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24 July 2015

Annette Vietti-Cook
Secretary
U.S. Nuclear Regulatory Commission
Washington, D.C. 20555-0001
Rulemakings and Adjudications Staff

Subject: Comments on Guidance for Conducting Technical Analyses for 10 CFR Part 61 (NUREG-2175, Draft Report for Comment)

Reference: Docket ID NRC–2015–0003

Dear Ms. Vietti-Cook:

Neptune and Company, Inc. (Neptune) is submitting the attached comments in response to requests for comment on the proposed *Guidance for Conducting Technical Analyses for 10 CFR Part 61*, NUREG-2175 (Draft Report for Comment).

We believe that the revision to 10 CFR 61 is a worthwhile endeavor that will lead to radioactive waste disposal decisions that are more beneficial for and protective of current and future generations, and that the supporting guidance is important for achieving consistent and effective implementation of part 61.

Thank you again for this opportunity to comment. Questions regarding these comments may be directed to Dr. Paul Black at (720) 746-1803 ext 1001 (pblack@neptuneinc.org), or Dr. John Tauxe at (505) 662-0707 ext 15 (jtauxe@neptuneinc.org).

Sincerely,

John Tauxe, P.E., Ph.D. and Paul Black, Ph.D.
Neptune and Company, Inc.

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Comments on Guidance for Conducting Technical Analyses for 10 CFR Part 61 (NUREG 2175)

Neptune and Company, Inc. (Neptune) appreciates the opportunity to provide comments on the U.S. Nuclear Regulatory Commission (NRC) proposed *Guidance for Conducting Technical Analyses for 10 CFR Part 61* (NUREG-2175), provided as a draft report for comment.

The comments below are organized into a General Comments section, with application to the overall rule, and a Specific Comments section, with comments following the same order as they appear in the proposed revisions document.

General Comments

This guidance for technical analyses required by the proposed revisions to 10 CFR 61 is welcome. The bulk of the document is well written, and it is clear that the authors put a lot of thought and care into its development.

The draft NUREG-2175 suffers from occasional redundancy, and some disconnects in the flow of development of concepts, especially in the early sections, but these shortcomings could be addressed with some additional careful technical editing.

Specific Comments

§ 1.1.2 Safety Case

It remains unclear how the safety case is to be constructed. It seems that this is a collection of documents and analyses required to demonstrate adequate protection of public health, but the mechanics of how all these analyses are to be collected into a Safety Case is lacking. Admittedly, this is a new concept (for US regulators) and will require vetting and the development of examples. Is the Safety Case a document that serves as a wrapper for the performance assessment (PA), intruder assessment (IA), defense-in-depth analysis, etc.? Please provide clarity on what is expected in terms of a license applicant's submittal.

§ 1.1.4.1 and 3.0 Performance Assessment

Bullet (8): Uncertainty addresses the contaminant transport part of a PA, whereas the exposure part is better addressed through variability (population characteristics per EPA's description of probabilistic risk assessment). Variability should not be used directly in the projected behavior of the system except to inform the uncertainty in the mean estimates.

An issue with section 1.1.4.1 is that it addresses modeling for the sake of modeling. Modeling should be performed in some context of the decisions to be made. Compliance decisions are insufficient for evaluating the efficacy of a disposal system and optimizing disposal, which are critical for maximizing use of these precious resources (disposal systems). Modeling should be performed to evaluate options, in which case a framework for options analysis is needed within

which the modeling implied here should be performed. The beginnings of addressing this appear in Item (10) of Section 1.1.4.1, but this does not go far enough.

§ 1.1.4.2 and 4.0 Inadvertent Intrusion Assessment

Consider this text from p. 1-5: "Because there is no scientific basis for quantitatively predicting the probability of a future disruptive human activity over long timeframes, an inadvertent intruder assessment does not consider the probability of inadvertent intrusion occurring."

This simply is not true. This has been done before and can be done again (at NNSS and WIPP, for example). We are not in the role of prediction, we are in the role of modeling. We are modeling a reasonable facsimile to provide insights into what might happen, while conditioning on current knowledge of the system. Otherwise, a dose assessment beyond a few decades (let alone hundreds of years, or even thousands) is futile.

We do not know what the future holds—we know that it will be different from today, but all we can do for decisions that depend on the long term future is to project current conditions as we best understand them. This applies just as much to inadvertent intrusion scenarios as the environmental system.

