Comments on Revisions to NUREG-1757 vol. 2

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To Cynthia Barr, Office of Nuclear Material Safety and Safeguards, NRC

Regarding Docket NRC-2020-0192: Review of NUREG-1757 vol.2 rev.2

The United States Nuclear Regulatory Commission (NRC) has drafted changes to its document NUREG-1757 volume 2 revision 2, and requested in the Federal Register that comments be provided to the NRC. I write now to provide such comments.

Document reviewed: `NUREG 1757 Vol 2 Compare Rev 1 to Rev 2.pdf`

General Comments

This is a well-written document, generally. In fact, I have no general comments other than that.

Specific Comments

p. xlvi, definition of "*Distribution Coefficient or* K_d": The definition provided is specific to a "solid/water distribution coefficient or K_d." Note that there are other similar distribution coefficients that are used in similar analyses, such as an air/water distribution coefficient, K_H, or the octanol/water partition coefficient, K_{ow}. All are distribution coefficients. I would suggest that the proposed definition be either more broad to include other distribution coefficients, or be made more specific if referring only to K_d. Also consider adding a definition of the air/water distribution coefficient, K_H, also known as the Henry's Law coefficient.

p. xlvi, definition of *Exposure Pathway*. It is my understanding that the exposure pathway also includes activities and behaviors of the exposed individual(s), as well as uptake factors into tissues, dose conversion factors, and the like. In my risk analyses, I generally refer to environmental transport pathways as such, or as contaminant transport pathways, to get to an exposure medium, such as soil, water, or air to which a receptor may be exposed. Calculations from the exposure media to dose or risk (everything in the Exposure Scenario) are to me considered the exposure pathway.

p. xlvii, definition of *Floodplain*. Two very different examples are included in this definition. Is it both? The second sentence seems to imply that the definition is limited to what is generally called the "100-year floodplain." Is the definition of "floodplain" to be equated to the "100-year floodplain"?

p. xlvii, definition of *Groundwater*. It seems that NRC is changing its style for this term to be one word, rather than two (ground water). It retains the definition that groundwater includes both the unsaturated and saturated zones, however, which is consistent with previous NRC definitions. Note that most organizations outside NRC restrict its use to only the saturated zone. Is the change to "groundwater" intentional?

p. xlviii, definition of *In Situ Recovery (ISR)*. With all these steps included in the definition, it seems to be overly prescriptive. Is the composition of the lixiviant necessary to this definition? Is it really necessary that 55-gallon drums be used? What if the uranium is used for purposes other than nuclear power reactors? Suggest omitting steps (3) and (4), or better yet, avoiding the steps in the definition altogether.

p. liv, definition of *Transmissivity*. Replace the word "depth" with "thickness" (of the aquifer). Depth can sometimes be confused with the depth to the top of the aquifer. Thickness is unambiguous. Further hydrogeologists use the term "saturated thickness." You might add that transmissivity = hydraulic conductivity × saturated thickness.

Consider adding a definition of a *Receptor*.

p. 13, replacement figure following Table 1.3: It seems that step 9 is the victim of a copy/paste error, since its contents are identical to step 8. Should probably list step 9 in the original figure.

p. 1-15 ¶ 1.4.1: The statement is made that "The iterative approach ... of using existing information for generic screening and using site-specific information as appropriate ... provides assurance that obtaining additional site-specific information is worthwhile, because *it ensures that a more realistic dose assessment will generally result in an estimated dose no greater than that estimated using screening*." (Emphasis added.) To the contrary, there should be no expectation that making an assessment more site-specific will reduce doses. It will make it more accurate for a given site, however. Generic screenings do not always produce higher doses than site-specific analyses. This statement should be heavily modified or deleted.

p. 1-16 *et seq*. §1.4.2: The steps defined here and shown on the accompanying figure are a bit out of order, in comparison to those in the DQO approach (see § 3.2). Rather than starting with amassing available data and seeing what sort of model could be built from them (an outdated approach), the approach should be to start with the question to be answered, and then determining what data are needed to answer that question.

Step 2 dances around the concept of determining the Features, Events, Processes, and exposure Scenarios (FEPSs) that are relevant at the site to answering the question at hand. Why not invoke that well-known FEPSs analysis outline here?

A discussion of uncertainty is also missing from these steps. There inevitably comes a time when a decision-maker must assess the degree of confidence s/he has with a result. For example, does the model show that there is a 95% probability that performance objectives will be met, or only 50%? This matter when deciding how to proceed in either making a termination decision or iterating more on the modeling in order to reduce uncertainties. Uncertainties and sensitivity analysis are briefly addressed in the third paragraph on p. 2-2, however.

