should point out that by conducting a number of simulations the likelihood of obtaining fast flow paths and, therefore, conservative PA results is increased.

On page 5.2, second paragraph, how, exactly, are the fracture and matrix hydraulic conductivities weighted for the equivalent-continuum model used here? Also on page 5.2, third paragraph, the assumption that porosity in either the tuff or carbonate aquifers is spatially constant will profoundly affect the GWTT estimates. Justification for assuming constant porosity should be given. Also, the RFIELD code is not referenced.

The third paragraph, from line 6 to the end of the paragraph on page 5.2 is very confusing. How, exactly, were the gradients for the tuff and carbonate aquifers statistically determined? The basis for assuming a uniform distribution of regional hydraulic gradients must be clearly outlined.

On page 5.3, Table 5.1, the document should provide units for each of the rock and hydraulic properties should be provided. The rock density ranges from 2.76 to 2.23 and it is either specific gravity or units of gm/cc. Assuming that dispersivity has units of length and refers to effects of mechanical dispersion, is diffusivity equivalent to hydraulic diffusivity (L**2 / T) or the coefficient of molecular diffusion? Also, east and west must be marked on these plan views of the aquifer domains. Is the division of the tuff aquifer into a partially welded zone and a zeolitized zone the result of the shallow west-east dip of the units (6 degrees) exceeding the west-east dip of the water table? This requires further explanation.

On page 5.4, Table 5.2, since each hydraulic conductivity realization is associated with a separate regional hydraulic gradient realization, it is impossible to distinguish the effect of each on GWTT.

Concerning the last paragraph page 5.5, why were only seven particles tracked through the velocity field generated by each of the aquifer realizations? Clearly, seven particles are not enough to compile meaningful statistics for determining GWTT distributions. If the purpose of performing the particle tracking was simply to delineate those realizations whose GWTT was significantly less than the 10,000-yr regulatory period, and thus would be most likely to violate the EPA standard, it should be explicitly stated.

On page 5.10, second paragraph, why were any of these results reported in view of the obvious deficiencies in the random field generator used to produce the log-K realizations? Can this be justified on the basis of the results being conservative from the standpoint of radiological safety? Also on page 5.10, second paragraph, the description of the coding error in the random field generator appears to imply that the correlation length parameter was mistakenly interpreted as the number of grid spacings over which the field was correlated rather than the number of length units. However, this does not explain why the coarser grid spacing in the x-direction generates long, linear features only in the vicinity of very fine grid spacing in the y-direction. This section either requires more explanation or all fields should be regenerated.

On page 5.11, second paragraph: Does simulation T12 produce the shortest GWTT of the twelve tuff aquifer simulations due to its having the highest regional hydraulic gradient, the most conductive paths, or some combination of these two? If the shortest GWTT is solely the result of the high imposed head gradient, it would seem that because the flow regime is essentially 1D, more effort should be devoted to accurately determining the distribution of the regional gradient and less on the conductivity field.

Page 5.12, first paragraph, the assumption that the time of drilling intrusion is log-normal implies that drilling is more likely to occur immediately following closure than at some time in the distant future (10,000 yr). What is the basis for this assumption? Is it employed since early intrusion would undoubtedly produce more conservative results?

A clarification is needed on page 5.12, third paragraph, regarding the twelve steady-state flow fields. They are presumed to be those for the carbonate aquifer, since only realization T12 for the tuff aquifer violated the 10,000 yr travel time rule, but this needs to be further explained.

On page 5.12, third paragraph, explain what is meant by the phrase, "[t]he porosity and density were consistent..."

On page 5.13, in Table 5.4, assuming that the sorption values referred to here are equilibrium distribution coefficients, why aren't the k_d values for the zeolitic tuff layer greater than those for the partially welded tuff unit? One frequently cited attraction of the Yucca Mountain site is the presence of the highly chemically sorbing, zeolitic bedded tuff unit which underlies the repository horizon. Also on page 5.13, first paragraph, why are both the transverse and longitudinal dispersivities set equal to 1 (meter?) for the 10 stochastic carbonate aquifer simulations?

In the first paragraph on page 5.21 it is very difficult to understand the difference between the so-called deterministic and stochastic realizations of the carbonate aquifer. Where is the stochastic nature of the stochastic realizations to be found? Does it arise from the conductivity field, the hydraulic gradient, the sorption values? Why are the dispersivities two orders of magnitude larger for the stochastic realizations? Also on page 5.21, fourth paragraph, the assertion that technetium (Tc) is highly sorbing appears to be a typographical error. This assertion is contradicted by Table 5.4 on page 5.13.