Using IA to establish WACs is a mistake. This will not allow the nation to effectively use the limited disposal facilities that we have, and will arbitrarily cause disposal facilities to function sub-optimally. The arbitrariness is palpable. For example, this would cause sites in less populated areas to be evaluated the same as sites in more populated areas, since the probability of intrusion is assumed to be 1 for either case in the IA. This approach subverts the idea that site-specific analyses should drive decisions. Forcing the probability of intrusion to be unity removes the site-specific nature of the probability of intrusion, which can be a significant discriminator of between sites. WAC should be based on site-specific PA, not on IA.

The IA should be folded into the PA, and intruders should be addressed as potential receptors just like any other MOP, with probability of occurrence included in the analysis. All animals are equal, and some should not be more equal than others. Our approach in developing models has been to develop appropriate probabilities of exposure for all receptors, without labeling them as MOP or IHI, which is an artificial and sometimes misleading distinction. Models can provide for a special case in forcing the probability of an even (e.g. drilling through waste) in order to examine the consequences of that event in detail, but those consequences should not enter in decision making out of context, which is what the IA does. We expect that this approach will fit into the proposed requirement of having both a PA and an IA by using the very same model, with the only difference being forcing of a particular (and otherwise potentially unlikely) event. That special case of the PA modeling would be an IA, but the resulting IA should not be considered out of context of its probability of occurrence.

It is therefore curious that the last statement in Section 1.1.4.2 is that "An intruder assessment shall... Account for uncertainties and variability" when the largest uncertainty may be whether an intrusion event would occur at all.

On a positive note, it is encouraging to see the language that an inadvertent intruder is in fact a member of the public (§2.2.4.2, p. 2-14, lines 39-41.)

§ 1.1.4.3 and 5.0 Site Stability Analysis

The point of the stability analysis is not clear. How will it be evaluated? It needs metrics. We strongly suggest that risk (dose) be used to evaluate stability, along with cost. That is, this should be folded into the PA, with potential options for stability evaluated. Stability for the sake of stability, removed from the context of risk, is not useful except for appearances.

§ 1.1.4.4 and 6.0 Protective Assurance Period Analyses

This statement is most welcome: "The primary purpose of the protective assurance period analyses is to provide information that demonstrates that releases of radioactive from a LLW disposal facility are minimized during the protective assurance period..." What is refreshing here is the use of the term "releases", which makes the analyses reminiscent of the Containment Requirements for transuranic waste, in EPA's 40 CFR 191. Given the long time frame of these analyses, it is good to be rid of the term "dose", since this is burdened with the unfathomable uncertainties of human behaviors past 10,000 years.

Later discussions of the protective assurance period analyses sometimes revert to the use of the term "dose" as a performance metric, and we encourage that such references be modified to make reference to "releases" instead.

§ 1.1.5 and 9.0 Waste Acceptance Criteria

As mentioned above, a site-specific WAC should be based on a site-specific PA, and not on an IA, or even on the classification tables.

At the end of section 1.1.5 is a bulleted list of requirements for a certification program. Missing from this list is the characterization of uncertainty in waste documentation. In our experience, having developed PA models for over half a dozen radioactive waste disposal sites, the uncertainty in inventory is the most significant and most irreducible variable in the model. Unless positive steps are taken to require generators to characterize uncertainty in their wastes, this will continue to be the case. Without this uncertainty characterization, decisions about waste acceptance and disposal site operations will continue to be clouded, leading to inefficiencies in the *de facto* national radioactive waste disposal program and a squandering of the precious resources that are waste disposal facilities.

§ 1.4 Risk-Informed Approach

The term "risk-informed" is promising, but there is little of substance here. How should a risk-informed decision be made? This guidance sets up an approach based on *ad hoc* decision making—some people get together and make a decision. We have the technology to do better than that. "Risk-informed" would have meat if it were framed in a decision analysis context. This is the paradigm shift that is needed to support effective decision making, and remove the confusions that stigmatize this industry.

§ 2.2.1 Data Adequacy

It is unclear what is meant by "some amount of incompleteness in the data may be overcome by appropriately accounting for parameter uncertainty." This indicates a confusion of ideas.

Incompleteness of data leads to greater uncertainty in the sense that there is less data than there might have been otherwise. Parameter uncertainty is based on the amount of data available. Hence, this sentence makes no sense. We suggest it be deleted.

