PEER REVIEW OF EPA S HAZARDOUS WASTE IDENTIFICATION RULE RISK ASSESSMENT MODEL

Background Document for the Ecological Exposure and Ecological Risk Modules for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) Software

Prepared for:
David Bartenfelder
Office of Solid Waste
U.S. Environmental Protection Agency
2800 Crystal Drive
Arlington, VA 22202

EPA Contract No. 68-W-99-001
Work Assignment No. 106

Prepared by:
Eastern Research Group, Inc.
2200 Wilson Boulevard, Suite 400
Arlington, VA 22201-3324

December 17, 2000

NOTE

This report was prepared by Eastern Research Group, Inc. (ERG), an EPA contractor, under Contract Number 68-W-99-001. The report presents comments provided by peer reviewers on the *Background Document for the Ecological Exposure and Ecological Risk Modules for the Multimedia, Multipathway, and Multiple Receptor Risk Assessment (3MRA) Software System* document that is part of EPA's Hazardous Waste Identification Rule risk assessments.

The comments presented in this report have been compiled by topic and by individual peer reviewer. As EPA requested, this report provides the peer review comments exactly as they were submitted to ERG. Also attached are the original comments submitted by each individual reviewer.

Peer Review Charges for the HWIR Ecological Exposure and Ecological Risk Methodology

Background

The multi-media, multiple pathway and multiple receptor risk analysis (3MRA) model was designed to establish safe, constituent-specific exit levels for low risk hazardous wastes under the Hazardous Waste Identification Rule (HWIR). Wastes to be assessed under HWIR are those currently designated as hazardous because they were listed, or had been mixed with, derived from, or contained listed wastes. One of the intended outcomes of HWIR is to reduce possible over-regulation arising from application of the "mixture" and "derived-from" rules that were promulgated as part of the first comprehensive regulatory program for the management of hazardous wastes under RCRA in May of 1980. Both of these rules remain important in reducing risk to human health and the environment associated with the management of hazardous wastes; however, because they apply regardless of the concentration or mobility of hazardous constituents in the wastes, they also open the possibility of over-regulation. Therefore, one of the primary purposes of 3MRA is to provide a tool for identifying possible instances of over-regulation, and to provide an avenue for the safe relief from Subtitle C disposal regulations.

In December of 1995, the Agency proposed a methodology designed to identify the exposure pathway associated with the highest predicted risks to both human and ecological receptors. This methodology constituted the first multi-media risk assessment tool developed to support risk-based exit levels (i.e., acceptable chemical concentrations in wastes), and was referred to as the Multiple Pathway Receptor Analysis (MPRA). It utilized the revised EPACMTP modeling approach for the groundwater pathway analysis, and the indirect exposure methodology for other pathways. The MPRA was designed to simulate each exposure pathway independent of other pathways, and the model was parameterized such that the contaminant fate and transport favored one pathway for each simulation. That is, the parameters to which each pathway was most sensitive were set to high end values, and the model was executed to drive risks to one pathway at a time (i.e., contaminant losses to other environmental media were not tracked). During an extensive series of reviews of the MPRA, the EPA Science Advisory Board (SAB) and others urged the Agency to consider using a simultaneous, mass-constrained analysis that would account for dispersal, transport and transformation of contaminant mass through all media and exposure routes. This was perhaps the most important and strongly expressed element in all of the review comments received.

The goal of the 3MRA is to identify wastes currently listed as hazardous that could be eligible for exemption from hazardous waste management requirements. The HWIR99 risk assessment predicts chemical-specific potential risks to human and ecological receptors living within a radius of 2 kilometers of industrial nonhazardous waste sites that could manage HWIR-exempted waste. These risk estimates, along with other information, may be used to identify the chemical-specific concentrations for exempted waste that would be protective of human health and the environment at selected sets of risk protection criteria.

The 3MRA assessment strategy provides a methodology to evaluate multiple exposure pathway risks to human and ecological receptors at a statistically representative sample of waste management units (WMUs) and associated environmental settings to estimate the distribution of risk nationally. It is a

forward-calculating approach that begins with selected concentrations of a chemical in waste, and estimates the associated hazards and risks to human and ecological receptors.

The risk assessment is designed to produce chemical-specific distributions of cancer risks or hazards to humans and ecological receptors living in the vicinity of industrial waste sites that could manage HWIR-exempted wastes throughout their operating life. For each site and waste concentration, the model generates risks for each receptor location and then sums the number of receptors that fall within a specified risk range (bin) to get the distribution of risks for the population at each site. We can use the distribution of risks for a setting to determine whether the setting is protective based on the percentage of the population protected, a specified cancer risk or hazard level, and the initial concentration in waste. The model then uses these data to generate a percentile distribution based on the number of settings protected at a specified risk level for each waste concentration to generate the national distribution.

The 3MRA model consists of 17 media-specific pollutant fate, transport, exposure, and risk modules; 6 data processors to manage the information transfer within the system; and 3 databases that contain the data required to estimate risk.

As shown in Figure 1, the 3MRA Model incorporates the following interacting modules:

- Source modules, which estimate the simultaneous chemical mass losses to the different media and maintain chemical mass balance of the releases from the waste management unit into the environment

- Fate/transport modules, which receive calculated releases from waste management units and distribute the mass through each of the media to determine the chemical concentrations in air, groundwater, soil, and surface water across space and time

- Food chain modules, which receive the outputs from the fate and transport modules and estimate the uptake of chemicals in various plants and animals

- Exposure modules, which use the media concentrations from the fate and transport modules to determine exposure to human and ecological receptors from inhalation (for humans only), direct contact (for ecological receptors only), and ingestion (for both receptor types)

- Risk modules, which predict the risk/hazard quotient for each receptor of concern.

Ecological Exposure and Risk Modules

The Ecological Exposure (EcoEx) Module receives an annual average time series inputs of environmental media concentrations from the Watershed and Surface Water Modules; and food item concentrations from the Aquatic Food Web and Terrestrial Food Web Modules. These concentrations are then converted into receptor doses in the Ecological Exposure Module for use as inputs to the Ecological Risk (ER) Module. In addition, the Ecological Risk Module also receives concentrations in soil, sediment, and surface water

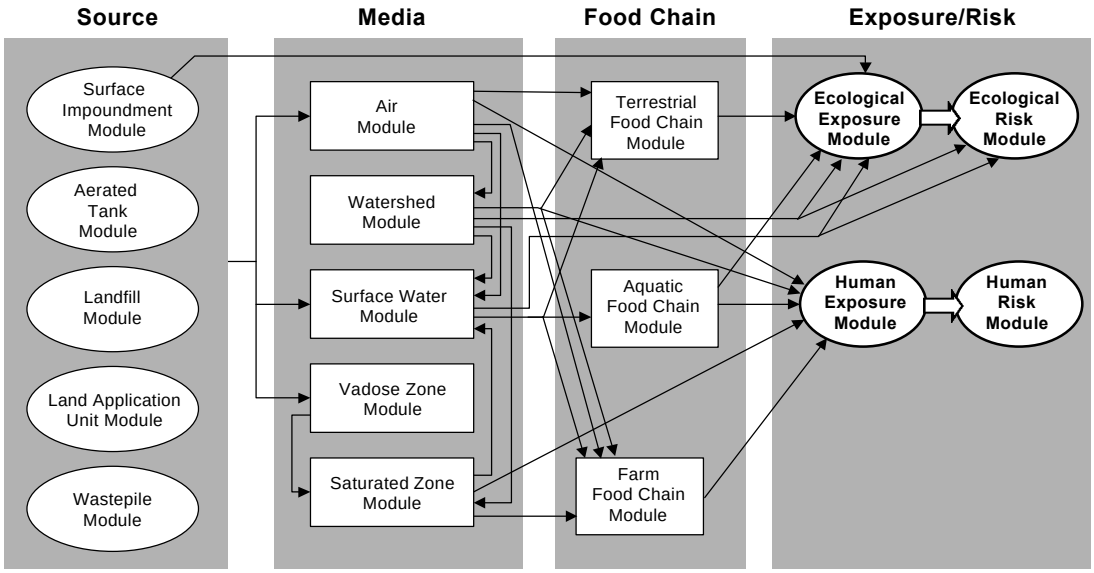


Figure 1. Source, fate, transport, exposure, and risk modules of the 3MRA Model

and compares those values to media-specific Chemical Stressor Concentration Limits (CSCLs) for other ecological receptors such as the sediment community. The Ecological Risk Module outputs are risk statistics aggregated by various descriptors (e.g., habitat type; receptor group) and are provided to the 3MRA Exit Level Processors (ELPs) I and II for national aggregation across sites. A detailed description of the EcoEx and EcoRisk Modules is provided in *Background Document for the Ecological Exposure and Ecological Risk Modules for HWIR99 Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) Model*, (EPA, 2000). A summary of the key functionality provided by these two modules is summarized below.

1. *The EcoEx Module calculates the constituent-specific applied dose (in mg/kg-d) to ecological receptors that are exposed to contaminants via ingestion of contaminated plants, prey, and media (i.e., soil, sediment, and surface water). In essence, the module estimates an applied dose for birds, mammals, and selected herpetofauna that reflects the spatial and temporal characteristics of the exposure.*

2. *The EcoEx Module calculates exposures for each receptor home range placed within a terrestrial or freshwater aquatic habitat (as defined in the site layout). Thus, exposure is a function of: (1) the home range (or portion, thereof) to which the receptor is assigned; (2) the spatial boundaries of the home range, (3) the food items (plants and prey) that are available in a particular home range, (4) the dietary preferences for food items that are available, and the media concentrations in the receptor's home range.*

3. *The EcoRisk Module calculates hazard quotients (HQs) for a suite of ecological receptors assigned to habitats delineated for study sites. These receptors fall into eight receptor groups: (1) mammals, (2) birds, (3) herpetofauna, (4) terrestrial plants, (5) soil community, (6) aquatic plants and algae, (7) aquatic community, and (8) benthic community.*

4. *The EcoRisk Module assigns the HQs for all receptors assigned to the study site into one of five risk bins: (1) below 0.1, (2) between 0.1 and 1, (3) between 1 and 10, (4) between 10 and 100, and (5) above 100. The module uses the HQ risk bins to develop cumulative distribution functions of risk.*

5. *The EcoRisk Module reports the attributes of each HQ that may be used to answer a variety of risk management questions. For instance, distance from the source (i.e., 1 km, 1 km to 2 km, or across the entire site) is important in understanding the spatial character of potential ecological risks.*

6. *The EcoRisk Module then determines and outputs cumulative HQ distributions during that critical year for which the maximum cumulative risk and/or HQ occurs across the ecological receptors at that site. These critical year outputs are also specified for each attribute such as receptor group (e.g., birds), trophic level (e.g., producers), and habitat group (e.g., terrestrial).*

Peer Review Charges

While reviewing the document, please address the following general issues:

1. Comment on the organization of the ecological exposure and risk document. Does the document present the information in a clear, concise, and easy to follow format? If not, please provide suggestions to improve the presentation.

2. Does Section 1 provide an adequate description of the purpose and context of the exposure and risk modules? If not, please explain.

3. As with any risk assessment, there are always additional data and method development efforts that could be undertaken to reduce the level of uncertainty. Are you aware of any major methodological or data gaps in the exposure and risk modules that have not been identified? If so, how should they be addressed?

In addition, the following specific issues should be addressed.

1. EPA determined that the smallest meaningful unit relevant to ecological risk within a national analysis should be defined in terms of representative habitats. The representative habitats serve as the basis for ecological receptor assignments, and provide the spatial boundaries used to assess exposure and risk. These representative habitats reflect a broad range of ecological settings, although all of the habitats share common characteristics. For example, EPA assumed that the representative habitats are capable of sustaining a variety of wildlife. Further, each habitat is assumed to be of sufficient quality to support multiple trophic levels (with at least one reproducing pair of upper trophic level predators), and predator-prey interactions for each habitat are represented by a simple food web. Do you agree with the criteria used to define representative habitats to address ecological risks? Would you recommend improvements or additions to the suite of representative habitats given the national context of this analysis?

2. Once the representative habitats were defined, EPA used a variety of GIS coverages (e.g., Anderson land use codes; National Wetlands Inventory) to delineate habitats at each site in the sample population. We developed a GIS-based tool that employed a weight-of-evidence approach in manually delineating the representative habitats. Using this tool, approximately three to five habitats were delineated at each site. Have we used appropriate coverages in delineating habitats? Are there other coverages or methods that would provide significant improvements over the methodology developed to support 3MRA? Is the delineation of habitats using GIS an appropriate method to define the spatial boundaries and relationships among ecological receptors, given the goals for this national assessment?
3. As described in U.S. EPA, 2000, receptor assignments for each representative habitat were based on extensive research on food web structure (e.g., guild theory and feeding strategies), representation of species based on geographic region, appropriate resolution given the fate and transport models in the system, and the availability of data relevant to exposure for potential species of interest. In addition, Bailey's ecoregions were used to determine the appropriateness of a receptor assignment for any given site location in the

contiguous United States. Consequently, the list of receptors assigned to a particular representative habitat (e.g., forest) could vary from location to location, although the master list for the forest would remain constant. Do you agree with our rationale for assigning receptors to representative habitats? Have we adequately represented a full range of feeding strategies and trophic elements across the simple food webs constructed for each habitat? Are there additional sources of information that you would recommend to provide meaningful improvements to the risk assessment results for national application?

4. As described in the report, the 3MRA system requires a single site layout file for each site in which the coordinates for all areas requiring spatial averaging are identified. A single random placement of four home range bins[1] was implemented for each site, and used to calculate the spatial averages for soil, plant, and prey concentrations in each home range bin. If the home range for a given receptor was larger than the habitat (i.e., extends beyond the area of interest), the exposure was averaged across the habitat and then prorated based on area (truncated to be not less than 10%). Is the random placement of home ranges and prorating a reasonable approach for estimating applied doses? If not, what other alternatives would you recommend?