4.2 MAJOR PROBLEMS

The basic approach taken in this preliminary TSPA is to conduct a few simulations using very complex models instead of using simpler models and conducting the many simulations required to perform a complete Monte Carlo analysis. It is thus confusing when statistical or Monte Carlo sampling procedures are used to generate alternative realizations of the hydraulic conductivity field, regional gradient, time of intrusion, and sorption coefficients. Since so few simulations are to be performed for the TSPA, it is unlikely that statistical methods will sample the tails of the distributions and thus lead to extreme scenarios in which the EPA rule would be violated. When using a few, complex simulations it would be best to simply choose reasonable upper bounds on the hydraulic and chemical properties, namely: large hydraulic conductivities, high hydraulic gradients, early time of intrusion, and low sorption values. Attempting to make do with very few stochastic realizations of these properties increases the likelihood that extreme values will not be sampled and thus the results of this TSPA are likely to be extremely nonconservative.

4.3 AREAS OF POSSIBLE CONTROVERSY

The decision to produce a final document when there are known deficiencies in the computer code used to generate the random hydraulic conductivity fields may require further justification. If publishing can be justified on the basis that the incorrect hydraulic conductivity field realizations are apt to produce conservative results, then it should be explicitly stated in the document.

Choosing not to use different sorption values for the zeolitic and partially welded tuff aquifers is a questionable decision. The sorption values reported in Table 5.4 for the tuff aquifer may be from zeolitic tuffs, in which case those sorption values for the partially welded unit are too high; or, if the sorption values are based on the partially welded units, then those for the zeolitic unit are too low. In the latter case, one may argue that the results can be justified on the basis that they would be conservative from the standpoint of radiologic safety. However, if the former case is true, the TSPA results are clearly not conservative.

4.4 REFERENCES

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5 REVIEW OF CHAPTER 6.0 — VOLCANIC INTRUSION MODEL

This chapter reports the approaches used in the modelling of basaltic volcanism consequences and their predicted results. A simple dike-emplacement model based on Lister and Kerr (1991) was used to generate a stochastically-derived set of dike lengths and widths (Sections 6.2 to 6.3). A literature survey of the volcanism occurrence probability has been conducted (Section 6.5). The analysis of the volcanic disruption scenario was performed to estimate the amount of each radionuclide that reached the accessible environment (ground surface) as a function of the volcanic event (Section 6.6). In this preliminary study, it was concluded that given the model assumptions, the amount of material moved to the ground surface by a volcanic intrusion can exceed limits established by the EPA.

5.1 GENERAL COMMENTS

The PNL report provides a brief summary of probability and consequence issues associated with potential volcanism in the Yucca Mountain region and outlines preliminary attempts to develop a PA model for igneous intrusion. The report provides insight into volcanism issues on several levels. Although, as stated in the report, results of the PA model are preliminary and model development is in its earliest stages, many aspects of the report, such as the assumptions used in the dike emplacement model of Lister and Kerr (1991), and uncertainty in the degree of entrainment of shallow crustal xenoliths, are well presented.

The model uses equations developed by Lister and Kerr (1991) to derive likely dike geometries. The model depends on parameters including mass flow, density contrast, magma viscosity, source depth, and elastic properties of the surrounding country rock. These parameters are used in a Monte Carlo simulation to produce a range of dike geometries, and together with an entrainment factor, the likely release of radionuclides is calculated. Dike geometries calculated in this manner agree well with observed dike geometries. The report indicates that it is possible that EPA standards may be exceeded due to volcanic intrusion and entrainment of material. However, the parameters used in the volcanism consequence modeling for the volcanic scenario remain to justified. Conservatism in choosing the modeling parameters should be adopted by DOE in modeling volcanism consequences.

5.2 MAJOR PROBLEMS

The PNL PA model for volcanism is in its preliminary stages of development and does not attempt to account for many of the direct and indirect effects of volcanism on repository performance. These direct and indirect effects include dispersal of radionuclides in an erupting ash column, multiple dike intrusion, geochemical and thermal loading that result from magmatic intrusion and degassing, and disruption of the groundwater table. The PNL PA model, in this preliminary form, does not address important aspects of volcanism that will need to be incorporated to make it more realistic.

5.3 AREAS OF POSSIBLE CONTROVERSY

The report clearly states that eruption energetics are not considered as part of the disruption model. However, use of the terms strombolian, Hawaiian, and hydromagmatic to describe Yucca Mountain region volcanism inherently limits the range of eruptive energetics considered in these models. This persists throughout the report and is not supported by current information on small volume basaltic systems in the southwestern United States and elsewhere. Certainly numerous authors have referred to ultra-strombolian, sub-plinian, and ultra-vulcanian eruptions to describe the ashfall sheets and observed eruptions of many cinder cones (e.g., Amos et al., 1983; Connor et al., 1993; Walker, 1991, 1993; Rowland et al., 1991). There may be few cinder cones where eruptions are limited to comparatively low-energy, ballistic events. Eruption energetics may have substantial impact on estimates of entrainment and dispersion (Valentine et al, 1992). Possibly, future PNL PA models will incorporate this range of energetics.