§§2.2-2.4 not reviewed.

p. 2-12 Table 2-2: The disadvantage of the DCGL approach as an ill-posed problem is only hinted at in mentioning the sum-of-fractions problem. It is worse than what is stated, and is a fundamental limitation of this approach. It works if there are only a very few contaminants that pose risks, but more than a few and the problem becomes intractable. This is addressed a bit more fully in §2.5.2, but is still a fundamental flaw in the method.

§§ 3 through 7 not reviewed.

Review of Appendices P and Q:

p. P-1 § P.1.1: The issue of uncertainty in engineered barrier performance increasing with time is very important. This identifies a problem with many modeling paradigms that do not vary input parameters with time. We really must devise methods for allowing uncertainties in general to increase with time without causing issues for the sensitivity analysis.

Appendix P does not address the role of covers in attenuation of biotic intrusion, or the role of biota in the degradation of cover materials and structure, except briefly on p. P-16. Biotically-induced transport is too often overlooked in PAs, and yet it has been demonstrated to be the most significant contaminant transport pathway at some sites. Much more attention needs to be paid to this critical transport pathway, from FEPSs to contaminant transport modeling.

In discussing the degradation of engineered covers (e.g. §P.1.4.1), consider using the term "naturalization", rather than "degradation". In some cases, natural processes acting on a cover may actually enhance cover performance, so it is not always "degraded" *per se*. The term "naturalization" of covers was introduced at the workshop mentioned on p. P-16, leading to NUREG/CP-0195.

p. P-19: Consider promoting the section entitled "Potential Levels of Functionality and Uncertainty" in the outline to level §P.1.4.X, rather than having it under "degradation".

p. P-36, lines 25-29: Indeed it is beneficial to monitor those parameters that are most significant in evaluating cover performance. This is one of the conclusions of NUREG/CR-6948 (especially vol.2), which should be cited here.

p. P-39-40, Table P.4: The same reference is listed twice, using slightly different names:

NAS, "Assessment of the Performance of Engineered Waste Containment Barriers," National Academy of Sciences, Washington, D.C., 2007

and

National Research Council, National Academy of Sciences, "Assessment of the Performance of Engineered Waste Containment Barriers," 2007

According to their web site (https://www.nap.edu/catalog/11930/assessment-of-the-performance-of-engineered-waste-containment-barriers), the proper reference should be:

National Research Council. 2007. *Assessment of the Performance of Engineered Waste Containment Barriers*. Washington, DC: The National Academies Press. https://doi.org/10.17226/11930.

p. Q-7, §Q.5.2.1. The example discussed here and shown in figure Q.3 contains an interesting issue that is not addressed in the text: The problem of truncation in stochastic data. Since saturation cannot be physically greater than 1, the plot is truncated (appropriately) at 1. But the distribution that is used to represent this area is not discussed. There is a statistical problem of how to handle data that are naturally truncated. Should the mean and median reflect the truncation, or should they move smoothly through? In other words, does the truncation itself move the mean and median of a distribution, or does it simply truncate the values? In procedural

terms, does the analyst sample from a non-truncated distribution and then set all values > 1 to 1 (which does not affect the mean and median values from the original distribution), or does one build a truncated distribution (which does move the mean and median)? This is a subtle but sometimes important distinction, and is encountered often in PA modeling.

p. Q-8, Figure Q4: The colors identified in the legend do not seem to make sense with the plot. For example, the lavender is tied to a saturation of 0-0.15, and adjacent to it is the salmon color, with 0.75 to 0.9, with saturations dropping to a rather dry "hole" in the middle. This does not make sense.

p. Q-9 *et seq*. §Q.5.2.2: Comments on the examples:

First example, regarding food consumption. This is a good example, and I have yet to see a PA that implements it properly. The idea is to set a value (or better, a distribution) for average daily caloric consumption, and then allocate that consumption among the various food sources, which are also stochastic in amount. In essence, this is an example of the problem of getting several stochastic inputs to sum to 1 (or a value that can be normalized to 1). This is also the issue for something like assigning area coverage by various plant types, so that the areas occupied by different plant types always add up to the overall plant coverage area. These are instances of a statistical problem that apparently is nontrivial to solve on the fly.

In the second example, the corrosion rate and the retardation factors in soil are both a function of pH and water content. They are both likely a function of more things as well, but for the purposes of the example we can assume that these are the most important controlling factors. The solution here is to construct the calculations of corrosion rates and retardation factors to be functions of pH and water content (which will also be uncertain and may be functions of other things, like the infiltration rate), in the model itself. Other modeling parameters may also be a function of pH (and often temperature), such as solubility and diffusion coefficients.

