US EPA ARCHIVE DOCUMENT

US EPA ARCHIVE DOCUMENT

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

## Terrestrial Food Web Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99

*Prepared for:*
Dr. Zubair Saleem
OSW/EMRAD
U.S. Environmental Protection Agency
2800 Crystal Drive
Arlington, VA 22202

EPA Contract No. 68-W-99-001
Work Assignment No. 225

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

October 5, 2001

**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 *Terrestrial Food Web Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99* and *Data Collection for the Hazardous Waste Identification Rule: Section 10.0 Farm Food Chain and Terrestrial Food Web Data* documents that are 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.

US EPA ARCHIVE DOCUMENT

**Peer Review Charge for the Terrestrial Food Web Module and Supporting Module Input Data**

**Background**

The multi-media, multi-pathway and multiple receptor risk assessment (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 3MRA 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.



US EPA ARCHIVE DOCUMENT

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.



US EPA ARCHIVE DOCUMENT

## Terrestrial Food Web Module

The Terrestrial Food Web module (TerFW) calculates chemical concentrations in soil, terrestrial plants, and various prey items consumed by ecological receptors, including earthworms, other soil invertebrates, and vertebrates. These concentrations are used as input to the Ecological Exposure (EcoEx) module to determine the applied dose to each receptor of interest (e.g., deer, kestrel). The module is designed to calculate spatially-averaged soil concentrations in the top

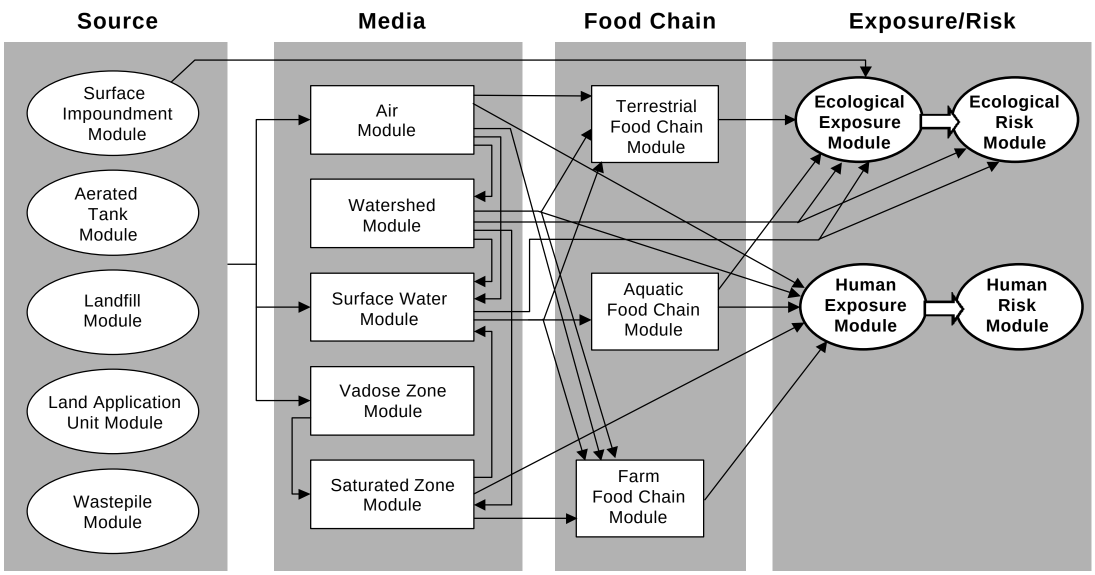


**Figure 1. Source, fate, transport, exposure, and risk modules of the 3MRA Model**

layer of soil (i.e., surficial soil) as well as deeper soil horizons (i.e., depth-averaged over approximately 5 cm), and to predict a range of concentrations in plants and prey items to which a given receptor may be exposed. The spatial averages are defined by the home ranges and habitats that are delineated within the area of interest (AOI) at each site.

## Terrestrial Food Web Module Database

Data were collected to quantify parameters required to develop exposure profiles for ecological receptors evaluated in the 3MRA model. These parameters include bioaccumulation factors, partitioning coefficients, and ingestion rates that the module uses to estimate movement of chemicals through the food web. Two primary databases were constructed to support the module: the chemical-specific database and the exposure-related database. Data sources for databases included EPA documents and primary literature.