5. The exposure profiles generated with the EcoEx module are based on the average annual concentrations in food items and media. Consequently, we recognize that one limitation of this analysis is that concentration spikes due to episodic events (e.g., rain storms) or elevated source releases following waste additions or control failures are not evaluated. In addition, to develop a methodology to support the risk assessment of low-level, long-term chemical constituent releases, we did not design the system to address acute exposures that might occur during sensitive life stages. Do you believe that this is a serious limitation, that is, that the inclusion of episodic events or failure scenarios would substantially change the estimates of ecological exemption levels? If so, what recommendations would you make to use the 3MRA system, available toxicological data, and ecological modules to allow for the evaluation of episodic events?

6. The data sources for ecological exposure factors are fully described in U.S. EPA, 2000, and rely heavily on EPA's Wildlife Exposure Factors Handbook (U.S. EPA, 1993). We believe that we have identified the most important sources of information; however, we recognize that additional data may have become available since this methodology was developed. Do you have recommendations for newly developed data sources that may improve the quality of the exposure factors database or allow us to include new receptors to improve the regional resolution of the receptor lists? Have the exposure data been derived appropriately for use in a national analysis?

7. Because media concentrations are averaged annually, the EcoEx module constructs the dietary preferences for each receptor based on dietary data covering one or more seasons. Some of the seasonal variability in the diet is captured indirectly by the hierarchical

---

[1] As described in the report, each receptor is assigned to one of four discrete home range sizes, depending on the receptor-specific home range size. The four home ranges overlap in a manner that reflects the predator-prey relationships.

algorithm used to determine the dietary preferences. However, the random preference algorithm is implemented on data across multiple seasons and, therefore, does not necessarily reflect seasonal differences. Although we believe that this algorithm is a positive step in addressing dietary variability and data deficiencies in prey preferences, it greatly simplifies predatory-prey dynamics. Are there refinements that you can suggest to the random sampling algorithm used to assign prey preferences?

8. The HQ methodology - the ratio of an exposure dose or concentration to a benchmark - is applied uniformly across all ecological receptors. Because the HQs are simple ratios rather than probabilities, they provide a relatively coarse metric to interpret the significance of ecological effects. Moreover, the HQ for a receptor population is not distinguished from an HQ for a receptor community (e.g., benthos) and, in fact, the former is based on a low adverse effects level whereas the latter is based on a statistical interpretation of effects to species within a community. These differences notwithstanding, the significance for these HQ values is implicitly assumed to be equal even though the effects may involve different levels of biological organization. Put another way, the HQ estimates for different receptor groups represent somewhat different risk metrics. Please comment on the value of calculating ecological HQs to represent risks to different levels of biological organization. Because the evaluation of ecosystem-level effects is beyond the scope of the site-based analysis, do you agree with the approach used to evaluate a suite of ecological receptors within the context of a habitat? Does this provide a meaningful representation of the potential ecological risks at a national level?

9. Data on chemical stressors are seldom available above the level of an individual organism; that is, the study endpoints focus on individual organisms rather than processes crucial to assemblages of organisms. Although reproductive and developmental endpoints have been used by EPA as relevant to population sustainability, they are not always the critical effect for an individual organism. Consequently, we implicitly assume that, for mammals and birds, endpoints associated with the populations' ability to reproduce and survive are an appropriate surrogate for true population-level endpoints (e.g., adverse effects leading to a 10% reduction in the population size). Do you agree with the selection of these endpoints, and do you have recommendations for expanding the development of benchmarks to include other, relevant endpoints? Please comment on the approach used to infer effects at the population and community levels based on the selected endpoints for this analysis (e.g., endpoints relevant to reproductive fitness, growth, and survival). In particular, we would appreciate your perspective on whether evaluating risks through inference constitutes a reasonable approach for a national analysis.

10. In generating risk estimates for each habitat, it was assumed that one and only one population of each wildlife species is carried by a given habitat. For example, although there may be a number of receptors assigned to a habitat, multiple populations of shrews or robins are not evaluated. Each receptor population has the same spatial characteristics, as defined by the home range. Hence, there is one HQ calculated for each receptor population in each habitat. Does this approach seem reasonable given the level of resolution for this site-based analysis? If not, what recommendations could you offer to incorporate carrying capacity and mega-population dynamics in the risk framework?

11. The HQ estimates for the aquatic and benthic communities, respectively, are resolved at the habitat level. Because the HQ estimate for the aquatic habitat reflects an average chemical concentration across all stream reaches in the habitat, it is possible that a highly contaminated reach would be "diluted" by other reaches in the habitat that are relatively pristine (e.g., upstream reaches). As a result, there is some uncertainty associated with calculating risks to aquatic life across an entire aquatic habitat defined within the area of interest. Species of fish such as brown trout tend to utilize certain segments of stream habitats; impacts at the segment level may be obscured by the average HQ for the habitat. Conversely, establishing artificial boundaries between stream reaches is contrary to the goals of the assessment strategy, namely, to evaluate ecological risks using the habitat as the fundamental unit. Do you agree with the appropriateness of this approach and, if not, are there alternative approaches that you could recommend?

12. The risk characterization of ecological HQs includes a qualitative framework to assign confidence indicators to the constituent-specific data set on ecotoxicity and bioaccumulation potential. The framework considers the adequacy of the available data to assess ecological risks to receptors across the trophic continuum given the physical and chemical properties of that particular constituent. The framework also considers the quality of data available for each constituent (e.g., how well we met data quality objectives), particularly with respect to toxicity to different receptor groups such as mammals and soil biota. Please comment on the appropriateness and utility of this framework as well as the adequacy of the databases on ecotoxicity and bioaccumulation potential for the constituents of concern. Indicate, as appropriate, additional sources of data that you feel could improve the quality of our databases. Do you have suggestions on how the confidence indicators might be implemented within the 3MRA system? Are there any improvements to the indicators that you could recommend?

References

U.S.EPA (Environmental Protection Agency). 2000. *Background Document for the Ecological Exposure and Ecological Risk Modules for HWIR99 Multimedia, Multipathway, and Multireceptor (3MRA) Risk Assessment Model*. Office of Solid Waste, Washington, DC.

U.S. EPA (Environmental Protection Agency). 1993. *Wildlife Exposure Factors Handbook. Volumes I and II.* EPA/600/R-93/187. U.S. Environmental Protection Agency, Office of Health and Environmental Assessment and Office of Research and Development, Washington, DC. December.

General Issues

General Comments

Dr. Sample: I have completed a rapid review of the HWIR Ecological Exposure and Ecological Risk Methodology. My review focused on the materials provided by ERG [i.e., *Background Document for the Ecological Exposure and Ecological Risk Modules for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) Software System*, and Appendices A through H of this document]. The authors of this model and supporting text should be commended for their not inconsiderable efforts to develop a comprehensive assessment approach for this complex problem. Unfortunately, I have found extensive problems with approaches, methodologies, and assumptions employed, in addition to numerous quality assurance/quality control (QA/QC) issues, that raise serious questions in my mind as to the utility and accuracy of the current model and it's output. Some general comments are presented immediately below. Responses to the charge questions, specific comments, and references follow.

In general terms, I found the presentation and description of the model convoluted and hard to follow. Much of the information that was needed to understand the derivation, development, and application of the model is not presented. Some (but not most) of the needed material is available in the supporting documents located on the World Wide Web (WWW), however, references to these documents and the supporting information are lacking. I would highly recommend copying descriptive material from the WWW into the background document.

Further, as with any modeling effort, model development (although important) is only part of what is needed to produce useful and valid results. Model parameterization with appropriate and defendable values is essential. Although values are presented for all parameters selected for the 3MRA model, insufficient information is provided to explain how these values were selected or derived, or from where they were obtained. Because of the high importance of this effort and the extreme scrutiny it will receive, it is imperative that the exact details of derivation of all parameter values be provided. They cannot be simply described in general terms (as is the case in the current draft).

An additional concern associated with parameterization relates to the methods by which concentrations of chemicals in wildlife dietary components are estimated. First, risk estimates are based on modeling concentrations of contaminants into 17 wildlife food types including worms, other soil invertebrates, small mammals, birds, herpetofauna, three types of fish, and 7 plant categories. Although aspiring to capture this level of detail is laudable, the current state of knowledge is insufficient to estimate bioaccumulation for all 17 categories for any single chemical, much less all the chemicals considered by 3MRA. As a consequence, many parameter values default to 1 (see Section 10 in Data Collection for the Hazardous Waste Identification Rule). Dietary compositions need to be simplified so that they are expressed in terms that are in accordance with available bioaccumulation modeling capabilities.

Second, the 3MRA modeling approach relies heavily on the Kow-based models for plants and beef presented in Travis and Arms (1988). As part of the development of the Ecological Soil Screening Levels (EcoSSLs) for EPA Superfund Headquarters (See EPA 2000), multiple errors and inconsistencies in the models presented in Travis and Arms (1988) were found. These errors render the Travis and Arms (1988) models unreliable and un-usable. Corrections to these models were completed, have been incorporated into the draft EcoSSL guidance (EPA 2000), and were recently presented at the 2000 SETAC meeting. It is recommended that all usage of models presented in Travis and Arms (1988) be removed from the 3MRA model and be replaced with the updated models.

Third, to estimate concentrations of inorganic chemicals in biota, the 3MRA model categorically excludes the use of regression models in favor of simple bioaccumulation factors (BAFs) (see Section 10 in Data Collection for the Hazardous Waste Identification Rule). Multiple studies (i.e., Sample et al. 1999, Sample et al. 1998, Bechtel-Jacobs 1998) have shown that BAFs grossly overestimate chemical concentrations in biota and that regression models produce more accurate and reliable results. For this reason, the EcoSSLs preferentially use regression models for estimating concentrations in wildlife foods. BAFs are only used if regression models are unavailable. To provide the most accurate estimates, the 3MRA model should make use of empirical regression models if they are available.

Under QA/QC concerns, multiple inconsistencies were identified between notations in models and those in the model legends (see Equations 4-4 and 4-5 as examples). Further, in Table 4-3, the 'a' parameter values for birds and mammals should either be presented to 3 decimals (as are the 'b' parameters) or rounded correctly. Bird and mammal values are presented in Table 4-3 as 0.64 and 0.23, respectively. In EPA (1993) they are reported as 0.648 and 0.235, for birds and mammals. The value for herpetofauna is simply incorrect; it is presented as 0.012 but should be 0.013. These inconsistencies may appear trivial, but given the ease with which they can be found and the high scrutiny that the 3MRA model will receive, they raise the concern that other more significant errors exist in the model. This concern is further compounded by the inadequate detail with which model components are presented. It is highly recommended that a detailed QA/QC effort be applied to the 3MRA documents, models, and data. Results and documentation of the QA/QC should be included with the documentation as an additional appendix or as another section of the supporting documentation on the WWW.

1. *Comment on the organization of the ecological exposure and risk document. Does the document present the information in a clear, concise, and easy to follow format? If not, please provide suggestions to improve the presentation.*

Dr. Fairbrother: Organization – the document is generally well-organized. Each section follows the same pattern, with a general overview, details of the approach, and a discussion of limitations. There was some redundancy, particularly in the introduction/overview of each section, but this was acceptable and made it easier to follow the logic.

Dr. Kapustka: The structure of the document is appropriate for the material presented. The authors made good use of figures, tables, and appendices to achieve good flow of information. Despite repeating many caveats or limiting assumptions in several sections of the document, I found the repetition useful and appropriate. Given the complex nature of the materials discussed, I feel the authors did a commendable job conveying the core details.

Dr. Matthews: The document was reasonably clear and easy to follow. However, I found the support documents, particularly EPA 1999a (Data Collection for the Hazardous Waste Identification Rule. Section 1. Introduction and Overview Section 2: Spatial Layout, http://www.epa.gov/epaoswer/hazwaste/id/hwirwste/risk.htm), particularly helpful for understanding the model structure and data collection approach. I strongly recommend sending a copy of EPA1999a to reviewers that are not intimately involved with EPA's technical jargon ... if for no other reason than the list of acronyms. I also found the 6-point module summary (page 4 in Peer Review Charges) to be very helpful. I suggest adding that summary to the end of Section 1.0.

Dr Sample: No, the document does not present information in a clear and concise format. Too much information is glossed over or presented (but not necessarily referenced) in supporting documents on the WWW. Because this model is intended to be applied for ecological risk assessments, information should be presented following the risk assessment framework. Although Section 1.1 provides a good (albeit brief) summary of the purpose of the modeling effort, the writers jump immediately into the model description in Section 1.2. A detailed problem formulation section (or equivalent) is needed so that reviewers can understand what is being assessed and why – what are the assessment endpoints? What are the measurement endpoints? What are the contaminants of concern and how were they selected? What is the conceptual model? How are wastes in sources in the AOI expected to be transported? Where do all of the source data come from? Some, but not all, of these questions are answered (in varying degrees of detail) later in the report. Addition of a problem formulation section would greatly enhance the readability and comprehension of the report.

In addition to adding a problem formulation section, much of the supporting material that is currently presented in documents on the WWW needs to be referenced in the appropriate sections of the document (at a minimum) or included in the document.

2. *Does Section 1 provide an adequate description of the purpose and context of the exposure and risk modules? If not, please explain.*

Dr. Fairbrother: Overall, Section 1 provides an adequate description of the purpose and context of the exposure and risk modules. See below for specific comments on the details of the material.