Note also that there are other ways to obtain data to support a specific parameter. These include model abstraction and meta analysis. It's not clear what is meant by "last resort" for expert elicitation. There is nothing inherently problematic with expert elicitation. If "last resort" is meant to imply cost-effectiveness, then this should be stated, but it is incorrect to imply that expert elicitation is not a reasonable approach, which seems to be the intent here.

Upscaling is not needed only to achieve representativeness. It is critical to the whole approach of modeling fate and transport of the radionuclides. A probabilistic PA must be built around averaging over large spatial and temporal scales, which is the essence of upscaling (though the analyst needs to be careful about what is being averaged, per the precipitation example). This is how a contaminant fate and transport model ultimately characterizes and addresses uncertainty. Then receptors are exposed to average concentrations and the uncertainty associated with that average.

§ 2.2.2 Uncertainty

The final sentences of Section 2.2.2 state, "For example, an uncertainty analysis could provide information about where a licensee should focus model support activities, which in turn could reduce uncertainty. Parameter uncertainty is uncertainty in the parameters used in the technical analyses." This is not the role of uncertainty analysis—this is the role of *sensitivity analysis*, which is not discussed much in this document. Uncertainty analysis (UA) addresses uncertainty in the decision. Sensitivity analysis (SA) addresses what drives the outcome and how to reduce uncertainty. There should be a separate and distinct discussion of sensitivity analysis to clarify the different roles of UA and SA.

§ 2.2.2.1 General Structure of Uncertainty

It is not clear that uncertainties are greater for long-lived waste. In fact some things become certain for long-lived waste even if the exact time frame is uncertain (the exact time frame is not very relevant in deep time). For example, in the Clive DU PA Model, it is relatively clear (not uncertain) that in 2 million years (My) all waste disposed below grade will be part of an organically developed geologic repository (i.e., under about 300 m of sediment). Whereas, for short lived waste disposed above grade, for example, the waste will be dispersed at some point well before 2 My. DU does not actually reach secular equilibrium until about 2.1 My—there is a lot of certainty then, even if the exact time is not known. The sentence needs to be qualified.

§ 2.2.2.1.1 Scenario Uncertainty

A challenge here is that probabilistic modeling essentially considers a continuum of scenarios. For example, climate variation spans a continuum of possibilities, which can be handled through probabilistic modeling (i.e., through probabilistic specification of input parameters). Scenarios are best left for truly discrete distinctions that cause the system to move in a different direction, and not for changes that are unknown but possible across a continuum.

§ 2.2.2.1.2 Model Uncertainty

In this case, different distinct models could be proposed. So, model uncertainty should involve evaluation of distinct models (for example, there are several models of tortuosity in the literature—which one is best for a specific PA and site?) For this type of model uncertainty, some model averaging could be brought in to support the PA effort.

Conceptual model uncertainty, on the other hand, can probably be handled for the most part through uncertainty characterization of the parameters. Simulation uncertainty is different yet again, and should be separated into its own section. Model uncertainty, conceptual model uncertainty, and simulation uncertainty are really all different enough that they need separate sections.

§ 2.2.2.1.3 Parameter Uncertainty

Parameter uncertainty is not reducible or irreducible. Parameter uncertainty is simply reducible by collecting more data/information. It could be argued that variability is irreducible, but variability should not be applied to the fate and transport model (except to support development of uncertainty estimates—more or less standard deviation (variability measure) divided by n). This is always a difficult discussion because of the number of literature articles on the subject, most of which cause greater confusion. It would be better to define terms here, and, since these are statistical/probabilistic issues, that should be the basis for the definitions.

Note that the approach to parameter uncertainty depends on how the simulations are performed. Most models involve drawing random numbers at the beginning of time, and then using those as deterministic values through the model's propagation in time. If, instead, random numbers are pulled at each time step, then some further thought needs to be put into how parameters are probabilistically specified.

Note also that in a probabilistic risk assessment (which is essentially what a probabilistic PA is), the exposure parameters need to cover the potentially exposed population, in which case they are based on variability rather than uncertainty. If the terms uncertainty and variability are not palatable here, then at least it should be recognized that some parameters (fate and transport leading to concentration assessment) are addressed through upscaling (distributions of means, essentially, which implies characterizing uncertainty), whereas, some are addressed without upscaling (because PRA is about addressing all members of the population, not the average member).

§ 2.2.2.1.4 Uncertainty Example—Transfer Factors

Applying conservative values *does not address uncertainty*. It addresses misplaced value judgments. It obfuscates effective decision analysis and decision making.