US EPA ARCHIVE DOCUMENT

**Materials to be Reviewed:**

US EPA, 1999. Terrestrial Food Web Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) For HWIR99.

U.S. EPA, 1999. Data Collection for the Hazardous Waste Identification Rule, Section 10.0 Farm Food Chain and Terrestrial Foodweb Data

**Peer Review Charge**

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

1. Comment on the organization of the review documents. Do the documents present the information in a clear, concise, and easy to follow format? If not, please provide suggestions to improve the presentation.

2. Is there an adequate description of the purpose and context for the Terrestrial Food Web Module and its companion Data report? 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 limitations or data gaps in the terrestrial food web module or supporting database that have not been identified? If so, how could they be addressed in the near-term (for example, less than six months) and the longer-term?

In addition, the following specific issues should be addressed.

4. The algorithms used to estimate biotransfer factors do not distinguish physiological differences across various kinds of plants. For example, in estimating biotransfer factors for the category "forage" (e.g., forbs, grasses, fungi, shrubs, trees), it is implicitly assumed that the physiological differences in different plant species do not significantly affect chemical loadings in plant tissues. Is this a reasonable approach for a national-level assessment?

5. Within the module, two soil concentrations are calculated: 1) a surficial soil concentration (1-cm) used to estimate incidental soil ingestion exposures for foraging animals, and 2) a 5-cm depth averaged soil concentration used for plant uptake and direct exposures for soil dwelling organisms. Is this a reasonable approach to estimate exposures for different types of ecological receptors and, if not, can you recommend an alternative?

6. We designed an approach for estimating prey tissue concentration that modifies contaminant exposures in the prey based on the fraction of home range that is located within the habitat rather than assuming 100% of its diet is contaminated.



US EPA ARCHIVE DOCUMENT

The purpose of this adjustment is to acknowledge that wildlife can feed outside of the defined area of interest. Is this a reasonable approach for a national-scale assessment?

7. In general, there are a number of data gaps in the terrestrial food web database. We combined field-derived and laboratory-derived data and used small mammals data as surrogate values for other types of vertebrates in the food web. Are these approaches reasonable for including information into the database for a national-scale assessment? Are there data sources or references that you are aware of that would reduce the uncertainties associated with this approach?

8. A default factor of 1.0 was applied for chemical uptake into prey organisms when chemical-specific data were not available. Would you recommend a different approach rather than assuming a default value of 1.0 which may likely lead to an over- or under-estimation of uptake into prey species and which could be developed and implemented within a timeframe of from one to several months?



US EPA ARCHIVE DOCUMENT

## General Comments

**Dr. Fairbrother:** The Terrestrial Food Web Module is relatively straightforward and reasonably well laid out. Overall, it follows standard methods and appears appropriate. The Data Collection support document, however, is rather confusing. Farm food chain and terrestrial food web data are gathered into a single report, and it is unclear which data are being used for which modules. I think it would be much better to have two separate reports, even if this means exact duplication of many of the sections. Thus, someone running the Farm Food Chain module would know that all the information from the data collection document would be used, as would those running the wildlife food web module. Otherwise, the documents are well laid-out. The Uncertainty sections are very welcome; however, it would be very helpful if they would be more explicit about whether the default assumptions will result in over- or under-estimation of risk.

**Dr. Sample:** The Terrestrial Food Web Module document was generally well written and understandable, albeit somewhat jargony. The authors have created some new terms which I think are of questionable utility. For example, I think the terms 'omnivert' and 'herbivert' (page 3-3) are unnecessary and potentially confusing. As another example, in equation 3-20, instead of having a moisture adjustment factor (MAF) that is subtracted from then divided by 100, why not simplify the equation to:

$$Pxx_{ww}=Pxx_{\_DW}*F_{DW}$$

Where $F_{DW}$ is the dry weight fraction.