Dr. Kapustka: Given the apparent directives of the work group, Section 1 does provide adequate and appropriate description of the overall project. As I explain under several of the Specific Issues, I do not believe it is reasonable to expect as much of these models as was done. The state-of-the-science does not permit accurate estimates of many of the details expected from the modules, but that is quite different from the purpose or context addressed in Section 1. The authors descriptions are clear and appropriate.

Dr. Matthews: The purpose and context was sufficiently clear, but I would add the 6-point module summary described above.

Dr Sample: No, see previous response.

3. *As with any risk assessment, there are always additional data and method development efforts that could be undertaken to reduce the level of uncertainty. Are you aware of any major methodological or data gaps in the exposure and risk modules that have not been identified? If so, how should they be addressed?*

Dr. Fairbrother: There are some methodological issues that need to be addressed. These are covered in my detailed comments below.

Dr. Kapustka: The fundamental question that should be asked here is whether it is feasible technically to develop realistic exposure estimates or project risk for so many potential receptors. I contend that the models currently available cannot do so.

The approach adopted for this project is commendable, nevertheless. The attempt to consider habitat, albeit at a relatively gross level, represents a major advance in how ecological risk assessments can and should be conducted. For too long, ecological risk assessments have had precious little ecology incorporated in the process. The relatively simplistic effort of delineating major breaks in landscape features represents a positive step forward. One can argue the merits of more refined descriptions of habitat, but for a screening-level effort, this program presents a defensible approach.

Exposure estimates are at the mercy of the source modules, which are not discussed in any detail in this report. How accurate and how precise are the models describing chemical fate and transport? What empirical calibration has been done to support the models? How well do they perform across the breadth of environmental conditions captured in the 201 sites used for this study? At the minimum, there needs to be estimates of error and uncertainty associated with the fate and transport models. And that needs to occur across all media! I suspect the models may be adequate for simple aquatic systems, unsatisfactory for sediments or soils, and horrible as predictors of tissue concentrations in plants or soil invertebrates. In particular, I am very suspect of any model that purports to estimate plant uptake of chemicals from soil. Virtually all of the assumptions built into the plant uptake models are violated in field conditions. None of the models comes close to allocating chemicals into different tissues (i.e., leaves versus stems or fruits or seeds). None of the models considers the dominant role of mycorrhizae typical of plants in the field. Seldom do the models factor in metabolism of organic compounds by plants, let alone that of mycorrhizae or associative bacteria. Because mycorrhizae can either facilitate uptake or block uptake of chemicals, there is little chance for plant uptake models to get it right.

The sources described in the report rely heavily on theoretical relationships of bioaccumulation and physical parameters. Empirical data for earthworms and PAHs (see Marquenie *et al*., 1987 as cited in Beyer, 1990)[1] reveal values for PAHs ranged from 0.051 for anthracene to 0.419 for indeno[1,2,3-cd]pyrene; yet the theoretical values generated from as K_{ow} and K_{oc} reach as high as 3,000 (see appendix in draft guidance documents of the US EPA Ecological Soil Screening Levels http://www.epa.gov/superfund/programs/risk/tooleco.htm). This clearly attests to the fact that factors affecting bioavailability in soils (e.g., organic carbon, clay, pH) are not accounted for by the simple assumptions. Consequently, levels in organisms may easily be overestimated by three or four orders of magnitude.

Finally, the overall approach taken here exhibits a naïve understanding of the underlying ecotoxicology data sets. The US EPA Eco-SSL effort has focused on identifying and evaluating toxicological data for plants, invertebrates, birds, mammals, and herpetofauna. Literature on 24 priority chemicals of interest was acquired and evaluated in an exhaustive search for all relevant papers (see website above for details). As it stands today, it appears that none of the 24 chemicals will have sufficient data in each group (i.e., plants, invertebrates, birds, and mammals) to set Eco-SSLs. At least 10 of the chemicals of interest will have data gaps for birds; and there may be data gaps for as many as 17 of the 24 chemicals for birds. Similarly, there may be gaps for mammals for as many as 16 chemicals. Some of us have noted, that this is the identical literature available for ecological risk assessment; and that surely this should be sobering as there is fundamentally insufficient data to generate Toxicity Reference Values (TRVs) directly from these data – only by making fairly untenable assumptions can TRVs be posited. As TRVs are lacking at the level of mammals or birds for even these most prominent chemical constituents of Superfund sites, how can one reasonably generate TRVs for individual receptor species within these groups? In practice what occurs is that a handful of studies involving a very restricted suite of test species are used to generate TRVs across a broad spectrum of potential receptor species of interest. Consequently, the exercise is merely one of

computational convenience, usually involving allometric adjustments; with little or no validation of the resulting numbers.

Taken together, these three shortcomings of risk assessment (namely poor plant uptake models, inadequate accounting of bioavailability, and limited ecotoxicity data) generate great uncertainty even for purposes of estimating individual-level effects. Extrapolation of effects from individuals to populations or communities introduces even greater uncertainty. With the compounding of uncharacterized uncertainty, one can fairly ask whether the approach taken in this project is meaningful. I hold firmly that the only way one can know whether or not there has been over-regulation is to go directly to the sites and take direct measures of the ecological resources to determine their status.

So, in concluding my remarks on this charge question, I am convinced that the primary policy directive addressed by this program (i.e., issues of over-regulation) cannot be addressed through the approach taken. Nevertheless, this project has developed a framework of incorporating habitat characteristics that represents a substantial improvement of how ecological risk assessment ought to be done, especially at the screening level. Therefore, I consider this effort quite successful.

[1] Marquenie, J. M., J. W. Simmers, and S. H. Kay. 1987. Preliminary assessment of bioaccumulation of metals and organic contaminants at the Times Beach Confined Disposal Site, Buffalo, NY. Misc. Paper EL-87-6, U.S. Army Corps of Engineers, Waterways Experiment Station, Vicksburg, MI. (as cited in Beyer 1990).
Beyer, W. N. 1990. Evaluating Soil Contamination. Biological Report 90(2). US Fish and Wildlife Service, Department of Interior, Washington, DC 20240.

Dr. Matthews: This model presents a logical approach to a difficult problem ... how to identify sites that have small amounts of hazardous wastes but nevertheless represent a low environmental and human health risk, without extensive on-site testing of air, water, soils, vegetation, and biota. As with any model of this complexity, compromises have been made. Environmental interactions have been simplified; major portions of the environment (e.g., microbiota) have been omitted from the model. Data were obtained from existing sources, which varied in age, spatial coverage, and accuracy. In addition, many of the model's data needs, despite the environmental simplification, are not available (see Tables F-1 through F-10). These types of problems are inherent in ecological modeling, and certainly to be expected for the 3MRA model, which is designed to work at the national scale. However, this model is not intended to be a hypothetical approximation of reality; the model is designed to support exemption decisions at hazardous waste sites. Therefore, I ask my most important question: how will the performance of this model be validated? I saw no plans to test the model against reality. One obvious test would be to use the EcoEx model to estimate exposures at sites where ?real" exposure data were collected (e.g., Savannah River, Hanford, Oak Ridge). Another possibility would be to compare EcoEx and EcoRisk output to historic case studies at contamination sites where we now know that a high or moderate risk was present. I think it would be quite interesting to see whether the EcoRisk model would have predicted some of the environmental contamination ?surprises" that have occurred at waste sites across the U.S. over the past few decades.

My second major concern with the model is that it doesn't address microbial transformations, and, in fact, mostly ignores the microbial role in the ecosystem. The microbial community (bacteria, algae, protozoa, etc.) plays a major role in contaminant mobilization and transformation. I don't think there is much you can do about this omission. The toxicity data become increasingly sparse as you move down the food

chain, and unless EPA mounted a major research effort to characterize microbial responses to environmental contaminants, there seems to be no realistic way to model their effect.

Dr Sample: I have included references to updated methods and data below in response to other comments.

Specific Issues

1. *EPA determined that the smallest meaningful unit relevant to ecological risk within a national analysis should be defined in terms of representative habitats. The representative habitats serve as the basis for ecological receptor assignments, and provide the spatial boundaries used to assess exposure and risk. These representative habitats reflect a broad range of ecological settings, although all of the habitats share common characteristics. For example, EPA assumed that the representative habitats are capable of sustaining a variety of wildlife. Further, each habitat is assumed to be of sufficient quality to support multiple trophic levels (with at least one reproducing pair of upper trophic level predators), and predator-prey interactions for each habitat are represented by a simple food web. Do you agree with the criteria used to define representative habitats to address ecological risks? Would you recommend improvements or additions to the suite of representative habitats given the national context of this analysis?*

Dr. Fairbrother: Habitats – in general, I agree with the use of the habitat approach. It is an excellent way to make use of existing data that puts an ecological context into the risk assessment. However, I was disappointed in that the concept is not followed through very well. First, I cannot discern which of the habitats is relevant for areas in the arid west: grassland or shrub/scrub? Neither of these seemed to fit well. I recommend the addition of another habitat type: arid lands. These definitely have well-characterized plant species and their own associated fauna. Second, I strongly disagree that coniferous and deciduous forests be lumped together as "forest." The only commonality between these two ecotypes is that they both have large, single-stemmed woody species as the dominant plant type. Otherwise, they are very different in terms of soils, understory plants, and associated fauna. I am surprised that the fate and transport modules do not distinguish between these two, but as that is outside my field of expertise, I will not comment further. From an ecological perspective, however, these two need to be treated separately. In fact, it is interesting that "forest" and "woodland" are differentiated, but not coniferous and deciduous forests.

Dr. Kapustka: Absolutely, it is important to consider landscape features as a means of identifying reasonable assessment species. However, this is of greater importance for more in-depth site-specific risk assessments. For the screening-level effort detailed in this project, one should consider limitations of the source data necessary to complete a risk assessment. Are toxicity data available for different receptors so that it is truly meaningful to consider the various receptor species separately? At this stage of development of the discipline, even this cursory level of habitat characterization is probably more refined than the input data warrants. The real value of the approach developed in this project is that site-specific refinements can follow for more detailed ecological risk assessments. Specific measures of habitat suitability for different species could be incorporated into the effort. And, site-specific measures of the status of receptor species could be undertaken. This would be especially valuable if site-specific measurements of body burden of various diets were made.

Dr. Matthews: True, there is a national context for the analysis. The 3MRA model, out of necessity, must use simplified habitat units. But this is not a valuable **?**feature" but rather a necessary fault. By choosing to use broadly simplified habitat units, you lose much of the accuracy you would have if you build a site-specific model.

Using the aquatic river/streams group as an example, the model combined stream orders 1--5 together into a single habitat unit. Table 3-2 shows the receptor species that would be included in this habitat unit. For

streams in my part of the country (NW), most of the receptors are birds and medium- to large-sized mammals that are rarely found in the riparian zone of headwater (1-2 order) streams. Headwater streams make up a disproportionately large portion of a watershed's drainage network, so the model's habitat estimate for most river/streams receptors would be much larger than reality.

Given the **?**national context" of this analysis, however, I doubt that creating more habitat units would result in a more accurate model.

Dr Sample: In general, the habitat-based approach is suitable. Most categories are logical and appropriate. Additional clarification of the wetland categories should be provided, for example are there 3 or 6 wetland categories? Only 3 categories are presented in Figure 1-3 and in Section 2.2.2, but 6 categories are considered in Sections 2.3.3 and 3.

2. *Once the representative habitats were defined, EPA used a variety of GIS coverages (e.g., Anderson land use codes; National Wetlands Inventory) to delineate habitats at each site in the sample population. We developed a GIS-based tool that employed a weight-of-evidence approach in manually delineating the representative habitats. Using this tool, approximately three to five habitats were delineated at each site. Have we used appropriate coverages in delineating habitats? Are there other coverages or methods that would provide significant improvements over the methodology developed to support 3MRA? Is the delineation of habitats using GIS an appropriate method to define the spatial boundaries and relationships among ecological receptors, given the goals for this national assessment?*

Dr. Fairbrother: The use of GIS for development of data layers and delineation of habitat types is an **excellent** approach. ArcView® is a versatile program that allows a significant amount of data visualization and some quantitative use of information. ArcView® files are easily imported to ArcInfo® should additional spatially-explicit data manipulation be needed in the future. I am impressed with the manner in which the various data layers were used to delineate habitats, showing that they can take several different shapes, have holes of non-habitat in the middle, etc. The primary issue I have with the approach, is calling these "habitats," since in its correct use, the term is applied to a species-specific delineation of biological and physical features. Nevertheless, the application of the term to specific types of plant associations is commonly done, so this document does not deviate from common practice. See my detailed comments for some additional issues related to the development and description of the habitats.

Dr. Kapustka: The level of delineation used was appropriate. It would be more correct technically to refer to the types as landscape features rather than habitats. That is because a forest is not per se a habitat; only after one selects a species of interest, does it make sense to talk about habitat for that species. One clear example of this was underscored in the report (page 2-4), as there was no distinction between coniferous and deciduous forests; even though there are profound differences regarding the type of wildlife species these gross forests types would favor.

At one point, I believe the report indicated that land use types (e.g., roads, etc.) were not incorporated in the characterization (page 2-9). If that is correct, then I consider that a serious error. Indeed Habitat Suitability Index (HSI) models for some species use distance from roads or distance from human settlement as primary input parameters. The GIS delineations should provide for such parameterization of HSIs, in the event that higher-tiered risk assessments are undertaken. Though, I wonder about the scale of

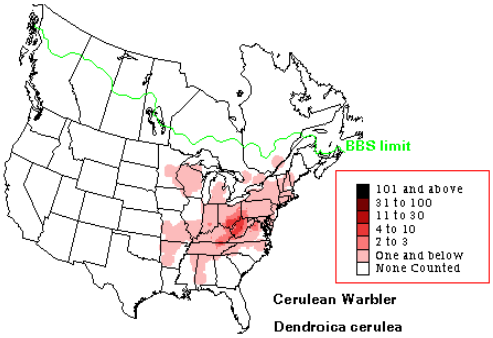
resolution (page 2-13; 16 ha) being too gross for many ecological receptors. Can the level of resolution be improved without overburdening the system?