This document would benefit from further sections that specifically address upscaling and correlation. Why do large variances only require careful treatment in an intruder analysis—this makes no sense. Since transfer factors are applied to fate and transport components, they address uncertainty and upscaling. The degree of skew in the supporting data (or expert opinion) is mitigated to some extent through upscaling. It is important that the fate and transport parameters (those that are used to lead towards estimation of concentrations in various media, locations, etc.) are established as upscaled (averaged) quantities. The care that needs to be taken in upscaling

must address the dilution effect mentioned. The role of inter- and intra-site values is not made clear. This is an issue of ability to generalize data from one site to another. In general, we probably do not assign sufficient uncertainty to that generalization, but a combination of data from another site and expert opinion can be used to at least address some of the missing uncertainty. Note again that this must be in terms of uncertainty in the upscaled values.

Conservative values should not be used. One is reminded of the question asked Charles Babbage: "Mr. Babbage, if I provide you the wrong inputs, can your machine produce the right output?" His answer was, "I fail to understand the confusion of ideas that led to such a question." And yet, that is what is proposed here. If there are no data, then use expert elicitation. If there are no experts, then put a wide distribution in, and let the sensitivity analysis determine if it matters. If it matters, then put more effort into it (at greater cost.) If it doesn't matter, then leave it alone and move on.

Page 2-10, line 24: Replace "uncertainty" with "sensitivity". This is an example of the confusion between the UA and SA concepts that exists in this document.

If this section is to remain, it should be re-cast as a section on data generalization issues, instead of being presented as an uncertainty example.

Also, the NRC Regulatory Guide 1.109 (NRC 1977) should not be cited as an example, as it is quite dated and has values in it with no support.

§ 2.2.3.1 Peer Review, Expert Judgment, and Expert Elicitation

Peer review does not belong in a section with expert judgment and elicitation. Peer review is a form of model support (which could be described in Section 2.2.3), whereas expert judgment and elicitation are methods to specifying a model (akin to data collection). These are different concepts that do not belong in the same section.

§ 2.2.4.1 Human Activity - Scenarios

Receptor scenarios should be based on site-specific knowledge, and projected into the future based on that knowledge (i.e., conditioning on current knowledge). The point is made more effectively on page 2-16, line 11, but should be reinforced in this section. This is also an example of how use of the term "FEPS" works better than merely "FEP" (see editorial comments below.)

§ 2.3.1 Context of the Performance Assessment

Although the data quality objectives (DQO) process has some problems in its implementation, philosophically it is a sound rendition of the scientific method. It basically starts with the question, "What decision needs to be made?" The same should be true here. The decisions include compliance, but much more importantly, should include optimization of waste disposal: the best cover design, best placement of waste, best institutional controls, etc. Rather than DQOs, it would be better if this were all framed in a decision analysis construct. More specifically, it should be framed as a stakeholder-engaged structured decision making process—this is the paradigm shift that is needed so that the public is protected rationally and defensibly, while allowing for the disposal of waste in responsible ways.

§ 2.3.2 Approach to Different Time Frames

Evaluation against these time periods is unfortunate. It is not clear why radionuclides are treated so differently technically than wastes that will never change and might pose greater hazards (lead, arsenic, asbestos), and other wastes that have effective decay sequences that lead to more hazardous waste (PCE to TCE to VC, for example). There are several reasons why 1,000 years is too long—these include reasonableness of evaluating dose beyond 200 to 300 years, change that is inevitable, vastly increased uncertainty with time, and economic considerations (e.g., discounting, which is a natural phenomenon) coupled with the need for long term financial planning. There is a great confusion of regulations and guidance across different radioactive waste issues, let alone expanding further to include hazardous waste regulations. There are occasional opportunities to make real effective change. This is one of them, but this is not being achieved.

§ 2.3.2.3 Performance Period

The reference to "releases" (p. 2-23, line 24) is appropriate, but that positive is quickly negated with mention of the metric "peak annual doses that are projected to occur after 10,000 years" (p. 2-23, line 30, and again on p. 2-24, line 15). We recommend sticking to the "releases" or "activity concentrations or fluxes" concept, as invoked again at the top of p. 2-24.