There is also some internal inconsistency. On page 2-1 of the Terrestrial Food Web Module document, 5 cm is stated as the assumed soil averaging depth. However, on page 3-1, this depth is stated to be 10 cm. Which is actually used in the model? This inconsistency aside, many, if not most, plants are likely to have rooting depths of >10 cm. If the shallower depth was selected to be conservative and prevent dilution of contaminants over depth, that is OK. It simply needs to be stated in the document. Regardless, references to support any selected rooting depth (or to support an assumption that it is conservative), should be provided.

Throughout the report, there is a reliance on simple BCFs, BAFs, or uptake factors, regardless of whether or not loglinear bioaccumulation models exist. Multiple studies, including some cited by the authors, have indicated, if it is the authors intent to be conservative, then this is OK. However, if accuracy is desired, loglinear models have been shown to produce better result.

Ultimately, one of the greatest concerns I have for this module is the general lack of data for many of the chemicals evaluated. This results in the extensive use of default values. Depending on the parameter, values are lacking for anywhere from 41% to 98% of the 46 chemicals listed in Appendix 10A. For 24 chemicals, all values for <u>all</u> parameters are listed as default values. I understand issues associated with data limitations, but I question the utility of any model results for which all values are set at an assumed default. If data are lacking, some effort should be applied to develop some better rules as to what the default should be. Simply setting a value to one because data do not exist is not very defensible.



US EPA ARCHIVE DOCUMENT

## Responses to Specific Charge Questions

1. *Comment on the organization of the review documents. Do the documents present the information in a clear, concise, and easy to follow format? If not, please provide suggestions to improve the presentation.*

**Dr. Fairbrother:** Organization – see General Comments above.

**Dr. Pastorok:** Overall, the organization of the documents is excellent, and the writing is concise. In certain areas, the writing is too concise. Certain concepts are not presented clearly (e.g., the home-range based spatial analysis; the mechanistic basis for some processes represented in the models) and would benefit from a graphic presentation of how the model works. The overall approach to be used in implementing the model is not clear. From several places in the text, it appears that minimum and maximum values will be used for some variables in separate runs of the model to characterize variability and uncertainty. However, the authors do not clearly state how many variables and which ones will be treated in this way. It appears that an interval (min, max) approach will be used as the overall basis for quantifying uncertainty, but the rationale for such an approach rather than Monte Carlo analysis is not presented.

**Dr. Sample:** The general organization of both documents is adequate and appropriate. I would suggest however that Section 10.3.1.3 be dramatically expanded. Information presented here does not adequately address the significant uncertainties presented with the models and the available data.

2. *Is there an adequate description of the purpose and context for the Terrestrial Food Web Module and its companion Data report? If not, please explain.*

**Dr. Fairbrother:** Purpose and context are adequately described for the Terrestrial Food Web Module. The Data report requires some additional explanation vis-à-vis differences between application to Farm Food Chain and Terrestrial Food Web models.

**Dr. Pastorok:** The introductory sections (Overview) did not establish the context well enough and did not describe the systems being modeled in sufficient detail. Additional explanation of the rationale for the model, the components of the modeled system, and the key processes is needed for the reader to understand why the modeling is being done. What are the habitats and receptors being modeled? These need to be described early in the document. Halfway through the document, I found a mention that plants and soil invertebrates would be considered as receptors. There was no hint of this before this mention. Moreover, the uses of the terrestrial food-web module need to be described. Is it only a forward-mode model? Will it be used to derive exit concentrations?

**Dr. Sample:** Yes - adequate description of the purpose is provided



US EPA ARCHIVE DOCUMENT

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 limitations or data gaps in the terrestrial food web module or supporting database that have not been identified? If so, how could they be addressed in the near-term (for example, less than six months) and the longer-term?*