The discussion of dike dimensions is comprehensive. However, single dike intrusions associated with a period of magmatism are not necessarily more common than multiple dike intrusions. For example, at least three dikes feed the Fortification Hills basalt. These dikes outcrop over a wide area (on the order of 30 m in width) and each dike is about 1 m wide[1]. Multiple dike intrusions are common and should be incorporated into models at an early stage. Furthermore, dikes are poorly modeled as planar sheets at the repository scale. Dikes intruding a variety of lithologies are normally sinuous at depths of 300 m or less, provided they are not controlled by pre-existing structures, such as joints. For example, some dikes in the San Francisco volcanic field undulate at wavelengths of about 10 m and amplitudes of 1 to 3 m (Connor and Hill, 1993). Bifurcation of dikes is also common at depths of less than 500 m. This is important because it will increase the region likely affected by dike emplacement within the repository block.

In further model development, it is recommended that dike intersection within the repository region should be modeled accurately. The spatial distribution of waste container inventory will critically determine the release amount of wastes. It appears that the amount of release modeled did not depend on the location of volcanic extrusions. Thus the model is too crude in this respect. The probability of a volcanic extrusion event should be much less than volcanic intrusion events. The study did not address the difference in the probability between extrusive and intrusive volcanic events.

The analysis of Lister and Kerr (1991) represents an important attempt to model dike emplacement. The equations may not apply to the repository setting directly, however, because of free surface effects. Specifically, close to any free surface, such as the surface of the earth or the repository itself, confining pressure and the differences between principle stresses become low. The result is that dike geometry can change rapidly in response to subtle changes in the stress field and elastic properties of the surrounding rock. At shallow depths, bifurcation and sinuosity become more important than can be explained through simple application of the Lister and Kerr (1991) model. It is recommended that future PNL IPA models attempt to incorporate the change in confining pressure on dike behavior.

The entrainment factor is quite important and, as the report indicates, difficult to constrain (Crowe et al., 1983). One aspect of the problem that should be kept in mind is that the early, explosive phases of eruption during new cinder cone formation are likely to be rich in crustal xenoliths compared

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1 (J. Mills, personal communication, 1993)

to later stages, and these early stages of eruption are not usually preserved in accessible deposits. It is reasonable to assume that the early, possibly gas-driven, hydrofracturing episodes will carry the most xenoliths to the surface and that subsequent stagnation along the margin of the conduit and crystallization will retard crustal xenolith entrainment. Foshag and Gonzalez (1956) provide descriptions of the early phases of the Parícutin eruption that are well-worth reviewing. At Crater 160 in the San Francisco volcanic field, crustal xenoliths are very common throughout the stratigraphic section compared to most cones and are extremely abundant in the surge facies, high in the stratigraphic section. Conservative models will need to take high entrainment factors into account (Shanster, 1983; McBirney et al., 1987). Furthermore, the choice of source depth at a fixed depth of 10 km cannot be justified (Section 6.3.4). The range of source depth is 10-35 km in accordance with the reference. The Monte Carlo simulation should allow a range of source depths to be chosen randomly. The approach is questionable because of fixing the source depth at the upper end of the range.

The report states that a geophysical basis is required before spatial variance in cinder cone distribution can be used in a predictive way. This statement is true to a limited extent. More needs to be learned about the nature of dike emplacement as it relates to differential stress and pre-existing structures before models (Ho et al., 1991; Sheridan, 1992) can be thoroughly assessed. The degree to which structures influence magma ascent, the longevity of cinder cone alignments, and related questions are poorly constrained at present. Presumably this is what is meant in the report by spatial bias. Ultimately, the relationship between the cinder cones and mapped faults will serve as the basis for probability models, rather than purely theoretical investigations into dike emplacement. Purely deterministic models likely will not be adequate. It is worth keeping track of and possibly developing more sophisticated probability models that can incorporate the geophysical detail alluded to in the report. This will likely be an important step in TSPA model development. The study arbitrarily set the probability of a dike intersecting a waste drift to be 0.25. The model should simulate the dike location and determine whether a dike intersects a waste drift. Furthermore, the model should determine the number of waste containers damaged from the area of intersection. Margulies et al. (1992) have developed such a model and obtained meaningful results.