Here are more examples that further illustrate the point, as well as hint at the complexity of interactions in these models:

Consider the "fact triangle" of porosity, water content, and saturation, all of which have physical limits at 0 and 1. These three parameters are closely tied together, since $S = \theta_w \times n$. If each is specified as a modeling input independently, it is easy to generate nonreal material properties. They MUST be correlated functionally. This can be done, for example, by defining stochastic inputs for porosity and water content, and calculating saturation from them (conditional on physical limits), or alternatively, calculating water content from porosity and saturation.

Similarly, consider porosity, bulk density, and particle density. Again, if defined with independent and uncorrelated distributions, nonreal materials are the result. In all such cases, it is best to define those parameters that have actual data (e.g. porosity from a pycnometer and bulk density from mass and volume measurements) and let the third one be a function of those.

Note that both of the previous examples involve porosity. In effect, this ties together saturation, water content, bulk density, particle density, and porosity. Further, the retardation from the preceding example is a function of bulk density and water content, $R = (K_d \times \rho_b) / \theta_w$, so that gets tied in. If we consider that plant growth and chemical uptake are also dependent on pH and water content, and that water content is conversely related to plant growth, we see how this all becomes nearly unmanageable. Again, functional relationships are the best approach.

Ultimately, the web of physical and chemical properties becomes deeply intertwined, with complex correlations. (An influence diagram showing these interrelationships would be an excellent addition, here.) If these are not accounted for, all calculations that depend on these properties will not reflect reality. This is one of the serious limitations of using generic data, addressed in the subsequent section of the report.

p. Q-10, last bullet: It is not clear what constitutes a case where mean of the peaks is favored over peak of the means. When would this ever be the case?

p. Q-14, § Q.5.2.4: I would suggest a fourth method to evaluate submodel uncertainty, which has been successfully implemented. This was for the specific submodel of the calculation of air phase tortuosity in porous media. There are a handful of different models in the literature, most of which involve different ratios and power factors for porosity and air content. The implementation was to include all these submodels in the larger model, and choose between them using a stochastic switch. This switch could choose among the models in an unweighted or a weighted fashion (as suggested in the third method). In this particular implementation, it was discovered that the choice of air phase tortuosity model was quite significant in the calculation of radon flux at the ground surface. This result prompted a laboratory investigation of the effective diffusivity of radon through site-specific materials. Obviously, the confidence in the site-specific laboratory measurements was much greater than all the mathematical models found in the literature. This resulted in a lower overall uncertainty (and therefore greater confidence) in the radon flux model endpoint.

p. Q-14, equation in line 39: The concentration of the chloride ion should be expressed using standard chemistry notation as "$[Cl^-]$", not "(Cl^-)". Also, italicize a, b, and C, and present generic dimensions L/T for the corrosion rate, rather than specific units.

p. Q-15, line 6: The quantification of corrosion rates here should be "mils/yr" rather than simply "mils".

p. Q-16 §Q.5: A much more interesting and complex example of process model abstractions may be found in a recent model by Neptune and Company, Inc., where streamflow rates and infiltration rates were both modeled using the process models SWAT and HYDRUS, respectively. Briefly, each was run probabilistically, and then analyzed using sensitivity analyses to determine the most significant inputs for each model. Many of these are shared, such as precipitation. Lookup tables were constructed for both model outputs, providing streamflow rates and infiltration rates, respectively, for values of inputs (e.g. precipitation) selected from stochastic distributions. The input distributions and lookup tables were combined in the probabilistic site model. For various realizations in the site model, the inputs were sampled, and resulting streamflow and infiltration rates were interpolated from the lookup tables. This produced a functional correlation between precipitation (and other variables), streamflow, and infiltration rates, and the technique was a significant advance in model abstraction methodologies. Neptune could share more about this if there is interest in including it in this NUREG.

Missing: Measurements of ^{226}Ra concentrations in covers. Radium has the potential to move as diffusion in the water phase, even through a radon barrier. Since ^{226}Ra is a parent of ^{222}Rn, the decay product can be produced above the radon barrier.

Terminological errors:

p. P-26 ¶ 2: The word “erratics” was changed to “erratic”, but in fact “glacial erratics” is the proper term, unless referring to a single stone.

p. P-35, line 14: Replace “Sandia National Laboratory” with “Sandia National Laboratories”.

p. Q-14, line 41: Change “flux rate” to “flux”. A flux is already a rate. See Stauffer, P.H., *Flux Flummoxed: A Proposal for Consistent Usage*, Ground Water 44(2) pp. 125-128, 2006, DOI: 10.1111/j.1745-6584.2006.00197.x

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General Comment

See attached file.

Attachments

J Tauxe comments NUREG-1757 vol2