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Dr. Atef Elzeftawy
Repository Projects Branch
Division of Waste Management
US Nuclear Regulatory Commission
7915 Eastern Avenue
Silver Spring, MD 20910

Dear Dr. Elzeftawy:

Enclosed you will find our review comments on two BWIP documents, "Proposed methodology for composition of scenario analysis for the Basalt Waste Isolation Project," Rockwell Hanford Operations, Rept. RHO-BW-CR-147 P, and "Probability encoding of hydrologic parameters for basalt," Rockwell Hanford Operations, Rept. RHO-BW-CR-146 P.

The cost of these reviews was less than $4000. Please feel free to call me if you have any questions.

Sincerely,

Regina

Regina L. Hunter
Waste Management Systems
Division 6431

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Review Comments

by R. V. Guzowski and R. L. Hunter

Roberds, W. J., Plum, R. J., and Visca, P. J., 1984. "Proposed methodology for composition of scenario analysis for the Basalt Waste Isolation Project," Rockwell Hanford Operations, Rept. RHO-BW-CR-147 P, Variously paginated.

General Comments

1. The report is well organized and thorough.

2. Inclusion of a process tree that starts with an event affecting performance assessment and derives the causative events introduces an unnecessary complexity to the method. Comprehensive lists of causative events, features, and processes already exist. In addition, the technique proposed to prune the tree is not practical for the early stage of the methodology.

3. The use of fault trees in the methodology has several drawbacks:

   a. many geologic events and processes occur gradually over a long time period, so a "yes" or "no" statement is not appropriate;

   b. interactions between factors that influence radionuclide movement are not adequately incorporated;

   c. for a particular set of conditions, many processes are deterministic; and

   d. the representations of processes are forced into artificial divisions.

4. The authors need to define what they mean by a panel of experts. Does the panel consist of experts in the same field, as were the BWIP hydrologists in Runchal and others, 1984 (report RHO-BW-CR-146 P), or panelists who are experts from a variety of specialties, as in Davis and others, 1983, (Rept. RHO-BW-ST-42 P)? The use of either or both the Delphi techniques and probability encoding is defendable only if the panel consists of experts in the same speciality and only items in their speciality are considered.

5. The chapters on available methodologies (Ch. 2), guiding additional work (Ch. 4), and management and organization (Ch. 5) are for the most part good. Chapters 4 and 5 cover

topics generally not included in methodologies for scenario analysis and are notable both for their presence and content.

6. Although not supportive of all aspects of the proposed method, Appendix B (concepts to support recommended method) is excellent and should be required reading for anyone dealing with scenario analysis or site evaluation.

Specific Comments

p. 7 and 8, Section 2.1. This section is confusing. It is stated on p. 8 that the first list of techniques "are generally well understood." We doubt this. For example, what are the "classical statistics and probabilistic techniques" they refer to on p. 7? If such a set of techniques were well known, Sandia would not be funded to do the Probability Project under A1165, Task 2. In contrast, they do not consider the list on p. 8 to be widely known, even though the Principle of Uniformitarianism is taught in all introductory geology courses. (Admittedly, the Principle is misrepresented in this report. See our next comment.)

p. 8, para. 1 of 2.1.1. The statement of the principle of uniformitarianism is not accurate. As presently applied to geology, the principle states that the physical and chemical laws at work today also were in effect in the past. By implication, these laws will apply in the future. The projection of past events into the future is not a use of uniformitarianism.

p. 9, para. 1, bullet 2. Overemphasis of major events and processes is a problem of the person doing the overemphasizing and is not inherent in predicting future events and processes.

p. 9, para. 1, bullet 3. How can a future event or process be predicted if the event or process has not occured before in the geologic record? Do the authors mean "in the geologic record of a given site?"

p. 10, para. 1, sent. 3. Formal opinion techniques, such as Delphi, also can be easily challenged.

p. 10, para 2. Any chosen "expert" who would change his mind simply because some other "expert" has different opinions is not an expert.

p. 10, para. 3. The so-called advantage of anonymity may not always be an advantage. Some experts have more expertise than others, and "lesser" experts routinely adopt the principles and ideas developed by "greater" experts. Delphi interferes with this process.

p. 10, para. 4. All expert opinion is personal bias. The fewer the data, the more the personal bias.