**Dr. Fairbrother:** Additional data – see Specific Comments below.

**Dr. Pastorok:** There are three primary limitations of the methodology. The first is the assumption of a linear relationship between tissue and soil concentrations, which is inherent in the use of constant biotransfer factors. Tissue-soil relationships are not always linear. The relation is usually sigmoidal to a greater or lesser extent. The document should make use of any available nonlinear tissue-soil relationships for earthworms as derived by researchers at Oak Ridge National Laboratory (Sample et al. 1998). At the very least, the transfer factors should be selected to most closely match the measured concentrations in the primary medium (such as soil). This process could easily be accomplished by listing multiple concentration-based transfer factors in the database. The second major methodological limitation is the use of equilibrium partitioning theory to derive chemical concentrations in root vegetables (Equations 3-15 and 3-16). U.S. Environmental Protection Agency (EPA) has recently abandoned the use of this theory to derive an approach to sediment quality guidelines. Again, this approach assumes linearity and ignores the sigmoidal nature of the relationship. Tissue-soil relationships should be based on empiric data. Where data gaps presently exist, the research should be done to provide the information. For years, people have been using theoretical biotransfer factors and pseudo-empirical approaches based on the assumption of linearity. We see over and over again in site-specific data that these approaches don't work. So why does EPA think it is justified in applying these equilibrium approaches to soil-water systems in a national-level model? This issue deserves detailed discussion in the text. The third methodological limitation is the use of uniform distributions based on minimum and maximum estimates. Because the model is already spatial in nature, it would be possible to characterize the proportional representation of the sampling using a two-dimensional interval analysis. Failing this, it could be simply assumed, based on past observations, that the distribution of the contamination is lognormal with a mean and variance derived from the sampling distribution. In either case, the contaminant distribution would be more accurately demonstrated. This approach would reserve the uniform assumptions for factors for which no distribution could be inferred. In effect, the model could then randomly select between minimum and maximum lognormal distributions. The algorithms to institute such an approach would be reasonably straightforward.



US EPA ARCHIVE DOCUMENT

**Dr. Sample:** Most data gaps in the TerFW module have been identified. Additional data development could be used however to reduce the influence of these uncertainties. As part of the data development for the Army Risk Assessment Model (ARAMS) we have developed a dataset containing bioaccumulation data for multiple taxa of terrestrial arthropods. Data are primarily for metals and other inorganics. Search for data on organics has not been performed. In addition to these data, we have developed some information concerning the variation in bioaccumulation across plant tissues (i.e., foliage vs root vs seeds vs fruit). Preliminary analyses have been performed on these data. Additional work would be needed before they would be suitable for use.

4. *The algorithms used to estimate biotransfer factors do not distinguish physiological differences across various kinds of plants. For example, in estimating biotransfer factors for the category "forage" (e.g., forbs, grasses, fungi, shrubs, trees), it is implicitly assumed that the physiological differences in different plant species do not significantly affect chemical loadings in plant tissues. Is this a reasonable approach for a national-level assessment?*

**Dr. Fairbrother:** Biotransfer to plants – given the lack of data for development of models with sufficiently small confidence intervals to be of use, it is appropriate to group all plants in the development of these uptake factors. Of course, it is known that different plants take up materials from soils differentially, but this is not always a function of whether a plant is a grass vs. a legume vs. a shrub. Some plants are hyperaccumulators (e.g., rabbitbrush that accumulates selenium) while others within the same morphometric group are not (e.g., sagebrush). Therefore, the uncertainty in realistic development of plant uptake factors would not necessarily be reduced by separation on this basis. Differentiation of amount of contaminants in roots, shoots, seeds, fruits, etc. is much more important, and has been applied to these models as there are data available for this approach.

**Dr. Pastorok:** The contaminant-specific database for plant and contaminant-specific plant transfer factors is recognized as currently inadequate. Therefore, at this time, the assumption of a similar biotransfer factor across plant types may be reasonable for a generic, national-level model. However, it is of concern that the model mechanistically groups unlike plants under broad categories such as forbs. This grouping would make it difficult in the future to incorporate more specific data as it becomes available. The plant categories within the model should be made more specific, at least within categories of family. Generalizations based on lack of specific data could then be applied at the database level (i.e., applying the same transfer factors to multiple categories). This approach would provide a system that would be more amenable to future developments in our general understanding of plant uptake.

**Dr. Sample:** We know that this assumption (physiological differences in plant species do not affect chemical loadings) is not true. If it were, there would be no such thing as hyperaccumulator plant species. That said, we do not have adequate data to allow us to estimate how bioaccumulation varies across all plant taxa and chemicals. We do know that responses will vary considerably by taxa and within taxa depending on environmental conditions, we just cannot predict it. Given the state of available data to address this topic, this approach however is reasonable. Use of this assumption could be strengthened if literature were evaluated and



US EPA ARCHIVE DOCUMENT

information to help put some bounds on the level and nature of uncertainty and variability were provided.