§ 2.3.2.4 Site Characteristics

The term "C" in the equations on p. 2-25 (lines 26 – 28) requires a careful definition in order for it to be implemented consistently. Is this the activity (or mass) concentration in disposed wastes at the time of closure? At 1000 years after closure, accounting or decay and ingrowth? Some other time? IS it radionuclides that are in the original waste volume, or would it account for radionuclides that have been transported elsewhere in the environment? These questions must be answered, or an analyst will be forced to guess what the intent is.

§ 2.5 Scenario Development

The use of stylized scenarios is potentially problematic. Their use implies conservatism, which is essentially misplaced value judgments. It is perfectly fine to make conservative decisions, but it is not fine to make important decisions based on conservative models. Conservatism should be addressed through value judgments, so that it is properly characterized.

FEPS screening should take place, and should be based on an understanding of probability and consequence. Refer to *The Foundations of Statistics* (Savage, 1954). This reference lays out exactly how models should be built, what considerations should be given, etc. It addresses both marginalization (ignoring distinctions between events), and conditioning (ignoring events), and offers useful insight for how a FEPS screening process could be implemented.

§ 2.5.3.1.2.2 Probability

Page 2-42, last bullet, lines 3-4: It's interesting that FEPSs that do not have information or data, and nothing is known about the process, are included automatically here. There are many processes that are never included in a PA because we don't know how to include them. For example: microbiological degradation of containers, colloid transport. However, NRC implies

here that they should be included.

Uncertainty associated with probability: Possibly most FEPSs evaluations would have to be evaluated based on expert elicitation. It is difficult to see how the FEPSs can be evaluated probabilistically otherwise. At the very least, expert elicitation is likely to play an important role. Perhaps this can be noted and referenced to the section on expert elicitation. Some more explanation is needed here. The Yucca Mountain Project evaluated the probability of volcanic hazard as part of the FEPSs process (at very large cost). This was done by building a complex model that led to (uncertain) estimates of this probability.

§ 2.7.4 Analysis and Evaluation of Results

This section discusses, among other things, the concept of SA, but subtly mischaracterizes it in saying that "...the purpose of sensitivity analysis is to evaluate uncertainty and variability in the assessment. (p. 2-61, lines 16-17.) The purpose of an SA is to identify which model input parameters contribute most to that uncertainty. Further discussion on this page revolves around the flawed approach of on-at-a-time (OAT) SA, which will not allow one to thoroughly evaluate the contributions of input parameters to uncertainty in the model results. The text on p. 2-62, while a bit garbled, attempts to put OAT-SA in its appropriate context, identifying its limitations, but this OAT-SA approach needs to be more forcefully deprecated.

§ 3.2.7 Direct Release

It is interesting to see the reference to "Anthropogenic direct releases..." (p. 3-16, line 41.) This would suggest that releases of radionuclides that might occur from intrusion into wastes should be considered as part of the contaminant transport in a model. For example, drill cuttings brought to the surface should be incorporated into the larger contaminant transport calculations, as they could result in exposures to not only the drilling crew but to other receptors as well, perhaps much later in time. We agree with this approach.

§ 4.0 Inadvertent Intrusion

See our comments above about why the IA is not useful for making decisions about radioactive waste disposal.

§ 4.3.1.1 Generic Intruder Receptor Scenarios

A generic IA is of even less use than a site-specific one.

At lines 12-13 on p. 4-13, the statement is made that "Loss of intuitional control is not expected..." Quite the opposite is true. Loss of IC is certain—it is only a matter of when control is lost.

§ 4.3.1.1.2 Intruder-Drilling Receptor Scenario

The drilling referred to in this scenario is for water wells. It should be made more generic to include drilling for petroleum resources (gas and oil) as well.

§ 4.3.2.2 Source Term

Included in an exposure source term should be not only radionuclides in the original waste layers, but those that have migrated upwards into the column above the waste, towards the ground surface. These, especially the decay products of ^{222}Rn which may have diffused upward, can add significantly to the receptor dose.

§ 4.3.2.2.1 Inventory

The last paragraph on p. 4-31 has problems. It says that licensees "may conservatively assume no decay." This is at odds with the very next sentences, which recognizes the "impacts of significant progeny", and that "radioactive decay can result in significant ingrowth of progeny at future times." ("at future times" should be deleted, as it is redundant.) As we know, using depleted uranium as an example, assuming no decay is in some cases definitely not conservative. The following sentence attempts to make this clear, but should be changed from "For example, doses from depleted uranium may increase for more than one million years due to ingrowth of shorter-lived and more highly mobile decay products." to "For example, activities from depleted uranium will increase for more than two million years due to ingrowth of shorter-lived and more highly mobile decay products."