The report suggests that the occurrence of polycyclic volcanism somehow reduces the likelihood of volcanism elsewhere in the field. This suggestion is unsupported. There are numerous examples of widely dispersed volcanic centers within the same field forming within 10,000 yr or less of each other. One example would be the Xitle-Jorrullo-Parícutin cinder cones. Furthermore, based on the geochronological data currently available, it is not certain that Sleeping Butte Cones and Lathrop Wells were not active contemporaneously (Vaniman and Crowe, 1981; Crowe et al., 1986). Evidence of polycyclic volcanism does not markedly change the probability of repository disruption.

5.4 AREAS TO BE EXAMINED IN GREATER DETAIL

In summary, the PNL PA report on volcanism provides a new and innovative approach to modeling sub-volcanic intrusions. The assumptions used in the report are preliminary and need to be supported by additional field data and theoretical model development. Several areas need to be investigated in greater detail, including:

- The Lister and Kerr (1991) model needs to be assessed for its applicability in near-surface situations, such as near the repository block.

- The entrainment factor needs to be better constrained using field evidence. If this factor cannot be constrained, it will be necessary to provide conservative estimates of entrainment, based on observations at historically formed cinder cones. Distributions for other input parameters also need additional justification. For example, why is a log-normal distribution for viscosity justified? Also, further research is required into the range of discharge and its effect on eruption dynamics and entrainment.

- It is important to incorporate a wide range of eruption scenarios, including more explosive eruptions than strombolian, in order to more closely conform to the types of eruptions that have been observed at historically active cones.

- The importance of dike interaction with faults and similar structures, the relationship between finite strain accumulation and magmatism, and the role of polycyclic volcanism should be introduced into probability calculations in order to lend a geologic basis to these models.

- Equation 6.3a has a typographical error. The last term should be:

$$-m\frac{\partial}{\partial x}\left[w^{3}\frac{\partial^{2}}{\partial x^{2}}H(w)\right]$$

- Equation 6.5a disagrees with Eq. (36) of Lister and Kerr (1991).

- The statement that "Expressions for continuity and global conservation of fluid are substituted into (6.1b) with some manipulation to give Eq. 6.3a..." is wrong. The statement probably means to substitute (6.2) into expressions for continuity.

- In Section 6.3.3 — Elastic Factor and Density Contrast, $\Delta\rho$ is not the density gradient. The value used by Lister and Kerr (1991) is not 300 g/cm^3; it is 300 kg/m^3. The units of density for high-silica tuff and basaltic magma should be in kg/m^3.

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6 REVIEW OF CHAPTER 7.0 — TECTONIC DISRUPTION MODEL

This chapter reports on the effects of earthquakes and accompanying fault displacement in changing the depth of the water table. Other earthquake effects are deemed improbable. This chapter is stated to be preliminary.

6.1 GENERAL COMMENTS

As stated in DOE's report, this analysis is preliminary. It is not adequately comprehensive in terms of the scenarios conducted or proposed. The report states that effort concerning tectonism models was not extensive because of budget constraints and a late start.

6.2 MAJOR PROBLEMS

Three scenarios were considered but only one was addressed. An assumption of failure of the waste canister in 300 yr was stated to eliminate the need for concern with faulting through the canister. Known high permeability of the repository rocks was stated to obviate concern for faulting through the repository block causing an increase in permeability. Only a scenario involving water table rise from vibratory ground motion was considered. The recent consideration by the DOE of more sturdy canister designs suggests that there are concerns with releases that are not moderated by engineered barriers for periods of time longer than 300 yr, if for no other reason than to abate uncertainties to an unspecified degree. Eventually, other scenarios will have to be considered including one in which the groundwater barrier restraining the high water table to the north of the site is breached by earthquake fault movement. This scenario was pointed out as critical by the National Research Council (1988).

6.3 AREAS OF POSSIBLE CONTROVERSY

Does Eq. (7.1) from McGuire et al. (1990) predict the 1983 Borah Peak, Idaho, water table elevation changes? If it does, it should be included as a verifying observation. If this comparison was not made, it should be a part of the DOE analysis to add credibility. Over 60 ft of elevation change was observed in 1983, but the rocks were limestones not tuff. The latter have more distributed porosity and are not as strong. It could be concluded that tuff would compress more than limestone causing a higher relative rise in the water table for a similar causative seismic event.