Dr. Matthews: Using GIS to delineate habitats seems to be an appropriate method for combining a variety of data sources. I am concerned, however, by the different ages of the data sets. The terrestrial data from GIRAS are 15-25 years old (Section 2.0, page 2-9), while other data (e.g., census) are quite recent. The age of the GIRAS data is particularly troublesome because it will be used to define land use patterns, which have changed markedly in the past two decades in many parts of the US. This is another example of the disadvantages of searching for data that fit into the **?**national" framework rather than doing a site-specific version of the model. Site-specific data would likely be more recent and more accurate. Out of curiosity, why it is so important to select model parameters that fit a national scale rather than developing local, site-specific mini-models? The public concerns about these sites are usually local rather than national concerns. How will you build trust in this model at the local level? (Speaking from my own local perspective, I don't think the model will accurately represent NW habitat responses.)

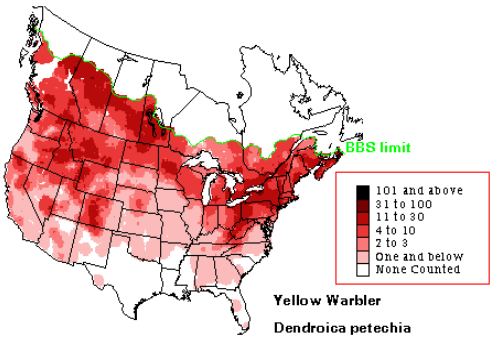
Dr Sample: I have insufficient topic area knowledge to comment on this question.

3. *As described in U.S. EPA, 2000, receptor assignments for each representative habitat were based on extensive research on food web structure (e.g., guild theory and feeding strategies), representation of species based on geographic region, appropriate resolution given the fate and transport models in the system, and the availability of data relevant to exposure for potential species of interest. In addition, Bailey s ecoregions were used to determine the appropriateness of a receptor assignment for any given site location in the contiguous United States. Consequently, the list of receptors assigned to a particular representative habitat (e.g., forest) could vary from location to location, although the master list for the forest would remain constant. Do you agree with our rationale for assigning receptors to representative habitats? Have we adequately represented a full range of feeding strategies and trophic elements across the simple food webs constructed for each habitat? Are there additional sources of information that you would recommend to provide meaningful improvements to the risk assessment results for national application?*

Dr. Fairbrother: Receptors – I agree in principle with the approach of assigning receptors to specific habitats, using Bailey's ecoregions to delineate who should be found where. However, I was disappointed in the list of potential wildlife receptors from which the program could choose. Not all feeding guilds are represented in all ecoregions or for all habitat types. For example, an insectivorous bird is represented by the cerulean warbler (*Denroica cerulea*). This species is found primarily in the Appalachian region and east coast (see map), yet appears to be selected for any of the ecoregions where "forest" habitat is available.



If a single representative warbler species is desired, I suggest using the yellow warbler (*D. petechia*), which is present everywhere except in Texas and Louisiana. Texas doesn't support any warblers to speak of except along the Louisiana border, while only a few species such as the pine warbler (*Dencroica pinus*), the prairie wrarbler (*D. disc*olor) and the northern parula (*Parula americana*) live and breed in Louisiana.



The immediate concern is whether there exists sufficient information about exposure parameters for these other species. The information for the cerulean warbler was complied by the EPA in the *Exposure Factors Handbook*, and therefore readily available. However, I do not buy the argument that information is not similarly available for other species. All warblers are insectivores, so food habits are easy to apply to any species. Body size may be available in the literature or in field guides, but is not too different across species, either. I believe that the reliance on a relatively short list of species that supposedly predominate in all ecoregions is a major weakness of this work. A lot of effort went into detailing habitats by ecoregion and by location; it is appropriate to devote a similar amount of effort to developing the required information for appropriate receptor species. This will entail some work in the primary literature, but there is a surprising amount of information available. Widely distributed animals such as the yellow warbler can be selected preferentially to reduce the total amount of work required. Species selection for birds can be done using the USGS Breeding Bird Survey maps (from which the above maps were taken: http://www.mbr-pwrc.usgs.gov/bbs). Mammal distributions are not as readily available, but there are fewer species to worry about so it is not an insurmountable task. The list of species with their corresponding habitats should be expanded to include a column for ecoregion, and the routine would then first select the appropriate ecoregion, then select the habitat type, and then select one species for each feeding guild.

Dr. Kapustka: This is an appropriate approach. However, as I have underscored above, for screening-level efforts such as this one, this approach provides more refinement than the input parameters from fate and transport models, biological uptake assumptions, or primary ecotoxicity data can support. I am quite

certain that if one properly accounted for uncertainty of the different input parameters, they would mask distinctions among the different species considered for different sites. I strongly suspect that a sensitivity analysis would reveal that the main driver of the model is the bioaccumulation factor; followed by the choice of toxicity benchmark; and thirdly the fate and transport model output. At the gross levels of the food chain (or food web in some limited capacity) relationships defined here, if the BAF used is grossly off, it matters little whether or not one adds refinements to the conceptual food web. For screening-level purposes, the approach used is fine.

Dr. Matthews: This is a difficult question. Given the goals of national applicability and a simple food chain, the receptor species at least partially cover the desired range of feeding strategies and trophic elements.

Although vegetation is included as a diet item in the model, plants are not included as receptors, despite the fact that plants can respond to contaminants very differently than animals. Can we really assume that a damaged wetland in the vicinity of a hazardous waste site provides that same habitat as a healthy wetland, or that the only ecological impact we want to model is an effect on animal populations?

What about major habitat shifts that result due to contaminant effects?

The choice of receptors is clearly driven by finding species with ubiquitous ranges. As I mentioned in previous comments, the trade-off between using site-specific vs. national information is a problem. Ideally, in headwater streams, you should use predatory invertebrates (e.g., stoneflies) and resident fishes as receptors at the top of the food chain.

I find that the choice of sites (Figure 1-2) and receptors (Tables 3-1 to 3-3) favor Eastern US habitats. No specific rational for this disproportionate allocation is given, but I assume it reflects the larger number of waste sites located in the East. Regardless, the resulting model will carry a regional bias despite its purported national application. If so, why not accept the necessity of regional receptors?

Dr Sample: Although I think that an appropriate range of feeding strategies and trophic elements have been included and considered, I question the validity of some of the functional categories and habitats assigned to some species. For example, alligator snapping turtle, American woodcock, belted kingfisher, bullfrog, burrowing owls, great blue heron, green heron, green frog, painted turtle, short-tailed shrews, and tree swallows are all listed as omnivores (Tables 3-1, 3-2, and 3-3). To be an omnivore, these species should be consuming significant amounts of both plant and animal foods. The diets of all of these species are virtually exclusively animal material: alligator snapping turtle, belted kingfisher, burrowing owls, great blue heron, green heron, and painted turtle are carnivore (primarily feeding on vertebrates), whereas American woodcock, bullfrog, green frog, short-tailed shrews, and tree swallows can be considered to be insectivores. Dietary breakdowns as provided are not logical.

When habitats are considered, many species are not listed as occurring in habitats in which they should be or are incorrectly listed as occurring in habitats in which they should not. For example, kestrels should occur in shrub/scrub but not forest; Cooper's hawk should not occur in cropland and residential; coyote and deer mouse should occur in residential; the Great Basin pocket mouse should occur in cropland; least and long-tailed easels should occur in shrub/scrub; the little brown bat, meadow vole, short-tailed shrew, red fox, red-tailed hawk, and white-tailed deer should occur in all terrestrial habitats; pine voles should occur in shrub/scrub and cropland habitats; American woodcock should not occur in rivers; box turtles and

white-tailed deer should not occur in any aquatic habitat; little brown bats, river otter, and snapping turtles should occur in all aquatic habitats; beaver, great blue heron, green heron, mallard, mink, river otter, and spotted sandpiper should occur in all wetland habitats; box turtle and mule deer should not occur in any wetland habitats; meadow voles and short-tailed shrews should occur in all intermittently flooded wetlands; short-tailed shrews should not occur in any permanently flooded wetlands; and tree swallows should occur in permanent or intermittently flooded shrub/scrub.

For birds, the Birds of North America species accounts should be consulted as another source of life history data.

4. *As described in the report, the 3MRA system requires a single site layout file for each site in which the coordinates for all areas requiring spatial averaging are identified. A single random placement of four home range bins was implemented for each site, and used to calculate the spatial averages for soil, plant, and prey concentrations in each home range bin. If the home range for a given receptor was larger than the habitat (i.e., extends beyond the area of interest), the exposure was averaged across the habitat and then prorated based on area (truncated to be not less than 10%). Is the random placement of home ranges and prorating a reasonable approach for estimating applied doses? If not, what other alternatives would you recommend?*

Dr. Fairbrother: The application of home range bins and their placement randomly within the habitat is appropriate, especially since spatial averaging across the habitat for exposure concentrations is being performed. However, I would like to see a sensitivity analysis done to determine if the use of 4 bin sizes is sufficient. It was pointed out that risk will be underestimated for those animals that are on the small end of the home range bin, but it is not immediately obvious what the magnitude of this underestimation will be. Hopefully, it will be less than 10-fold, so the risk estimate remains in its "correct" bin. If it is greater than this, then I suggest making the bins smaller and more numerous.

Dr. Kapustka: For a first cut, the approach described here is fine. This is a developing research topic that is likely to change relatively quickly. My group is doing work to incorporate habitat quality parameters as a modifier of the simplistic Area Use Factor approach that has been used to date. We anticipate being able to demonstrate the utility of using habitat quality as a meaningful modifier within the next year or two.

At the recent SETAC meetings in Nashville, there was a poster by Dr. Bruce Hope (bkhope@hotmail.com) entitled "Spatially-explicit exposure estimation for ecological risk assessments." He showed dramatic influences on predicted exposure through consideration of habitat quality. His recent publication (Hope, 2000) in *Risk Analysis* 20: 573-589 is worth consideration for this 3MRA project.

Dr. Matthews: Random placement of home ranges and spatial averaging of the dose is a reasonable place to start the modeling effort, but it is hardly realistic. Contaminant distributions in the natural environment are usually patchy, and animal distributions are rarely random. Since you are trying to decide whether to exempt a **?**safe" site, what you might want to model is the exposure to receptors in a minimum home range located in a region containing the highest contaminant levels of the site. Once again, model validation will be essential to the final decision process.

Dr Sample: I partially agree with the approach taken for estimation of exposure and risk. I agree that exposure should only be based on contaminant concentrations in habitats that are likely to be used by a

receptor and that total exposure should be adjusted to take into account a species home range size relative to the area of contaminated suitable habitat. I do not think the approach employed in the 3MRA is the appropriate way to perform this assessment. Random placement of single home ranges of predetermined size within the area of interest (AOI) does not adequately represent all exposures that may occur within the AOI, especially if area of suitable habitat>home range. In addition, species-specificity is lost by lumping all home ranges into four size categories. The approach I would recommend is as follows:

1 – for each receptor species, determine which habitats that occur within each AOI are suitable and therefore likely to be used.

2 – determine the spatial coverage across all habitat types suitable for the receptor.

3 – estimate the contaminant concentration present within the total area of suitable habitat for that receptor – this is the exposure source term for the receptor.

4 – divide the spatial coverage of suitable habitat (from Step 2) by the home range for each species (i.e., area use factor or in the terms of the 3MRA - $HomeRange_{frac}$). If $HomeRange_{frac}>1$ then home range size for the receptor of interest will fall within available habitat and no spatial adjustments to exposure are necessary. If $HomeRange_{frac}<1$ then home range size for the receptor of interest is greater than available habitat and the final exposure estimate must be adjusted by $HomeRange_{frac}$.

5. *The exposure profiles generated with the EcoEx module are based on the average annual concentrations in food items and media. Consequently, we recognize that one limitation of this analysis is that concentration spikes due to episodic events (e.g., rain storms) or elevated source releases following waste additions or control failures are not evaluated. In addition, to develop a methodology to support the risk assessment of low-level, long-term chemical constituent releases, we did not design the system to address acute exposures that might occur during sensitive life stages. Do you believe that this is a serious limitation, that is, that the inclusion of episodic events or failure scenarios would substantially change the estimates of ecological exemption levels? If so, what recommendations would you make to use the 3MRA system, available toxicological data, and ecological modules to allow for the evaluation of episodic events?*

Dr. Fairbrother: Exposure profiles – I am not overly concerned about averaging exposure concentrations over time and therefore missing spikes or acute effects. Most organisms and ecological systems can deal with short term pulse effects; it is repeated or chronic doses that cause the greatest impact. However, I would like to see the method account for exposure periods of less than a year, for example those animals that use the area only for breeding/summer habitat and then migrate elsewhere for winter, or those that hibernate or aestivate for significant portions of the year. This could be done easily by pro-rating the annual average by the number of months spent on-site. Otherwise, risk is significantly over-estimated. Furthermore, if a deterministic approach is used, the generally accepted approach is to use the 95^{th} upper confidence level (UCL) of the mean, rather than the average value for exposure concentrations. This adds a degree of conservatism to the screening assessment. However, it is not clear to me why such a simplistic approach is used. It would be preferable to use a probabilistic methodology and sample from the entire distribution of potential concentrations within the habitat. Again, a simplifying assumption would be that the entire habitat is used equally by the receptor (i.e., it does not favor a particular area nor does the

contaminant cause avoidance). This will, of course, generate a large amount of additional data that need to be stored during the model operation, but it makes use of more of the available data.