§ 5.0 Site Stability Analyses

As stated above, we do not see the need for a special site stability analysis. Site stability and its consequences should simply be modeled as part of the PA. If an unstable site produces no added risk, then why should we care? We agree that all the processes that contribute to loss of stability, as discussed in the subsections of 5.0, should be included in the FEPS analysis, and if they survive screening, in the PA modeling. The consequences to receptor dose will then naturally fall out of the PA.

Lines 17-19 contain an adequate definition of "stability" that should be used as a definition in 10 CFR 61.2. The current definition in 61.2 is self-referential and wholly inadequate.

§ 6.2.1.1 Alternatives Analysis

An alternative analysis as described is a decision (options) analysis. It should be described this way, and should describe how such an analysis is performed. It seems that this is simply a comparison of doses, but, if so, that is inadequate. Doses can be reduced to zero if money is no object (put a titanium box around the whole facility), but in reality the costs can be prohibitive, and still might not satisfy all stakeholders (because in that simple example, the waste is still there). Based on this approach, the titanium box is the best alternative, because cost doesn't come into it. This discussion should be deleted, or merged with the next section while maintaining the title of this section (that is, alternatives analysis should include costs).

This section also references "peak of the mean". This is a very poor decision metric for sites that have no receptors for long periods of time. This is why PA decisions should be based on population risk instead of individual risk (which was part of the original basis for the selection of many of the disposal sites in the country anyway).

We applaud the consideration of alternative sites for disposal. The final paragraph of this section hints at a larger scope of decision making with respect to radioactive waste disposal for the nation. IT is appropriate to consider that one site may be more suitable than another for disposal

so specific wastes, and yet this has received very little discussion in the waste disposal community. We are happy to see the issue raised here.

§ 6.2.1.2 Minimization Analysis

Discounting is difficult to deal with, but perhaps because of how little research has been done to date. A reframing of the decision problem to address both discounting and a long-term financial plan. With discounting, including discounting of receptor doses (contrary to bullet 2 on p. 6-8), compliance periods become unnecessary. Recall Neptune's presentation at the Spring 2013 meeting of the LLW Forum in Charleston, SC.

While we understand NRC's position, some further research on discounting coupled with financial planning seems warranted. At the moment the effective discount function on dose is zero discounting until the end of the compliance period, and then complete discounting (zero value) thereafter. (NRC has inserted one more step in there potentially with the second period having a different dose limit). This is not a reasonable discount function, so some research is needed.

A better approach would be to shorten compliance periods, and have financial planning that implies discounting. The net effect of the revolving time window that this approach would engender would be longer term management of the disposal facility. Under current regulations, at the end of institutional control the Government can simply walk away—does it make sense to make decisions under that paradigm? A paradigm shift is needed.

Also, decision analysis for radioactive waste disposal should depend on population dose rather than individual dose. Or, better, it should depend on risk. Even better, it should depend on mortality and morbidity, as is done in risk assessment within the medical community. Dose is used as a proxy for these, but it is not a good proxy.

§ 6.2.1.3 Other Decision Analyses

It is not clear how this would differ from what is presented in Section 6.2.1.2. Please clarify.

§ 7.0 Performance Period Analyses

p. 7-1, line 26 contains the statement, "The level of detail in the assessment should be risk-informed". It is not clear at all what that means. The term risk-informed has been used for several years now, but without definition. Metrics are needed to make decisions. What are the risk-informed metrics in this case?

The footnote to Table 7-1 says "Any isotope [sic] that is to be disposed of in sufficient quanitites should be considered as part of the LLW PA inventory." Please define "sufficient quantities".

Top of p. 7-4: It is not clear why more expert judgment is needed in this case. This seems to be a continued "knock" on expert elicitation as a "poor man's data analysis". This is not the case. Expert elicitation should be used when it is most cost-effective to do so. Since these long-term models essentially project today's conditions into the future, it is not clear why longer term modeling requires more expert elicitation. Most of the input distributions will not change from the 1,000-year model.

Further, why would simple conservative analyses be used? The model is already set up for 1,000 yr (unless there are new events)—it is trivial to project out the same model for 10,000 yr or longer. This section should be re-written. As former Commissioner Magwood once said a problem we face in waste management is "conservatism on top of conservatism on top of conservatism". Conservatism has no place in modeling for important decisions. Its place is in specification of value judgments in a complete decision analysis, based on realistic—not conservative—analysis.