Equation (7.3) from Doser and Smith (1989) may be contested because it uses an average stress drop when basin and range earthquakes appear to have stress drops about one third of the average. This formula, however, should be conservative. There are two mechanisms postulated to operate in raising water level from an earthquake. One is transient compression of pore spaces by wave propagation (probably the longer wavelengths from large earthquakes which involve a greater volume of earth). The other is longer lasting or locally a permanent reduction of porosity caused by compression of soil or rock material at the earthquake source consequent to fault slip. Only the latter is addressed. The former mechanism may or may not be as important. A larger volume of rock is involved, but permanent changes in porosity are not thought to occur. The latter is a local effect around which the water table will equilibrate after a time span of approximately 2 months. The limited number of scenarios outlined in Section 6.2 may be controversial.

6.4 AREAS TO BE EXAMINED IN GREATER DETAIL

The probabilities of earthquake occurrence cannot be tied only to historic seismicity or preliminary rates of fault movement based on very limited observations of one fault's offset. The observation that seismic activity appears to have skipped from one range front fault system to another in the basin and range tectonic province, on about a 1200 yr interval, has been published by several authors (Ryall and VanWormer, 1980; Wallace, 1985). Unless tectonic models are developed and verified which preclude this from happening at Yucca Mountain, magnitudes in the low to mid 7s are likely to be required on the Solitario Canyon fault adjacent to Yucca Mountain despite low estimates of average annual slip. Therefore, alternative tectonic models should be included in future analyses in addition to those variations discussed above. Analyses will be acceptable if DOE can demonstrate that they are conservative.

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7 REVIEW OF CHAPTER 8.0 — HUMAN-INTRUSION MODEL

This chapter of the TSPA assumes that future drilling at the site can be described using a homogeneous Poisson distribution. To incorporate this model into TSPA calculations requires knowledge of future drilling rates, borehole diameter, waste package design and configuration, and inventory at the time of intercept. If the maximum effect of human intrusion on repository performance is to be assessed, it must be demonstrated that conservative values have been assumed for these parameters.

7.1 GENERAL COMMENTS

The inadvertent human intrusion scenarios proposed in the TSPA are reasonable and include the possibilities most likely to affect repository performance adversely. As with most models designed to evaluate the effects of inadvertent human intrusion scenarios, however, the TSPA relies on an assumption of drilling activity occurring as a homogeneous Poisson process (page 8.1). The reality of drilling (either exploratory or production) for economic resources is that it tends to be conducted in a nonhomogeneous fashion. In particular, drilling tends to focus on targets delineated by surface indications of the commodity for which the search is being made.

In practice, incorporation of this type of geological and geophysical information in PA is uncertain, and systematic procedures have not generally been developed. The approach taken here is commonly used, however, and in the absence of more specific data such as indications of mineralization or favorable surface features at Yucca Mountain that might be perceived as potential targets and control drillhole location, this approach is reasonable.

7.2 MAJOR PROBLEMS

In the scenario involving bringing the contents of a waste canister to the surface, the TSPA uses a randomly selected uniform multiplier between 0 and 1 to represent cases where the entire contents are not mobilized. While this is reasonable, why was a similar multiplier not invoked for scenarios involving injection of the contents into an aquifer? Presumably, this scenario assumes complete mobilization of the waste, but the reasoning for the different approach is not given.

Radionuclide sorption was allowed to vary, but presumably this was based on an empirical K_d approach. This controls the volume of potentially contaminated rock around a waste canister at a given time. The standard arguments against the use of a K_d approach to transport modeling (e.g., the potential for changes of several orders of magnitude in K_d due to the effects of mineral/water/gas chemistry) apply.

7.3 AREAS OF POSSIBLE CONTROVERSY

The main difficulty is that the sensitivity analysis is not designed to consider either different drilling rates or different drilling and repository configurations (page 8.1). The drilling rate is the most important parameter in the analysis, and it is held constant at 3 boreholes/km^2/10^4 yr. This leads to an expected 17 holes penetrating the repository during the 10,000 yr regulatory history; the number which is used in all scenarios. The drilling configurations and the number and size of waste canisters contribute to the likelihood of a drilling hit on the canister or a volume of contaminated rock. The number of canisters assumed in the analysis is not specified.

The drilling rate is based on guidance given for drilling in nonsedimentary rock in 40 CFR Part 191, Appendix B, and is an arbitrary rate derived by taking one tenth of the drilling rate estimated for sedimentary rock. The drilling rate for sedimentary rock, in turn, is based on petroleum drilling histories in eastern New Mexico. The applicability of this rate to Yucca Mountain is not addressed in any detail in the TSPA. Apostolakis et al. (1991) report a range in possible drilling rates from 1 to 376,712 boreholes/km^2/10^4 yr. The values are based on different studies of oil and gas drilling, with the majority ranging from 1 to 47 boreholes/km^2/10^4 yr. Estimates based on water well drilling cluster near the upper end of the total range (45 to 8438 boreholes/km^2/10^4 yr). The sole example of drilling rates associated with mining activity is the upper value of 376,712 boreholes/km^2/10^4 yr; Apostolakis et al. (1991) indicate that this rate, which is based on assumed preproduction drilling on 400 ft centers, is probably excessive. Although it is true that Yucca Mountain is an unlikely spot for drilling a water well and there are no indications of mineral potential, it seems that different drilling rates should be evaluated.