p. 10, para. 5. See attached review comments of the article by Runchal and others, 1984. The authors present no evidence that Runchal and others' technique is widely considered to be the state of the art.

p. 11, para. 3. This technique does not address all personal biases, especially when few or no data exist upon which to base an opinion.

p. 11, para. 5. This technique may be useful in limiting uncertainty in the analysis of data, but Runchal and others (1984) used this technique on topics for which virtually no data existed, with questionable success.

p. 32, Sect. 3.3, para. 1. Based on the number of scenario lists, both generic and site specific, developed to date, comprehensive lists have been developed. The methodologies used in the cited references are no harder to track than this one.

p. 34, para. 2. The use of a process tree (event tree) in this way is a regressive step in methodology development. This tree quickly becomes unwieldy even with a screen-as-you-go approach and obviously does not work (see next comment)

p. 35. This example is a poor one to demonstrate the method. Note the first branch from the top--the only way for radionuclides released from the waste package to directly reach the accessible environment is by means of drilling that passes through or near the waste package. This mechanism is not even listed. By starting with a set of features, events, and processes (e.g. Cranwell and others, 1982), this mechanism is readily obvious.

p. 40, para. 2. How is the cutoff value of more or less than 1×10^{-2} toward complying with a pertinent performance criterion determined at the early stage of scenario development?

p. 40, para. 4. When pruning the tree during construction, is any consideration given to the possibility of cause and effect among components? A component that may have a probability lower than the specified cutoff may have a higher probability of occurance if another component occurs. Also, the combination of components with probabilities of 1×10^{-4} and 1 does not result in a joint probability "much less than 1×10^{-4}" (top p. 42).

The 1×10^{-4} conditional probability should be stated as for 10,000 yrs.

p. 44, last para., sent. 2. Expert opinion may not always involve the interpretation of data (e.g. Runchal and others, 1984).

p. 45, para. 4. Probability encoding does not necessarily make expert opinion more defensible than expert opinion without the encoding.

p. 46, para. 1, sent. 3. This sentence is generally true, but a single expert may have unique insights, whereas the group may express conventional wisdom. If concensus is the only criterion for correctness, no field of human endeavor, including waste management, will ever progress. See our previous comment on experts.

p. 46, para. 2, sent. 4. Is there any provision for factoring out extreme views? Having such a provision could result in removing the opinions of someone with profound insight, but not having such a provision could include values that unnecessarily broaden the uncertainty.

p. 46, para. 2, sent. 5. If the panel consists of experts (presumably all in the same specialty), then degree of expertise should be approximately the same. If not, the experts were poorly chosen. In any case, this sentence contradicts p. 10, para. 3, where the "advantage of anonymity" is discussed.

p. 46, para. 4, sent. 2. Even objective assessments can be controversial. For example, objective assessments show that coal power plants are more dangerous to the public than nuclear plants, but in general the public is more opposed to nuclear plants than to coal.

p. 46, para. 4, sent. 4. The willingness of an expert to change an opinion may well indicate that the data are so incomplete that no strong feelings are held or defensible.

p. 48, para. 2, bullet 3. How can the approximate degree of conservation be assessed? What constitutes reasonable values may not be known.

p. 48, para. 3. Why are the extreme values likely to change with increasing sample size? One or both extremes may have physical limits that restrict the possible spread.

p. 48, para. 4, sent. 1. The extremes also should stabilize, unless there are "outlyers." Generally, these outlyers don't affect the mean or median drastically.

p. 48, para. 4, sent. 2. The mean is not necessarily related to one-half of the experts' opinions.

p. 48, para. 4, sent. 4 and 6. An assumption that the experts' opinions are normally distributed about the mean will only be valid for random guesses about a physically limited possibility.

p. 48, para. 2, last sent. State the probability as 1×10^{-4} in 10,000 yrs.

p. 53, para. 2. Additional work may be needed to confirm the assumptions made in determining the probabilities of the scenario components. A shift from subjective values to objective values may be desirable and feasible.

p. 63, para. 5. The magnitude of the uncertainty may increase with more data as a result of poor testing techniques or an initial set of values that did not fully encompass the actual range.

p. 63 bottom - p. 64 top. Sophisticated models also require considerable simplification and approximation of natural systems and processes. These uncertainties will not be minor relative to other uncertainties.