Dr. Kapustka: Ecological effects occur across a spectrum of conditions. It is highly likely that low-level stress that may occur over the majority of time has less importance than episodic events. Pulses of toxicants can have lasting and large effects on population dynamics. One well documented example is that of the Clark Fork River in Montana, where metals levels generally are below toxic threshold levels; yet periodic releases associated with snow melt or other weather events send pulses of contaminants into the system resulting in fish kills. Repeated events over several years have resulted in depressed populations of fish. A running average would almost never indicate a problem in such systems.
Other practical examples come from agricultural practices. Consider the application of herbicides or insecticides, especially modern products with short half-lives. The average annual concentration would not provide efficacious control of the pests. Yet, episodic applications can effectively eliminate selected weed species or insect pests.

Dr. Matthews: Given the uncertainty that already is in the model, I don't think the absence of episodic events is a serious model limitation. The uncertainty that already surrounds the relationship between the model and reality is broad enough to include the added uncertainty of episodic events.

Dr Sample: To be most useful, the 3MRA model should be able to address both acute and chronic effects. Because (based on the available information and documentation) it is not clear to me how the annual average concentrations are derived, I cannot make any specific recommendations how to resolve this question. One solution could be to solve the exposure estimates probabilistically. This could be done for soil, sediment, and water concentrations, in addition to the wildlife exposure estimates. Comparison of the resulting concentration or exposure distributions to acute and chronic effect levels would provide an indication of the frequency with which effects are likely. [Note: Interpretation of these data will be highly dependent on the nature of the distributions that are generated. If they are representing spatial variability and not temporal variability, interpretations of the results would differ.] If risks are identified, the exposure models may be evaluated to determine which set of conditions resulted in the particular exceedances.

6. *The data sources for ecological exposure factors are fully described in U.S. EPA, 2000, and rely heavily on EPA s Wildlife Exposure Factors Handbook (U.S. EPA, 1993). We believe that we have identified the most important sources of information; however, we recognize that additional data may have become available since this methodology was developed. Do you have recommendations for newly developed data sources that may improve the quality of the exposure factors database or allow us to include new receptors to improve the regional resolution of the receptor lists? Have the exposure data been derived appropriately for use in a national analysis?*

Dr. Fairbrother: Wildlife exposure factors – see comment #3 for receptors. The exposure factors that have been used are appropriate but an insufficient sample of receptor species has been chosen.

Dr. Kapustka: The source information described here represents the best available today. It is my understanding that EPA may be considering a revision to capture more data on more species. Until that occurs, this information used here is appropriate

Dr. Matthews: For a national analysis, I don't know of any data sets that would be more comprehensive. Regional data sets abound, and would improve the ?regional resolution" of the receptor lists, but are not in keeping with EPA's stated goals.

Dr Sample: Ecological exposure factors are not fully described in EPA (2000) as suggested. General descriptions of how they were developed are provided and summary tables of final parameter values are presented in Appendices A through D. Much of this same information is also presented in Section 13 on the WWW. The information presented is inadequate to understand exactly how the values were derived. Data must be provided on the derivation of all parameters so that a reviewer can track the decision process and determine 1) if it was performed correctly, and 2) if they agree with the derivation process. As the 3MRA model currently exists, the derivation and selection of exposure parameters cannot be verified. Although references are cited as sources of information, the actual values extracted from these sources are not presented. For example, body weights (needed for estimation of food and water ingestion rates) selected are not presented. This is especially critical when the authors have combined values (from multiple studies or across sexes) to derive a mean value that is later incorporated into the model. Soil ingestion rates are also not presented. Because few soil ingestion data are available, values used and assumptions made must be summarized so that the validity of final exposure estimates may be evaluated.
As for newly developed data sources, the authors of the 3MRA model are directed to the EcoSSL guidance document and supporting literature (EPA 2000) for more recent models for estimating food ingestion (based on Nagy et al. 1999), Kow-based bioaccumulation models (corrections to the Travis and Arms 1988 models), and other sources of exposure parameters

7. *Because media concentrations are averaged annually, the EcoEx module constructs the dietary preferences for each receptor based on dietary data covering one or more seasons. Some of the seasonal variability in the diet is captured indirectly by the hierarchical algorithm used to determine the dietary preferences. However, the random preference algorithm is implemented on data across multiple seasons and, therefore, does not necessarily reflect seasonal differences. Although we believe that this algorithm is a positive step in addressing dietary variability and data deficiencies in prey preferences, it greatly simplifies predatory-prey dynamics. Are there refinements that you can suggest to the random sampling algorithm used to assign prey preferences?*

Dr. Fairbrother: Dietary preferences – it was not completely clear to me how the diet for each receptor is constructed. I understood the discussion about dietary preferences and randomly selecting a percentage from each food type. However, it was not intuitively obvious if this was done a single time, or if was repeated multiple times in a Monte Carlo type simulation to develop the range of dietary preferences. I urge that the latter approach be used, as the former (a one-time selection) can create some enormous biases just by random chance. I agree that it would be too difficult to develop seasonal-based scenarios (if this were done, the logical follow-on would be to adjust food intake rate by season, which is starting to get much too complicated and detailed for the needs of this assessment).

Dr. Kapustka: The approach described here is a good one. As I have argued on other points above, this dietary detail goes considerably beyond the quality of the input data. By relying on modeled uptake parameters, there is so much error already in the system that it really doesn't matter how the diet is allocated. The true value of this approach would be realized if one obtained site-specific empirical concentration data to be used as the input data. So again, I conclude that this 3MRA project has advanced

the field of ecological risk assessment, but it goes beyond the practical limitations of the current data used for screening-level assessments.

Dr. Matthews: Given the levels of uncertainty in the model, I don't think that fine-tuning the prey preference algorithms would actually add accuracy to the output ... just more complexity.

Dr Sample: This method for defining dietary composition is innovative and the authors should be commended for their effort. However, because this is both new and complex, additional effort is needed to ensure that the algorithm is clearly understandable. Additional detail of the algorithm, how it is implemented, and clearer examples of its application should be included. It is unclear whether the algorithm generates a distribution of dietary compositions or an unique point estimate of composition. If the result is a point estimate of composition, then this is not any better than using the point estimates obtained from literature.

Further, better information concerning the data input to the algorithm is needed. The quality of the diet composition data is not presented and therefore are unknown to reviewers. Additional questions about the diet composition data are raised by the approach to dealing with qualitative data described on Page 4-13. The approach described is unlikely to be repeatable and, as stated in the text, is subjective. Subjective diet composotion data should be excluded. Derivation of diet composition estimates should be restricted exclusively to quantitative data.

Finally, the utility of any additional clarity added to the dietary composition algorithm is limited by the availability of data concerning estimation of chemical concentrations in different food types. Due to the limited availability of bioaccumulation models for all possible food types considered in the 3MRA model, increased precision in the dietary composition algorithm is unlikely to produce a return in increased precision of exposure estimates. That is, uncertainty associated with chemical concentrations in food type are likely to be greater than any reduction in uncertainty that may be obtained by improved estimates of diet composition.

It may be better and simpler to present and explain results if the dietary preference algorithm is deleted and simply replaced with multiple exposure estimates for each species based on differing measurements of diet composition. Overall risks may be evaluated by comparing the range of exposure that is obtained based on differing diet compositions.

8. *The HQ methodology - the ratio of an exposure dose or concentration to a benchmark - is applied uniformly across all ecological receptors. Because the HQs are simple ratios rather than probabilities, they provide a relatively coarse metric to interpret the significance of ecological effects. Moreover, the HQ for a receptor population is not distinguished from an HQ for a receptor community (e.g., benthos) and, in fact, the former is based on a low adverse effects level whereas the latter is based on a statistical interpretation of effects to species within a community. These differences notwithstanding, the significance for these HQ values is implicitly assumed to be equal even though the effects may involve different levels of biological organization. Put another way, the HQ estimates for different receptor groups represent somewhat different risk metrics. Please comment on the value of calculating ecological HQs to represent risks to different levels of biological organization. Because the evaluation of ecosystem-level effects is beyond the scope of the site-based analysis, do you agree with the*

approach used to evaluate a suite of ecological receptors within the context of a habitat? Does this provide a meaningful representation of the potential ecological risks at a national level?

Dr. Fairbrother: Use of HQs developed from different endpoints is not a problem. The HQ provides information relative to the question being asked. So, in one instance it will provide an indication of whether the exposure concentration is above the level that is known to change species diversity of a benthic community, while in another case it will provide information about whether a particular wildlife species will experience reduced reproductive output. The important thing to remember is to carry the question through the entire risk assessment, so the risk manager understands what the HQs are telling him/her. If s/he wants to know if the exposure concentrations are sufficiently high to impair the growth rate of the population of chironomids in the area, insufficient information will be provided by the "benthic community" HQ. Likewise, the wildlife HQ will not provide information about population sustainability (see next comment). Therefore, it is important that the risk characterization reiterate what question each HQ is addressing. However, see my detailed comments below for Chapter 5, about the pitfalls associated with trying to arithmetically aggregate HQs, particularly those based on different endpoints.

Dr. Kapustka: Oh, for the day that EPA abandons its ties to HQs! As with its continued reliance on NOAECs and LOAECS, the use of HQs can only be explained in terms of institutional inertia. Peer reviews have repeatedly illuminated the limitations of HQs, yet they persist. At the very basis of the approach, the HQ is derived from a rather poorly defined point estimate for the numerator and equally poorly defined point estimate for the denominator. The result can only be a poorer quality quotient. One critical reason that the HQ is so deficient is that the point estimate of the toxicity endpoint fails to consider the slope of the response. Also, the quotient does not provide a measure of uncertainty. As values closer to the threshold response are used, the error increases. With very little adjustment, it HQs from <1 to >100 can be shown to be indistinguishable; well within the error of the input data. Moreover, given the issues of slope, there is no basis for saying that and HQ of 100 is different from an HQ of 10 in terms of ecological consequences. Both merely suggest that one might choose to examine the situation more closely.

Review panels have repeatedly argued that it quotients should not be summed. If one is fully committed to using quotients and finds the need to consider mixtures, one needs to sum the effective concentrations before calculating the quotient.

The concept presented here of summing HQs for different receptors has no technical foundation. It should not be used. It is meaningless.

Dr. Matthews: Without some sort of model validation, I have no idea whether the model in its present construct, or any other variation, can provide a meaningful representation of potential ecological risk. The complexity of this model doesn't even come close to matching the complexity of an ecological system. Models work best when they are not pressed to provide a picture of reality, but rather a simplification of reality. Unfortunately, what you are asking of the 3MRA model is to construct, from an admittedly oversimplified set of algorithms, an accurate prediction of reality. There needs to be a **?**reality check."

Don't misinterpret my comments ... I agree that the 3MRA is an appropriate first step in identifying exemptions sites, and I am impressed with the careful, thoughtful construction of the model. I just think that many of the questions in the peer-review charge have focused too much on refining the model, without providing information on the model's performance. It's like being asked to comment on the taste of a meal after looking at the menu.

Dr Sample: Although the use of HQs to quantify risk is consistent with existing guidance (i.e., EPA 1997; EPA 1998) and most current practice in ecological risk assessment, it may not be helpful for determining incremental risks, which is the purpose of the 3MRA model. Distributions of exposure and likelihoods for exceedance should be able to be generated from this model. For example, if (as described on Pages 4-2 and 4-3) the concentration inputs to the EcoEx module are mean concentrations of contaminants in various media, then it should be feasible to develop distributions for the estimated concentrations. For soil, sediment, and aquatic biota, generating distributions of risk is relatively simple – toxicity values are simply overlain on the soil, sediment, or water distributions. Risk is measured by the proportion of the distribution that exceeds the toxicity value. Although probabilistic methods are needed to generate exposure distributions for wildlife, interpretations are comparable.

It should be pointed out that correct risk interpretations require detailed knowledge of what both the exposure and effects distributions represent. Without knowing exactly how 3MRA will generate annual averages, distributions for chemical concentrations soil and sediment are likely to be dominated by spatial, not temporal, variation. Water, on the other hand, is likely to be more temporal than spatial (this will depend, however on how many and what type of source areas are present in an AOI). Wildlife exposure will also be a combination of both temporal and spatial variation. On the level of ecological organization, soil, sediment, and water distributions may represent population or community-level exposure, whereas wildlife exposure estimates are almost always at the individual level. Effects data for soil, sediment, and aquatic biota are mostly community level (the degree to which this is really true depends on the taxonomic diversity of data incorporated into the toxicity value – aquatic and sediment data are more likely to be community data than are soil data). Again for wildlife, all effects data are at the individual level. In application, although reproduction and mortality data are used to make assumptions about population effects, in reality individual-level effects are evaluated.

In the context of the 3MRA model, I would recommend expressing exposure and risk as distributions, moving away from the use of HQs. Further I think that it is essential to explicitly describe exactly what the exposure and risk estimates represent. If the user is actually performing individual-level exposure and effects evaluations and making assumptions about potential population-level effects, that should be made clear.