7.4 AREAS TO BE EXAMINED IN GREATER DETAIL

Although the upper end of the range in drilling rates given in Apostolakis et al. (1991) is excessive, most of the rates fall in the range of 1 to 50 boreholes/km^2/10^4 yr, up to an order of magnitude greater than that used in the study. A sensitivity analysis using different drilling rates, similar to that performed in the SNL TSPA (Barnard et al., 1992), would help to evaluate the different human intrusion scenarios. This is especially important where the ratio with the EPA limit is on the order of 10^{-1} as shown in Figure 8.2 of the TSPA. A 10 to 20 times increase in drilling rates might lead to an exceedance of EPA limits. This is especially critical if all radionuclides and decay products are considered, as mentioned on page 8.3. It is worth noting that the summary does not suggest evaluating different drilling rates in recommendations for future work.

Configurations are related to the horizontal area (profile) of the waste canister and will affect the probability of a hit on a canister during the different inadvertent human intrusion scenarios. Canister inventory will affect the radionuclides available for mobilization at a given time. Possible parameters to vary include: waste canister diameter, radionuclide inventory, canister orientation, and the number of canisters.

If the new EPA standard includes dosimetry requirements, the TSPA will need to account for the radiologic impact on drilling crews, etc., in the analysis. Future work should be directed to determine appropriate conservative values for parameters related to human intrusion.

7.5 REFERENCES

Apostolakis, G., R. Bras, L. Price, J. Valdes, K. Wahi, and E. Webb. 1991. *Techniques for Determining Probabilities of Events and Processes Affecting the Performance of Geologic Repositories*. NUREG/CR-3964, Vol. 2. Washington, DC: Nuclear Regulatory Commission.

Barnard, R.W., M.L. Wilson, H.A. Dockery, J.H. Gauthier, P.G. Kaplan, R.R. Eaton, F.W. Bingham, and T.H. Robey. 1992. *Yucca Mountain Site Characterization Project. TSPA 1991: An Initial Total-System Performance Assessment for Yucca Mountain*. SAND91-2795. Albuquerque, NM: Sandia National Laboratories.

8 REVIEW OF CHAPTER 9.0 — CUMULATIVE RELEASES FOR THE TOTAL SYSTEM

This chapter reports simulations of the total system performance conducted for various combination of scenarios. The simulation scenarios included various infiltration rates and had a given probability of human intrusion and volcanism. Release results from each scenario were computed using conditional CCDF, which show the repository total system performance.

8.1 GENERAL COMMENTS

The total system performance for volcanism is crudely modeled. It did not distinguish intrusive or extrusive events. The probability of volcanism activity is a critical parameter in evaluating the cumulative releases for the total system. The scientific basis and judgements used by the investigators to choose the probability of volcanism occurrence are unclear. It appears that the total system performance modeling has significantly underestimated the probability of magmatic activity. Because the numerical modeling of groundwater transport has not been conducted successfully for an infiltration rate higher than 0.01 mm/yr, the results from the combined scenario modeling of CCDF are not yet reliable. The analysis of the cumulative releases for the total system with conservative parameters and a complete inclusion of various scenarios would make the results more useful.

8.2 MAJOR PROBLEMS

In Section 9.1, the probability of volcanism occurring was set arbitrarily to 1.0×10^{-4}. The report claimed that the value is "in the range of values suggested in the literature (see Section 6.5)." However, Section 6.5 did not mention the range of values for the probability of volcanism occurring. The scientific basis and judgements used by the investigators to choose the probability of volcanism occurrence are unclear.

The probability of volcanism activity is a critical parameter in evaluating the cumulative releases for the total system. The probability of a volcanic event should be determined as the probability of magma activity per unit area multiplied by the area of the accessible environment. According to the most recent study by Crowe et al. (1992), the probability of a magmatic event for the repository area in the next 10,000 yr is 3.5×10^{-2} for an area of 1670 km^2. Because the area of the accessible environment is not specified in the report, an area of 100 km^2 should be assumed and the probability of a magmatic event of 2.1×10^{-3} should be estimated. This estimate of the probability of volcanism occurrence is 20 times larger than the value used in the scenario modeling.