Otherwise this section seems to ramble some, and might benefit from some reorganization and deletion of material.

p. 7-4, line 28: Change "is variability in hydrogeology" to "may be variability in hydrogeology". There are sites where hydrogeology is simply unimportant.

Editorial Comments

"FEPS", not "FEP"

This is not merely an editorial comment. We strongly recommend that the traditional term "FEP", for features events, and processes, be replaced with the term "FEPS", which includes receptor scenarios. Receptor scenarios are not merely the result of the assessment of other FEPs—they are fundamental to the scoping of analyses, and deserve "top billing", in the principal acronym. The development of modeling scenarios indeed is the product of the analysis of FEPs, as the document discusses, but such scenarios are also dependent on the receptor scenarios that are identified as being fundamental to the scoping of a PA. As an example, section 2.6, Conceptual Model Development, should include receptor scenario development as a foundation leading to CSM development.

This guidance is a great opportunity to start making this point more clear to the PA community. Please consider replacing all instances of "FEP" with "FEPS".

"radionuclide", not "isotope"

The word "isotope" is used throughout the document, in most cases incorrectly. There are a few cases where it is used appropriately to mean various isotopes of a single chemical element (e.g. p. 2-25 line 32, in reference to "uranium isotopes"), but in most cases, "isotope" should be replaced with "radionuclide", or even simply "nuclide". This is something NRC should strive to correct in general. Isotopes have the same number of protons, and hence the same atomic number. Isotones are chemical elements having the same number of neutrons. Isobars have the same atomic mass, meaning the same sum of protons + neutrons. All are nuclides, and radioactive ones are radionuclides. The difference in usage is perhaps best illustrated by example. These are isotopes: ^{234}U, ^{235}U, and ^{238}U (same number of protons, therefore same atomic number. These are not isotopes, though they are isobars: ^{238}Pu, and ^{238}U (same atomic mass, P+N). These are also *not* isotopes, but are isotones: ^{29}K and ^{37}Cl (same number of neutrons). These are none of the above, but are radionuclides: ^{3}H, ^{36}Cl, ^{99}Tc, ^{222}Rn. All of the above examples are radionuclides, and that is the best term to use, unless there is a specific reason to use "isotopes", as in, "All isotopes of uranium share the same geochemical

characteristics."

"naturalization"

In the context of the degradation, deterioration, and alteration of engineered features of a disposal facility, such as the cover, we would promote use of the term "naturalization," first introduced at the NRC Workshop on Covers. "Naturalization" includes all these processes, and more, and suggests the idea that these processes are natural and inevitable, and that engineers should work with nature in developing designs that will age well.

"disposed" not "disposed of"

Inclusion of the word "of" in essentially all occurrences is superfluous.

minor edits

p. 2-4, line 38: Remove "large quantities of", since depleted uranium "may pose a long-term risk to the public" in even small quantities. Don't let previous disposals of DU hide behind the "large quantities" terminology.

p. 2-5, line 2: Change "predominately" to "predominantly".

p. 2-24, line 2: Remove "rate". A flux is already a rate, as in mass per time. A "flux rate" is nonsensical, and would mean mass per time per time . See: Stauffer, P.H. (2006). "Flux Flummoxed: A Proposal for Consistent Usage". Ground Water 44 (2): 125–128.

p. 2-28, line 42: Change "which to "that". There are other instances of this grammatical error throughout the document. Fortunately, you got most of them (though introduced some new instances) in the proposed 10 CFR 61 rule changes.

p. 2-30, line 24: Change "Sandia National Laboratory" to "Sandia National Laboratories".

p. 2-42, line 20: Change "phenomena" to "phenomenon".

p. 2-42, line 32, and p. 2-55, line 43: Change "are" to "be".

p. 2-47, line 41: Replace "less" with "fewer".

p. 2-49, line 13: Remove "a"

p. 3-22, line 17 and p. 12-7, second reference: The last name of Ghislain de Marsily is "Marsily, not "de Marsily", and certainly not "deMarsily". He told me this personally. He should be referenced as "Marsily, G. de"

p. 7-8 *et seq.*, Examples 7.2 and 7.3: Use SI units: Bq (or MBq) instead of Ci.

This concludes comments from Neptune and Company, Inc. on the proposed *Guidance for Conducting Technical Analyses for 10 CFR Part 61* (NUREG-2175).