9. *Data on chemical stressors are seldom available above the level of an individual organism; that is, the study endpoints focus on individual organisms rather than processes crucial to assemblages of organisms. Although reproductive and developmental endpoints have been used by EPA as relevant to population sustainability, they are not always the critical effect for an individual organism. Consequently, we implicitly assume that, for mammals and birds, endpoints associated with the populations' ability to reproduce and survive are an appropriate surrogate for true population-level endpoints (e.g., adverse effects leading to a 10% reduction in the population size). Do you agree with the selection of these endpoints, and do you have recommendations for expanding the development of benchmarks to include other, relevant endpoints? Please comment on the approach used to infer effects at the population and community levels based on the selected endpoints for this analysis (e.g., endpoints relevant to reproductive fitness, growth, and survival). In particular, we would appreciate your perspective on whether evaluating risks through inference constitutes a reasonable approach for a national analysis.*

Dr. Fairbrother: Benchmark endpoint selection – I have argued strongly in the past, and continue to argue vociferously, that estimating potential risk to reproduction and growth does NOT allow us to make population-level predictions nor does it provide ANY information about community effects. There are too many other inputs to population demographics to be able to say that decreased reproduction will result in decreased population sizes. Animals might live longer, others might immigrate, or less emigration could occur in response to changes in reproductive output. There may be surplus individuals produced when the stressor is not present, so compensatory changes in predation and mortality will eradicate any potential population-level effect of reduced reproductive output. Some species can sustain large impacts on viability – for example, rodents typically have a large turn-over of individuals reaching as high as 80% per year in some vole populations. A little additional toxicant-induced mortality will have no significant impact on this type of species. The USDA has a stated goal of eradicating up to 5% of the population of pestiferous rodents (e.g., ground squirrels) *each year*, and has not yet come close to significantly reducing population sizes. Until we start to couple population models with the toxicologically-based risk outcomes (something which IS possible to do...) we can only make predictions about individuals and vague assumptions about populations. Extrapolating to community-level changes from information on reproduction, growth, and mortality is impossible. This suggests that we can predict that whole species will decrease in prevalence or cease to exist in the area, either through direct toxicological impact or indirect effects of increased predation, reduced competition, etc. Here, we need measures of species richness, biodiversity predictors, and species extinction models in order to make any kind of realistic predictions. Even measurement endpoints for plants are specific to individuals and cannot make predictions about shifts in community structure.

I suggest that this assessment state that the goal is to protect populations of animals, and that by managing *de minimus* risk to the individual, there is a high likelihood that the populations will remain viable. Do not try to take the data farther than it can go.

Dr. Kapustka: The confusion here comes from assuming that toxicity data translate directly to ecological effects. The use of growth endpoints or reproductive impairment endpoints is appropriate toxicological endpoints to consider. However, to take this to an ecological effect requires some consideration of population dynamics of the species of interest. For *r*-selection species, a toxicity response resulting in a 50% reduction of a population may have no detectable consequence even after just one generation. Conversely, a 5% reduction in a population of a *K*-selection species could have profound, long-term consequences. Unless one is prepared to generate sophisticated population dynamics models for many surrogate wildlife species, it is best to keep things relatively simple; use the risk estimates as warning signals deserving more detailed site-specific monitoring of target populations.

Dr. Matthews: Inferred effects might not be entirely realistic, but once again, it is probably the only feasible approach to modeling at the habitat level. The range of possible population and community endpoints is simply too large to measure. There are many published documents that demonstrate that reproductive fitness is a very sensitive endpoint ... one that is likely to be quite conservative. In my experience, the responses at the community and ecosystem level are usually less severe than at the population level, particularly when comparing the reproductive potential in a small, confined population to a larger, more natural community. That's the good news! Ecosystems and communities will often (not always) be less sensitive than their individual population. In my experience, the lower than expected response at the ecosystem level was apparently due to unexpected influences by microbial decomposers. Another reason why I hate to see microbiota left out of the risk assessment formula.

Dr Sample: This approach is acceptable. Unless population modeling is going to be added to 3MRA, there is no real solution available. The application of toxicity data employed in the 3MRA model is consistent with current usage in ecological risk assessment. To prevent confusion and reduce criticism, detailed discussions of the assumptions and uncertainties associated with the approach should be provided.

10. *In generating risk estimates for each habitat, it was assumed that one and only one population of each wildlife species is carried by a given habitat. For example, although there may be a number of receptors assigned to a habitat, multiple populations of shrews or robins are not evaluated. Each receptor population has the same spatial characteristics, as defined by the home range. Hence, there is one HQ calculated for each receptor population in each habitat. Does this approach seem reasonable given the level of resolution for this site-based analysis? If not, what recommendations could you offer to incorporate carrying capacity and mega-population dynamics in the risk framework?*

Dr. Fairbrother: Patch dynamics – given the size of the area of concern relative to the home range sizes of most of the species, I agree with the simplifying assumption of a single population within each habitat. Expanding the model to include patch dynamics would mean needing to estimate "hot spots," migration corridors, etc. The next step would be to construct a population model (see comment #8) to determine which areas would continue to be a "source" for emigration and which areas would be population "sinks." This goes well beyond what is needed for this type of screening level assessment.

Dr. Kapustka: There is already a major leap of faith that the toxicity data truly relates to population-level effects. Until methods are improved to get it basically right for a single species, it is beyond the capabilities of risk assessors to consider multiple-interacting sub-populations. Moreover, if all one is relying on is an HQ, it is not worth the effort of refining a population response that necessarily must use scaling responses proportionally using the slope of the response.

Dr. Matthews: Wayne Landis at Western Washington University has done modeling of the role of meta-populations in contaminant dispersal and exposure (e.g., Spromberg, Johns, and Landis, 1998, Metapopulation dynamics: indirect effects and multiple discrete outcomes in ecological risk assesment, Environ. Toxicol. Chem. 17:1640-1649). I believe that it would be fairly simple to add a meta-population component to the 3MRA model; however, given the level of uncertainty in the current model, I don't think adding meta-populations would add realism, just more uncertainty. The 3MRA model already restricts the population to a local site, so the larger issues (e.g., models that allow breeding between animals separated by vast distances) are not really a problem.

Dr Sample: Given the resolution of available data, this approach is reasonable. However, see Comment 4 for suggested changes in application of the exposure model.

11. *The HQ estimates for the aquatic and benthic communities, respectively, are resolved at the habitat level. Because the HQ estimate for the aquatic habitat reflects an average chemical concentration across all stream reaches in the habitat, it is possible that a highly contaminated reach would be diluted by other reaches in the habitat that are relatively pristine (e.g., upstream reaches). As a result, there is some uncertainty associated with calculating risks to aquatic life across an entire aquatic habitat defined within the area of interest. Species of fish such as brown trout tend to utilize certain segments of stream habitats; impacts at the segment*

level may be obscured by the average HQ for the habitat. Conversely, establishing artificial boundaries between stream reaches is contrary to the goals of the assessment strategy, namely, to evaluate ecological risks using the habitat as the fundamental unit. Do you agree with the appropriateness of this approach and, if not, are there alternative approaches that you could recommend?

Dr. Fairbrother: Aquatic habitat delineation – this is out of my area of expertise, so I will not address this question.

Dr. Kapustka: Forget the HQ as it is fundamentally not instructive of any differences across a site or among sites. However, if one used a dose- or concentration-response relationship, one could readily demonstrate that the "hot spots" rather than the site-wide mean values would be most meaningful. This issue is identical in structure to the temporal averaging method (Issue #5). If concentrations are high enough at the "hot spots," population-level effects can occur that would not be anticipated based on the site-wide average.

Dr. Matthews: First of all, although your goal is to evaluate risk at the habitat level, your choice of habitat units was somewhat arbitrary and artificial, so can't argue that establishing **?**artificial boundaries" would violate the goals. I do think that subdividing streams would create a finer level of habitat distinction that is present in the other habitat categories.

The problems you describe here are similar to issues I raised earlier. A first order stream is very, different from a fifth order stream/river. Combining them into one habitat class is a gross oversimplification of reality, but no more so than combining deciduous hardwood eastern forests with coastal evergreen forests, etc. I don't see the aquatic habitats as being any more or less oversimplified compared to the terrestrial habitats. It is absolutely true that the receptors such as brown trout will use only portions of the area defined as their habitat ... but the same comment can be made for all of the receptors and their habitats.

Dr Sample: If this were a site-specific assessment for which measured chemical concentration data were available for all areas of interest, the answer to the question would be yes – risks should be evaluated separately for different areas that have differing exposure profiles. However, because this assessment is based exclusively on modeled data for which uncertainties could be both high and significant, I think it would provide a false impression of precision if risks were evaluated specifically for individual reaches. Concerns about the effects of spatial (and temporal for that matter) averaging are legitimate. The simplest approach for dealing with this problem is to develop exposure distributions (in the case of the example, this would be the distribution of water concentrations within the habitat). If there is a 'hot-spot' but everything else is comparatively low, the distribution will be highly skewed. The risks from the hot-spot to the overall community in the habitat would be represented by that portion of the distribution that exceeded toxicity values.

12. *The risk characterization of ecological HQs includes a qualitative framework to assign confidence indicators to the constituent-specific data set on ecotoxicity and bioaccumulation potential. The framework considers the adequacy of the available data to assess ecological risks to receptors across the trophic continuum given the physical and chemical properties of that particular constituent. The framework also considers the quality of data available for each constituent (e.g., how well we met data quality objectives), particularly with respect to toxicity to different receptor groups such as mammals and soil biota. Please comment on the*

appropriateness and utility of this framework as well as the adequacy of the databases on ecotoxicity and bioaccumulation potential for the constituents of concern. Indicate, as appropriate, additional sources of data that you feel could improve the quality of our databases. Do you have suggestions on how the confidence indicators might be implemented within the 3MRA system? Are there any improvements to the indicators that you could recommend?

Dr. Fairbrother: The confidence indicators used in the 3MRA system are persistence and bioaccumulation. This is consistent with the Agency's PBT approach. However, it suffers from the same shortcomings when dealing with metals and other naturally occurring substances (hereinafter referred to collectively as "metals.") Metals are, by definition, persistent. Therefore, plants and animals have developed mechanisms to cope with their presence in the environment. In fact, some of the metals are essential micronutrients and have a minimum level of exposure that is required for good health. Of course, they can reach toxic levels as well, but the bioaccumulation of the materials from the environment to these levels is not a constant. That is, greater amounts are taken up by the organisms when exposure concentrations are low, and lesser amounts are taken up when the exposure concentrations are high. So, what "bioaccumulation factor" would be used in the ranking and confidence scheme? These issues have been discussed extensively with the Agency in regard to PBT issues, as well as in the European Union and OECD in relation to hazard classification of substances. See, for example, the following reports on the topic that I prepared in relation to terrestrial organisms:

> Fairbrother, A. and L.A. Kapustka. 1999. Proposed hazard classification system for metals and metal compounds in the terrestrial environment. International Council on Metals and the Environment, Ottawa, Canada.
>
> Fairbrother, A. and L.A. Kapustka. 1997. Hazard classification f metals in terrestrial systems – a discussion paper. International Council on Metals and the Environment, Ottawa, Canada.

Similar discussion papers are available from my colleagues at Parametrix, Inc. for aquatic systems. The point being made is that this simplistic approach to adding a "confidence indicator" to the risk outcome does not apply well to metals in general. More specificity is required to determine the persistence of the material in a *bioavailable* form. Therefore, I suggest that this approach not be done at this time, as it would require extensive work to make it meaningful.

Another approach to using "confidence indicators" would be to ask the question from an ecological perspective, rather than from a chemical perspective. How many of the possible number of trophic levels (or feeding guilds) were evaluated? How confident are you in your exposure parameters? How site-specific are the representative species that were used? How good is the toxicological data base? Some metric that combines semi-quantitative answers to these questions likely would be a good "confidence indicator" and not suffer the short-comings of the PBT approach

Dr. Kapustka: Okay, I'll say it one more time – dump reliance on the HQ and proceed to use the entire dose-response relationship. A lot of good work has gone into this program. What is needed to maximize its value is to do a credible job of documenting uncertainty. EPA guidance on uncertainty analysis should be incorporated into this effort. As a part of the exercise, a sensitivity analysis should be performed to demonstrate just what drives the model output. Once that is known, an objective effort could be undertaken to show which limitations of input data could be addressed profitably. In the end, the only way to answer the basic questions raised for this review, is to conduct field monitoring of a sizeable number of the

201sites used in this study. Does the model predict unacceptable conditions at sites? If so, does site data confirm the prediction in terms of failing populations? Absent good population data, do tissue concentrations of dietary items match the predictions used in the exposure models?

Dr. Matthews: There are huge gaps in the data sets needed to run this model. Most of these gaps are in the chemical benchmarks and CSLs data (Tables F-1 to F-10). Where data do exist, the values are usually based on a small number of tests, using a small number of animal species, and the results span decades in the reported literature. The confidence estimates around these data are very important, but I'm not sure how, or whether, to include them in the 3MRA model. One concern about including the confidence indicators in the model at this point is that once done, it implies a degree of accuracy about the model output that I don't think is warranted. The major improvement that I see, and indeed, the next essential step in model development, is some sort of model validation and analysis of the predictive accuracy of the model.