8.3 AREAS OF POSSIBLE CONTROVERSY

In Section 9.2, the report stated that five basic scenario simulations have been attempted at infiltrations of 0.0, 0.01, 0.05, 0.1, and 0.5 mm/yr. However, in Section 8.3, it is stated that the modeling of CCDF for releases was limited to a groundwater infiltration rate of less than 0.05 mm/yr. Section 4.2 admitted that only the 0.01 mm/yr condition was successfully simulated, and the 0.05 and 0.1 mm/yr conditions were only approximated (page 4.11). This raises a question about the reliability of the results shown in Figure 9.2 when the results are combined from five base scenarios at infiltrations of 0.0, 0.01, 0.05, 0.1, and 0.5 mm/yr.

8.4 AREAS TO BE EXAMINED IN GREATER DETAIL

The area of the accessible environment needs to be determined and specified in the report for review purposes. The probability of each base scenario was chosen to be 0.2 for each release scenario. It should be investigated whether the probability of a base scenario should vary with the infiltration rate. The cumulative releases will vary drastically according to subsurface magma activity (dike formation), cinder cone extrusion events or eruption (Margulies et al., 1992). The probability of volcanism also varies with magma activity. The scenario modeling cumulative releases should take into account various magma activities.

In Section 9.2, the scenarios modeled consider the occurrence of volcanism after human intrusion. The scenario of volcanism activity before human intrusion cannot be excluded and should be modeled.

8.5 REFERENCES

Crowe, B.M., R. Picard, G. Valentine, and F.V. Perry. 1992. Recurrence models of volcanic events: Applications to volcanic risk assessment. *Proceedings of the Third International Conference on High-Level Radioactive Waste Management, Las Vegas, NV, April 12-16, 1992*. American Nuclear Society 2: 2344-2355.

Margulies, T., L. Lancaster, N. Eisenberg, and L. Abramson. 1992. Probabilistic analysis of magma scenarios for assessing geologic waste repository performance. *Proceedings from American Society of Mechanical Engineers, Winter Annual Meeting, November 8-13, 1992*. Anaheim, CA: American Society of Mechanical Engineers.

9 REVIEW OF CHAPTER 10.0 — DOSE MODELING DESCRIPTION AND RESULTS

This chapter reports descriptions of methodology used to derive dose estimates for all PNL release scenarios with the exception of basaltic eruptions.

9.1 GENERAL COMMENTS

The health physics terminology in the report is used in an unrigorous and imprecise manner. For example, the term "whole body dose" is used repeatedly in Chapter 10; this should be "total effective dose equivalent" as presented in 10 CFR 834, "Radiation Protection of the Public and the Environment" and DOE/EH-0256T, "DOE Radiological Control Manual."

In Section 10.2, there are several errors in Table 10.1, page 10.3. "Muscle" should be replaced with "Breast," "Blood Marrow" should be replaced with "Red Bone Morrow," Lung and Lymph" should be replaced with "Lung," "Testes and Ovaries" should be replaced by "Gonads," and "Remainder" should be added with a weighing factor of 0.30. This is the terminology used in ICRP-26 (ICRP, 1977, page 21, paragraph 105), and the GENII manual (Napier et al., 1988).

In Section 10.4, plots of dose versus time should be created. The tables in Section 10.4 convey little information. CCDF of probability versus dose should also be created. The use of five significant figures to report doses in Table 10.8 (page 10.7) is not justifiable. A maximum of two significant figures should be reported.

The Summary, page iii, paragraph 4, states that the "results presented here are not sufficiently advanced to be usable in formulating prudent judgments about site suitability or the expected risk of the potential repository." However, in paragraph 5, it states that "the dose results presented here indicate that the potential repository may be able to meet both individual and population dose criteria." Based on the statement in paragraph 4, the statement in paragraph 5 does not appear to be justifiable.

In Section 2.4.1, page 2.5, the burnup, decay, enrichment, and type of fuel are not stated.

9.2 MAJOR PROBLEMS

Radiation protection standards for members of the public are typically in the form of a dose per a single year of external exposure and a single year of intake. The dose associated with a single year of intake is typically a 50-yr committed effective dose equivalent. The authors of this report state that "the doses reported here are lifetime effective dose equivalents" (page 10.2). This implies that they are calculating doses based on 70 years of external exposure and 70 years of intake. This is incorrect. The 25 mrem/yr and 4 mrem/yr limits are based on annual intakes, not 70 year intakes; therefore, the dose calculations designed to show compliance with these limits should also be based on annual intakes. This also implies that the doses calculated in the report are approximately 70 times too high.