Review Comments

by R. V. Guzowski and R. L. Hunter

Runchal, A. K., Merkhofer, M. W., Olmsted, E., and Davis, J. D., 1984, "Probability encoding of hydrologic parameters for basalt," Rockwell Hanford Operations, Rept. RHO-BW-CR-146 P, variously paginated.

General Comments

1. In instances where data for certain parameters are absent, rough estimates of values for parameters can be made by analogy to similar rock types in other locations for which data exist. Data from other basalt sites with which to supplement or replace data from the Hanford Site are inadequate or nonexistent. The values of parameters presented in this report are speculative or based on personal conceptual models. Speculation and personal conceptual models are a form of personal bias. One of the goals of the SRI encoding method is to eliminate personal bias. This goal cannot be accomplished in this application.

2. Expert opinion will play an important role in the risk assessment of potential repository sites. This role is especially applicable to the evaluation of events and processes for which data are not available or are sparse. Where data for a particular parameter do not exist but can be measured, expert opinion can provide temporary and hopefully reasonable values. The purpose of this report was to provide reasonable data until measurements of effective porosity and anisotropy are made. Although this was done, the wide range in the reported values indicates that the experts lack a concensus on the range and the magnitude of these values. By reporting the expert's estimates to two significant figures for values at 10 percent, median, and 90 percent, the authors seriously overstate the quality of the values.

Specific Comments

p. 2, para. 3. The statement is made that RHO currently needs defensible estimates of values of hydrologic parameters. How does SRI encoding make the values of data listed in this report more defensible than simply stating that no data exist and data based on expert opinion will be used until they do? (See General Comment 1).

p. 3, para. 2. The three RHO experts have extensive site-specific experience. As stated on page 2, para. 3, known representative estimates of anisotropy and effective porosity are not available. Neither the text nor Appendix B provide reasons why these individuals are any more qualified to make estimates of values of hydrologic parameters for basalt than any other hydrologist.

p. 5, para. 2. "...the analyst strives from the subject's responses to understand the modes of information processing used by the subject and to infer from this the biases that may exist."

What is the background of the analyst with respect to understanding the site's complexity, the features of the rocks, and hydrology that will enable the analyst to recognize prejudicial personal biases?

p. 7. See comment for p. 5, para. 2

p. 9, para. 1, bullet 2. What were the sources of the available data? Why did experts on the Hanford-Site hydrology need to be given this information? With each of the experts supplied the same information, what accounts for the very wide differences in their estimates, as presented later in this report? Did supplying this information make any difference in the estimates of the experts?

p. 10, last para. See comment for p. 5, para. 2

p. 11. Megascale refers to a block 100 to 1000 m on a side and entirely in the flow top or flow interior. To get flow tops in this size range would require incredible lumping of units in a model, thereby eliminating application of the definition to Cohassett basalt. This problem also applies to the flow interior in all but the lowest portion of the range.

p. 15, para. 1 and 2. Considering that the values are estimates and not fact, and that the estimates range over several orders of magnitude, reporting the median to two significant figures is frivolous.

p. 15, para. 2. It doesn't take much of an expert to conclude that effective porosity has a 100% probability of being at or below 1.

p. 15. The upper limit of effective porosity is controlled by a value beyond which the estimate is impossible. For the lower limit, the restriction does not apply. The convergence of values at the upper end of the range and the broad spread of values at the lower end should not be a surprise.

p. 16, Figure 1. Experts G and H are being evasive by assigning the upper limit of effective porosity to be 1.

p. 17, Table 3. See comment for p. 15, para. 1.

p. 18, Figure 2. See comments p. 15, para. 2, and p. 16, Fig. 1.

p. 19, para. 1. See comment for p. 15.

p. 19, para. 3. Whereas the end points were compared in previous plots, this paragraph shifts to a discussion of the reference point values with less than 90% cumulative probability. Consistent criteria should be used.

p. 19, para. 4. Experts F and G are in almost total agreement over the range. So are experts A and C (differing appreciably only below 10%).

p. 25, para. 1. A two-order-of-magnitude difference in anisotropy could mean a substantial difference in hydraulic conductivities.

p. 25, para. 3. What do the differences in range for porosity and anisotropy mean?

p. 26, Figure 6. Expert H has covered such a broad range as to be meaningless (more than eight orders of magnitude for the anisotropy!)

p. 27, para. 2, last sent. How can experts with no site-specific familiarity be less uncertain about the effective pososity of the flow interior than the RHO "experts"?