Dr Sample: Application of qualitative confidence criteria is both highly appropriate and useful to evaluate the strength of risk estimates and to help risk managers determine the amount of weight that should be placed on 3MRA results. The utility of the qualitative confidence criteria would be greatly increased if the data quality objectives described in the body of the question were actually included in the report or in an appendix. In this way, readers would be able to judge for themselves the available data, the data quality objectives (DQOs), and the qualitative confidence criteria to determine if they were all in line. Discussion of inadequacies of the bioaccumulation models applied in 3MRA have been discussed above. As for the ecotoxicity values (Section 14), a quick review of the plant, soil invertebrate, sediment, and aquatic benchmarks does not suggest any problems [QA should be performed to verify that a reference is present for every compound for which a benchmark is defined – divalent Hg – page 14-68]. I have some concerns about the wildlife toxicity values. First, toxicity data for some compounds consists of LOAELs but no NOAELs – in Sample et al. (1996), NOAELs were estimated by applying an uncertainty factor of 0.1. In reviewing Table 14A-1, I see that MATCs were calculated for several compounds (thallium, benzo(a)pyrene etc.) for which no actual NOAEL was available. I think that it is inappropriate to calculate MATCs based on estimated NOAELs. This could be alleviated by estimating risks using both NOAELs and LOAELs, one denotes possible risk, the other probable risk. Finally, I noticed that the study by Krasovskii et al. (1979) was used for the mammalian benchmark value for lead. I have grave misgivings about the use of this study. Not only are the results poorly documented, they are orders of magnitude lower than other toxicity values for lead acetate in rats. If the results of this study are to be believed, lead is more toxic to rats (MATC=0.016 mg/kg/d) than methyl mercury is to mink (MATC=0.099 mg/kg/d). It simply does not pass the reality test. I strongly recommend replacing this study with one of higher technical quality. A study I have been using recently is Ronis et al. (1998).

Miscellaneous Comments

Dr. Fairbrother:

Detailed Comments (by Section and Page)

Section 1

p. 8 – by this point in the document, I was wondering whether a deterministic or Monte Carlo type approach would be used. It eventually became clear that this is a deterministic model, but it would be very helpful to have this spelled out clearly in this section, along with the reasons why such an approach was chosen.

p. 10 – the discussion of time series is not clear. What is changing with time? Matrix (soil/water) concentrations? What is the unit of time? (later it becomes clear that the time step is years, but it needs to be spelled out here). How does the model account for time in the case of wildlife exposure, where the metric is in mg/kg-body wt/day. Is a daily average for a year's time calculated? If so, how? Using daily dose allows great flexibility to be built into the model, to account for animals that use the site only part of a year. That is, dose can be calculated for each day and averaged for the time period of interest.

p. 10 – this discussion summarizes the "binning" of HQs. I have severe reservations about this approach, and have discussed them fully below, in my comments for Section 5. Also, see my comments above about relating the assessment to population and community level endpoints.

Section 2

p. 1 – the spatial framework of the model is its greatest strength. This is an **excellent** approach to use.

p. 4 – the statement is made that organisms are selected "…based on the receptor species for which exposure data are available…" This is absolutely the wrong reason for selecting a particular species. The logic should be: define ecoregions; select species that are representative of each feeding guild within each ecoregion, with the caveat that species with wide distributions (i.e., found in multiple ecoregions) are given preference; find exposure data for the species selected or extrapolate from similar species for which such information is available.

p. 5 – more definition of the habitats are needed here. In fact, a map showing their locations within the country would be helpful. For example, what is the desert habitat in the southwest? Is it shrub/scrub or grassland? Or both, depending upon location? What is the difference between a forest and woodland (explained later, but needs to be described here). Why do these get separate designations but coniferous and deciduous forests do not? Receptor species differ significantly between coniferous and deciduous forests, just as they do between forests and woodlands.

p. 6 – information about [soil] moisture levels should be added to the habitat descriptors, since this is one of the primary delineators of the various types.

p. 8 – ponds are dismissed as less important than lakes. However, note that the ponds in the northern states ("prairie potholes") are extremely important for waterfowl reproduction and probably for amphibians as well.

Chapter 3

p. 2 – when "functional groups" are described, why are piscivores not included? Why are these called "functional groups" rather than "feeding guilds?" They don't serve any particular "function," such as decomposition or nitrogen fixation…

p. 2 – herpetofauna from the western regions appear to be under represented. See above discussion about selection of species within ecoregions.

Figure 3-1: note that flying invertebrates feed on foliage and are not part of the soil system. Also, the figure appears inconsistent, as it contains some specific names (e.g., white-tailed deer or least weasel), some animals specified by Order (shrew or rabbits) and others in larger groupings (snakes or ground birds). Consistency would be good… Furthermore, how can shrews belong to two boxes: herbivores and omnivores? Omnivores includes herbivory but a designation of an "herbivore" is inappropriate. Shouldn't shrews be classified as "insectivores" rather than omnivores? And how about including bats as another insectivorous species? Note that kestrels are really insectivores and that shrikes are omnivores (mammals and insects). Why is the kit fox (*Vulpes macrotis*) used an example carnivore? It's range is restricted to the arid west and it's very small, whereas the red fox (*V. vulpes*) is much more widely distributed and seems to be a better indicator species.

Table 3-1 – additional comments about species selection… Why are deer not included in cropland (they certainly like to browse in those areas!)? The red-winged blackbird *(Agelaius phoeniceus*) is another widely distributed species that could be included in the marshland habitats (they also occur in uplands, but the warbler will be a better representation of that feeding guild).

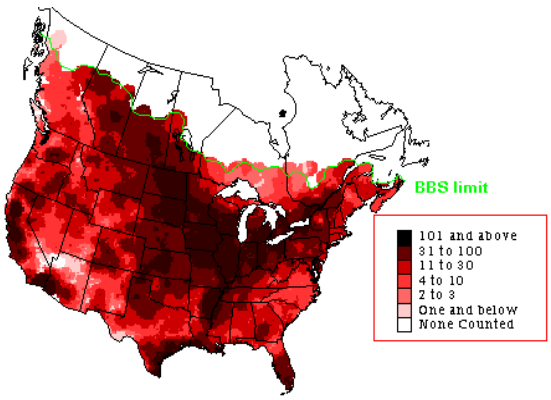
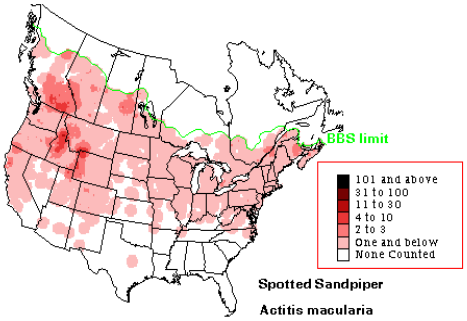
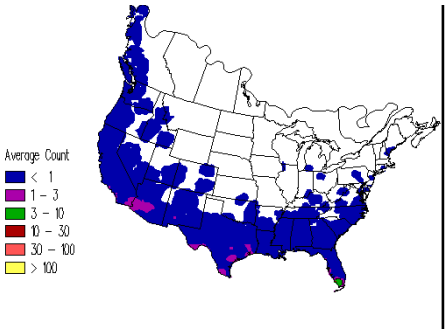
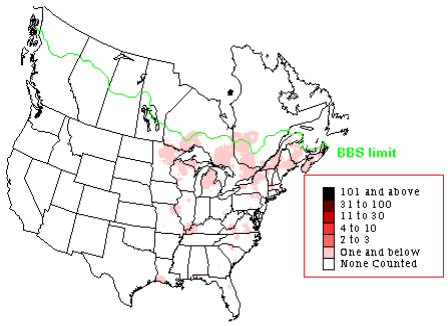


Figure 3-2 / Table 3-2 (aquatic habitat) – waterfowl are conspicuously missing from this system. They feed on aquatic plants primarily, and somewhat on aquatic invertebrates (during breeding season). Wading birds are identified on the figure only as a group, but appear to be represented by the spotted sandpiper and American woodcock. Therefore, they can be identified by species on the figure. It is not clear to me why the spotted sandpiper is considered a carnivore rather than an insectivore, however, nor why the woodcock is included since its distribution is more restricted than the sandpiper but its feeding habits are considered similar.

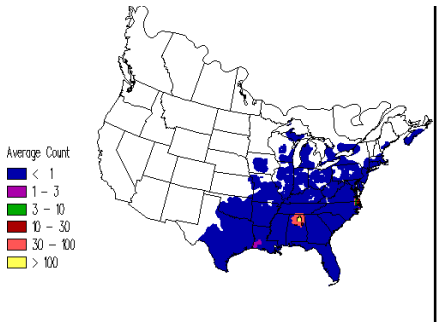




Spotted sandpiper
(winter)



American woodcock
(summer)



American woodcock
(winter)

The "deer mouse" (*Peromyscus maniculatus*) is replaced by the white-footed mouse in the east (*P. leucopus*) and by other subspecies in places like Florida. They are all functionally the same for the purposes of this assessment, so should be referred to collectively as "white footed mice."

Section 4

p. 1 – at the bottom, a statement is made that a cross-referencing database assigns species to correct habitats. This is great, and allows the database to be easily expanded to include more species. However, as mentioned in my general comments above, an additional field is needed to assign them to correct habitats *within each ecoregion*.

p. 3 – Time series management. This module seemingly could accommodate hibernation/ aestivation/migration issues. Currently, 1 year seems to be the shortest time frame considered. However, it is too gross a measure to accommodate the ecologically-relevant issues of time spent on-site. Another bullet in the list at the bottom of the page would take care of this: "prorate dose by proportion of the year spent (or active) on-site."

Table 4-2. It is not clear what is meant by "select prey concentrations from the min/max TerFW." Why is the average matrix (soil water) concentration used, but the min/max distribution of prey items used? Why not use a Monte Carlo sampling approach for both? Similarly, for body weight. Or, to be the most conservative, use the smallest body weight and the greatest food consumption rate. Do the concentrations in the prey items change with time in some manner similar to the change in the matrix concentrations? Is this calculated in the TerFW module?

p. 15 – the comment about lack of home range data needs more discussion. The source books (e.g., *Exposure Factors Handbook*) are getting somewhat dated. There may be information in the literature that could now be retrieved. Or, information from similar species could be extrapolated, with the acknowledgement that there is some uncertainty with the value. Recognizing the additional work with adjusting home range size for habitat type, I still think this would be of value as it likely is a significant contribution to the uncertainty associated with the risk estimate.

p. 16 – what is the "entire time period" over which risk is calculated? Several years, obviously, but how many?

p. 20 – where is the TerFW module explained? This contains information on uptake factors that are one of the most important parameters driving terrestrial risk assessments. As explained above, these must be used as equations, rather than single numbers, for metals and some organic substances. They can range over 3 orders of magnitude depending upon the study, how they were measured, etc. Therefore, it is very important that this receives good Peer Review.

p. 21 – A discussion of bioavailability is missing from this chapter. I assume that bioavailability and assimilation efficiency has been set at 100% for the purposes of this risk assessment. However, this needs to be acknowledged explicitly as a simplifying assumption. We know that for most metals, this value is significantly less than one (e.g., about 30% for lead, 15% for nickel), leading to an overestimation of actual exposure.

p. 24 – which portion of the aquatic plant is the animal assumed to eat? Stems or roots? Different receptors have different patterns; roots are likely to be much more contaminated than stems.

p. 26 – this page brings up the question I addressed above, about how "random diets" are determined; a single random selection among the potential items or a Monte Carlo simulation to develop a probability distribution?

p. 29 to 31 – add bioavailability issues and a discussion of potential interactive effects of contaminants of concern to the list of assumptions.

Chapter 5
p. 2 – This is the first time "ecorings" is mentioned, and it needs more definition. Are the spatially averaged concentrations of the chemicals averaged within each ring, or within the entire 2 Km area? Similarly, is the home range % calculated for each ring? How is the exposure to animals that traverse the rings incorporated? The information provided requires further clarification.

p. 3 – In the list of 5 bullets at the bottom of the page, it is stated that the probability density functions of the HQs are calculated "in much the same manner as with the Human Risk module." This raises several concerns. First, it is not sufficient to state that this is similar to the Human Risk module, since most people reading this material are not going to be familiar with the Human Risk models. Second, and more importantly, aggregating Hazard Quotients into a Risk Index is problematic at best for human health risk assessments and completely inappropriate for ecological assessments. Hazard Quotients are nonmetric indices. They have no units and are not linear functions of anything. HQs cannot be added, multiplied, or otherwise subjected to arithmetic manipulations and return any meaningful information, particularly when they are based on different endpoints (e.g., reproduction effects vs. benthic community structure). Setting

up a cumulative probability density function of HQs is, however, acceptable, because it merely tells you the probability of having one or more HQs greater than a given number (e.g., 1) at a site.

Why can HQs not be manipulated arithmetically? Let's look at an example. Suppose we have 5 chemicals to which a particular species is exposed. The HQ for each chemical is 0.5; that is, the species is exposed to less of each chemical than would be expected to cause an effect. If the toxic action of each chemical is independent of the others, then the fact that they are simultaneously exposed is irrelevant, and their hazard would not become greater than 0.5 (e.g., 5 times 0.5 equals an HQ of 2.5, indicating a risk, but this would not be true in this case). If the modes of action of the chemicals were similar (and, therefore, additive), then an HQ of 2.5 would be appropriate. However, if they are antagonistic, then the HQ would be less than 0.5, and if they were synergistic the HQ would be greater than 2.5. So, the HQ depends upon *mode of action* **and** should be calculated by first adding all the exposures in appropriate portions and then comparing to the toxicity threshold of the *mixture*. In other words, a single new HQ is developed that is not the result of adding (or multiplying) the HQs for individual substances.

Now let us suppose that we have 5 receptors exposed to the same chemical. Because their exposures and toxicity thresholds are different, an HQ of 0.5 for each is based on a different equation. Therefore, adding, multiplying or otherwise manipulating the HQs does not make any empirical sense. If none of them are at risk, why should 5 of them together have a risk index of greater than 1?

In conclusion, I continue to admonish against the use of Hazard Indices or arithmetic manipulation of HQs in any manner. Generation of PDFs to determine how many HQs are greater than a particular number is, however, perfectly acceptable.