9.3 AREAS OF POSSIBLE CONTROVERSY

Several scenarios are presented in Section 10.3, but none are justified, except by reference to the Hanford Defense Waste Environmental Impact Statement. The authors should take a more structured approach to the definition of scenarios because Hanford scenarios may not be appropriate at Yucca Mountain. The selection of scenarios for analysis should be justified in light of site-specific conditions and data. The steps taken to develop the scenarios should include: (i) identifying a complete spectrum of scenarios that are representative of Yucca Mountain; (ii) eliminating scenarios that are bounded by others; (iii) arriving at a representative and bounding list of scenarios; (iv) justifying the list of scenarios; and (v) analyzing the scenarios.

There are extensive environmental data in GENII and SUMO that the authors have adopted without critical review as to the applicability of the data at Yucca Mountain. Perhaps an appendix with a complete tabulation of the data, along with primary reference citations, should be provided.

According to Table 10.3, page 10.5, the ^{234}U progeny (^{230}Th, ^{226}Ra, and ^{210}Pb) were ignored in the dose calculations. These radionuclides (especially ^{226}Ra and ^{210}Pb) can be important dose contributors in a groundwater assessment where there is time to ingrow the progeny and to omit them without discussion is not justifiable.

It would appear that there is also the potential to release tritium as a gas from the repository. If the gaseous release of tritium were removed from further consideration through some screening calculation, then the screening calculation should be discussed and documented.

In Section 2.3, pages 2.3 and 2.4, the radionuclides chosen for analysis appear to be based primarily on the groundwater transport pathway. However, these same radionuclides were chosen for the intruder analyses. As such, these radionuclides may not provide a reasonable choice of radionuclides based on an intruder scenario. If radionuclides are to be screened, the authors should conduct a formalized screening process that takes into account the following:

- Radionuclide inventory
- Radiotoxicology (both internal and external)
- Environmental mobility
- Radionuclide half-life and travel time to the potential receptor
- Ingrowth of radionuclide progeny
- The specific scenario for which the radionuclides are being screened

9.4 AREAS TO BE EXAMINED IN GREATER DETAIL

The long-lived radionuclide ^{93}Zr was not chosen for analysis. For spent pressurized-water reactor (PWR) fuel (30,000 MWd burnup and 3.11 percent enrichment) at 10,000 yr, ^{93}Zr represents 0.44 percent of total activity (1.912 Ci), while ^{126}Sn represents 0.15 percent (0.6389 Ci). ^{93}Zr also has a half-life of 1,530,000 yr, while ^{126}Sn has a half-life of 100,000 yr.

Based on the objectives contained in Section 1.3, page 1.2, and the use of Hanford data in the dose modeling, the author's conclusions that the cited objectives have been met should be re-examined.

The conceptual model at Hanford, a highly agricultural area with a major source of surface water available, is far removed from the conceptual mode at Yucca Mountain, an area with minimal agriculture and no surface water. Therefore, the use of the Hanford conceptual model in the dose modeling does not appear to provide information that would fulfill the first objective, "identify further conceptual model...needs," relative to dose modeling.

The use of Hanford dose modeling data, as opposed to Yucca Mountain data, does not provide useful information that would enable Yucca Mountain dose modeling data to be refined and prioritized, nor does it help to guide site characterization. Therefore, the second objective, "refine and more fully prioritize identified data needs and help guide site characterization," has not been met relative to dose modeling.

The use of Hanford dose modeling data may provide estimates of sensitivities of Hanford data relative to total system performance, but it does not provide estimates of sensitivities of Yucca Mountain data relative to total system performance. Therefore, the third objective, "define relative sensitivities of total system performance to the conceptual model, data assumptions, and computational methods" has not been met relative to dose modeling.

Given that Hanford dose modeling data were used, any preliminary evaluation of performance should be regarded as tentative. The authors have not made an evaluation of Yucca Mountain repository performance. They have made an evaluation of a hypothetical entity based on a mixture of Yucca Mountain and Hanford assumptions.

9.5 REFERENCES

10 CFR Part 834 (Code of Federal Regulations). 1993. 10, Energy, Part 834, Proposed Rule. *Radiation Protection of the Public and the Environment*. Washington, DC: Office of Federal Register.

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International Commission on Radiological Protection (ICRP). 1977. *Recommendations of the International Commission on Radiological Protection*. ICRP Publication 26. New York: Pergamon Press.

Napier, B.A., R.A. Peloquin, D.L. Strenge, and J.V. Ramsdell. 1988. *Hanford Environmental Dosimetry Upgrade Project, GENII — The Hanford Environmental Radiation Dosimetry Software System, Volume 1; Conceptual Representation, Volume 2; User's Manual; Volume 3; Code Maintenance Manual*. PNL-6584, Vols. 1, 2, and 3. Richland, WA: Pacific Northwest Laboratory.

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