Thus, the calculation of "number of receptors times the average HQ for a bin, summed over all the bins" does not provide any meaningful information.

p. 6 – Again, I restate my argument that EBs and CSCLs are not population or community metrics unless they were derived in that manner (e.g., sediment values based on benthic community structure). Therefore, the statement of achieving "…*de minimus* level of effects to communities of organisms…" is a gross overstatement.

p. 10 – the argument is presented here for use of a scaling factor of body weight to the ¼ power when extrapolating toxicity data across species. While this was used in the Sample et al. benchmark calculations, it has since been recognized that it is a gross oversimplification and may be incorrect in mammals in much the same manner as was shown for birds. This extrapolation may be used in Human Health assessments, because we are extrapolating from species selected to resemble humans as closely as possible from a physiological point of view, and need to make adjustments relative to metabolic rate and size. For ecological assessments, however, we are extrapolating among species that are very different, particularly when looking at the dietary route of exposure. A deer, for example, is a ruminant with significantly different gastrointestinal physiology than a fox (carnivore; short GI tract) or a rabbit (hind gut fermenter). Also, we know there are significant differences in detoxification mechanisms, such as metallothionein (very well developed in horses and less so in some other species), target organ dose, etc. So, one needs to first sort the animals by physiological parameters and then make appropriate extrapolations. If this is not possible, using body weight without a metabolic rate adjustment is more appropriate.

p. 11 – it is not clear why different levels of protection are afforded to herpetofauna than are given to birds and mammals. Herpetofauna benchmarks are based on LC_{50}s, while birds and mammals use NOEAL and LOAEL values for sublethal endpoints. Thus, herpetofauna are not as protected as the other classes of animals. An explanation and justification for this difference is needed (beyond the fact that "it is prohibitive to develop a chronic CSCL at this time").

p. 12 – more information is needed to understand how the data for plant CSCLs were selected. Was soil type given any particular weighting, since chemicals are more bioavailable in sand than in clay, organic matter has an influence on the uptake rate, etc.? Many plant studies on metals were done with mixture and/or with sewage sludge ("biosolids") which greatly influences plant responses. Were these studies used or not? How were differences in study design accommodated (seedling growth versus full life cycle studies)? Were hydroponic or filter paper studies used? Is all this information detailed elsewhere?

p. 13 – soil invertebrate species sensitivity distributions used the 50^{th} confidence interval around the 95^{th} percentile, while the plant species sensitivity distributions did not use the confidence interval. Why are these two approaches different? A discussion is needed here about the assumptions and potential pitfalls of the sensitivity distribution approach. For example, the type of distribution used (lognormal, triangular, etc.) can have an effect on where the 95^{th} percentile is placed; was this examined for these data? Outliers may have an effect on the both ends of the distribution (e.g., very insensitive species will cause the lower end of the distribution to be extended as well); how was this handled here? (Note that the ambient water quality criteria make use of only the 4 most sensitive species, from which a triangular distribution is developed and the 95^{th} percentile derived.) Use of the species sensitivity distribution does not account for essentiality of metals, so the final number may be below required amounts for some organisms; was the result checked against sufficiency levels for essential elements? How representative were the species that made up the distribution? Are any particularly important groups missing or, conversely, over represented?

p. 14 – earthworms are not uniformly "more susceptible to contaminant exposure" (than what?). They line their burrows with organic matter so are not always in direct contact with the soil, they have avoidance behaviors to move deeper or laterally away from contamination pockets, etc. For some contaminants, Collembola have been shown to be more sensitive and at time centipedes may bioaccumulate more of a substance than earthworms.

p. 16 – sediment quality numbers derived by the Long and Morgan approach have been acknowledged to be problematic. These numbers were generated by association of benthic community changes with mixtures of contaminants; however, the effect is individually ascribed to each contaminant as though it were acting alone. These (and other) shortcomings need to be explicitly acknowledged. There are other sources and jurisdictions that have developed sediment quality guidelines and it would be worthwhile spending the time to examine alternatives before deciding on which one(s) to use.

p. 17 – were algae data used to set toxicity limits for aquatic plants (i.e., rooted aquatic macrophytes) as well? This is a bit of a stretch and, if so, needs to be acknowledged as an area of uncertainty.

p. 18 to 19 – see above discussion about use of persistence and bioaccumulation as "confidence indicators" for the habitat definitions.

p. 20 – it is not clear why this section on calculating average concentrations discusses water and sediment but not soil. It shows how concentrations were averaged for reaches; shouldn't a discussion be provided

for how soil concentrations are averaged by habitat (and "ecorings?")? Also, if "foc" (fraction of organic carbon) is applied to sediment, why is it not applied to soil?

p. 23 – how does "home range" fit into averaging soil concentrations for plants and soil invertebrates? Shouldn't this be done by habitat?

p. 25 – I suggest that equation 5-16 be deleted. See my impassioned discussion above about why HQs cannot be summed, averaged, or otherwise manipulated arithmetically. This also is nicely expressed in the last bullet on Page 5-27…

Dr. Kapustka:

- There is a fallacy repeated often in discussions of Water Quality Criteria to the effect that the Criteria are designed to protect 95% of the species 95% of the time. The origin of this mantra derives from attempts to explain statistical distributions. However, it is fundamentally false that setting the alpha level at 5% equates with a willingness to sacrifice 5% of the species. Indeed, the 95th percentile of a distribution of a small set of values often exceeds the lowest observed vales. In water quality procedures, the effect of the calculations is that effectively, all species are protected all the time. In this document, a similarly erroneous description of the 95th percentile distribution is made. Both from a technical perspective and from a risk communications perspective, EPA would be wise to correct this discussion.

- In the discussions of exposure, the authors use two categories i) earthworms and ii) soil invertebrates. At a minimum, these sections should be corrected to i) earthworms and ii) other soil invertebrates.

- On page 2-4 the term "continental United States" is used. Alaskans might be surprised to learn that they are not on the continent.

- On page 4-13, it is less important that an ecologist implemented the decision rules than to have the decision rules developed by an ecologist.

- Table 4-4 lists silage as a dietary intake item. I didn't think the risk assessment dealt with domestic animals; does it? Generally, the categories listed do not make much sense botanically. Silage is a fermentation product of chopped forage; grains are solely from graminaceous plants, and certainly do not include nuts; what does exposed fruit? or exposed vegetation mean? The break out of the "forage" and "exposed vegetation" types go way beyond any capacity to allocate tissue concentrations. Unless one intends to gather site-specific empirical data for these different items, it is disingenuous to suggest that these are treated in the exposure models. I recommend stating that all these values are set to some default value (pick one that can be defended from the uptake models); and suggest that with site-specific measurements, the exposure models could be improved dramatically.

- Check the assumption of home range on page 4-15. Should the linear distance be the radius or the diameter? It may vary from one source to another.

- Is it valid to take the average concentration of surface waters (page 4-17) given that ponds are lumped with streams? Wouldn't this depend greatly on proximity to the input source? Ecologically, it would differ among animal preferences for standing water versus moving water.

- In text Box 4-1, it's not clear whether soil ingestion is included in the capping algorithms. Wouldn't it be best to start the calculations by including the soil ingestion percentage, and then accumulate the rest of the diet to 100%?

- On pages 4-20 and 4-21, check to see that the use of wet weights and dry weights are consistent across all calculations. In particular, make sure that toxicity values were reported in the same units as used in these exposure estimates. Often, some conversions are needed.

Dr Sample:

- Figure 1-3—The term "PF" under methods needs to be defined.

- Footnote 2—page 1-6—The definition of assessment endpoint is vague. What is ecologically significant? What is population sustainability? What level of effect is acceptable? It is essential that these be defined in clear and unambiguous terms.

- Spatial bin description on page 1-7 is vague. Home range is important but the description sounds as if only individuals are being assessed. What number of bins/animal are assessed in a habitat? That is the measure of risk.

- Section 1-4, 1st paragraph—What about risk to fish, benthics, and soil biota?

- Typo Page 2-1-Section 2-1—"EPA 19991"

- Section 7.4—How many receptors within each bin size were placed in each AOI? Should be that as many as available habitat would support should be used. Seems like too much effort is being placed on resolution of spatial issues given the quality of available data. Suggestion at each AOI - ID amount of each habitat type – Calculate distribution of chemical concentration in each habitat type – assign receptors to each habitat based on home range and amount of available habitat.

- Page 3-1—It is unclear how weight of evidence can be used to select receptors.

- Page 3-3—Species sensitive to urbanization should not be used as receptors if urban land use in and around AOI is high. Black bear is an example. Inclusion of these species misrepresents risk estimates and reduces credibility of output.

- Figure 3-1—Neither least weasels or shrews are omnivorous. Shrews are insectivorous and weasels are predators. A box for carnivorous small mammals should be added. Moving of shrews to insectivorous box should be considered.

- Figure 3-2—Ducks should be listed as omnivorous; depending on the species, large amounts of insects and other invertebrates could be consumed.

- General Question—How were functional groups defined? Ecologically, groups many do not make sense. Quite a few problems with selection and assignment of receptors with habitats and functional groups incorrectly assigned.

- Section 4.1—It needs to be made clear that EcoEx is only for wildlife; exposure for all other receptor groups is based on media concentration.

- General Question—It appears that all exposure and risk estimates are based on individuals. Assessment endpoints are based on populations – how is jump from individuals to populations made? This needs to be explained.

- Section 4.3.1.1-this is not really an exposure calculation – ingestion of water is not estimated. What is presented is simply the algorithm for estimating contaminant concentrations in surface water. To be complete, $C^{j}_{sw\_ave}$ should be multiplied by water ingestion rate to provide an estimate of the dose from water. [note–typo in equation 4-4].

- Section 4.3.1.1 and Box 4-1. Sub- and superscripts are inconsistent and confusing. In equation 4-4, 'j' refers to both receptors and reaches. 'k' is included but not described. In Box 4-1, 'i' refers to receptors and 'j' to food types; 'k' is also added (but not specifically define) as 'j+1'. Consistency across all equations is essential.

- Page 4-18, 1st para. – reference to Table 4-3 is incorrect – should be 4-1?

- Equation 4-5. 1st term in equation and subscripts for CF do not match the legend. Modeling approach is difficult to follow and cannot be verified – Addition of $CF_{DW\text{-}WW}$ at this stage of the model is questionable. Would be better to estimate wet weight ingestion rates (using the range of diet compositions for a species) than to insert this conversion factor into the equation here. How is the value of $CF_{DW\text{-}WW}$ derived? It is entirely unclear as to where the values for this will come from and whether or not they are correct. If this information is included in supporting information presented on the WWW site (which after searching does not appear to be the case), it should be moved into this report. In addition the equation is unnecessarily complicated by splitting exposure from plant foods from that for animal foods. An alternate representation of the exposure model that could be used is listed below:

$$E_j = \left[\left[Soil_j \times P_s \times FIR\right] + \left[\sum_{i=1}^{N} B_{ij} \times P_i \times FIR\right] + \left[Water_j \times WIR\right]\right] \times AUF$$

Where:

E_j = total exposure to chemical (j) (mg/kg/d – wet weight)

$Soil_j$ = concentration of chemical (j) in soil (mg/kg dry weight)

P_s = soil ingestion rate as proportion of diet (unitless)

FIR = species-specific food ingestion rate (kg food/kg body weight/d – wet weight)

B_{ij} = concentration of chemical (j) in biota type (i) (mg/kg – wet weight)

P_i = proportion of biota type (i) in diet (unitless)

$Water_j$ = concentration of chemical (j) in water (mg/L)

WIR = species-specific water ingestion rate (L/kg body weight/d)

AUF = Area Use Factor (home range/contaminated area; unitless)

All parameters currently used in the 3MRA model can be modified and incorporated into the framework above. Ingestion rates can (and should) be corrected to be expressed in terms of L or kg/kg bw/d. This form of the model is much more straight forward and easy to follow than is the form currently applied in the 3MRA.

- Section 4.3.2.1 and throughout Section 4.3.2– States that it was assumed that wildlife would not use impoundments and they therefore were excluded from calculations for water ingestion. This is inappropriate. Although most impoundments may not have fish, many (if not most) are likely to support marginal vegetation and aquatic invertebrates. As such, they will provide habitat and food for wildlife and will present exposure pathways.

- Table 3-1 – Kit fox is listed as being in trophic level T3. This is incorrect – they should be T2. Kit foxes are small predators which are preyed upon by coyotes and other larger predators.

- Page 4-3. Acronyms TerFW, SW, and SR need to be defined when first used. This should be applied throughout the report – all acronyms need to be defined.

- Section 4.2.2.1.- Raw data (and sources) used to develop selected body weights needs to be presented.

- Section 4.2.2.2.- Food ingestion rates should be calculated based on more recent models presented in Nagy et al. (1999) as used in the EcoSSL development (EPA 2000).

- Page 4-10 - DW and WW need to be defined (although most people should know what they are). Use of the allometric model for iguanid lizards for *all* herps raises huge uncertainties. These uncertainties must be clearly described and summarized.

- Table 4-3 – Model parameter values are incorrect or are rounded incorrectly.

- Page 4-11 – Justification for the default assumption of 0.0001 for water ingestion by herps must be provided.

- Section 4.2.2.4 – Documentation of specific assumptions for soil ingestion rates for each species must be provided.

- Page 4-14 – the term FFC needs to be defined.

- Section 4.2.2.6 – 2nd Para – Assumption that home ranges expressed as linear distances represent a radius grossly overestimates home range size for these species. Linear home ranges are strips, not

circles. Home ranges for these species are expressed linearly because the species habitat preferences are defined by linear land features, such as stream corridors.

- Section 5.2.2.1, Eqns 5-1 and 5-2 – Allometric scaling values for interspecies extrapolation have been updated beyond the values employed in this section. It is suggest that the chemical-specific scaling factors from Sample and Arenal (1999) be used, if available for chemicals considered in the 3MRA model. In the absence of chemical-specific factors, the mean scaling factors for birds and mammals should be used.