5. *Within the module, two soil concentrations are calculated: 1) a surficial soil concentration (1-cm) used to estimate incidental soil ingestion exposures for foraging animals, and 2) a 5-cm depth averaged soil concentration used for plant uptake and direct exposures for soil dwelling organisms. Is this a reasonable approach to estimate exposures for different types of ecological receptors and, if not, can you recommend an alternative?*

**Dr. Fairbrother:** Soil depth – it is reasonable to use both surficial soil and a 5-cm averaging depth, as has been used in this model. See below comments for a brief discussion about leaf-litter.

**Dr. Pastorok:** The use of two soil strata to estimate exposure concentrations is insufficient. The variety of receptors and of prey considered dictates that more strata be used. There is little computational efficiency gained by using only two strata. The use of a 5-cm layer for all soil invertebrates and plants is a flawed approach. For example, earthworms are known to range from the surface to the water table, and their patterns are highly species-specific and impacted by local climatic conditions. Furthermore, trees and grasses have very different root horizons and are in contact with different layers of the soil. Moreover, the assumption that all incidental soil ingestion is related to the surficial layer is invalid for burrowing mammals, birds, and reptiles. The recommended approach is to use more soil strata and tailor the definition of depth horizons to the receptors being used.

**Dr. Sample:** Although separation of surface and deeper soil concentrations in terms of exposure makes sense, I'm not convinced that 1 and 5 cm are sufficiently different to warrant the distinction. In many assessments I've been involved in, surface is considered to be 0-15 cm. Many plants have roots extending to meters in depth. Burrowing animals almost always will go to depths greater than 5 cm. A review of rooting depths and depths to which soil invertebrates may be found is presented in Suter et al. (2000). Given the small difference between the values selected in the TerFW model, I would recommend using only a single value.

6. *We designed an approach for estimating prey tissue concentration that modifies contaminant exposures in the prey based on the fraction of home range that is located within the habitat rather than assuming 100% of its diet is contaminated. The purpose of this adjustment is to acknowledge that wildlife can feed outside of the defined area of interest. Is this a reasonable approach for a national-scale assessment?*

**Dr. Fairbrother:** Prey tissue concentration as a function of home range – it is difficult for me to reconcile the statement in the Charge to Reviewers with the description in the Module documentation. The Charge states that the concentration in prey tissue is estimated assuming the prey animals (e.g., small mammals?) forage on both contaminated and uncontaminated areas (e.g., based on the fraction of home range what is located within habitat, rather than assuming 100% contaminated diet). The document states that they "forage only within that part of the home range that is contained within the AOI." Is the AOI considered 100% contaminated, or is it made



US EPA ARCHIVE DOCUMENT

up of a mosaic of contaminated and uncontaminated habitats? And how does this 100% AOI agree with the statement of basing exposure on the fraction of home range that is located within the habitat? I agree with the statement as it is made in the Charge to Reviewers, and would like to see the Module documentation written to clarify that this is the approach taken.

**Dr. Pastorok:** The use of a home range factor (or area use factor) in estimating exposure is an approach that is now widely accepted among ecological risk assessors. This approach is a reasonable approach for a national-level assessment. However, the use of a randomly placed multiple home range for each home range size class is not valid, nor, within the resolution of the model, necessary. The following recommendations are offered:

- The model should be executed based upon maximum potential exposure and regional background exposures. Any offset of the habitat range from the maximum will then result in a linear reduction in the exposure proportional to the difference in the contaminant concentrations within and outside the site and the proportion of relative exposure to site versus not-site. Because the model currently does not differentiate variation within these two potential exposure sources, no need exists to arbitrarily assign random home ranges.

- The level of risk should be defined as that attributed to the site based on differential contaminant distributions between site and not-site.

- Where possible, a third distribution could be included based on best available information on the locations of the receptors. This step could be done based on site-specific data or hypothetically by utilizing comparative habitat suitability methods.

- The 10 percent limit on the site exposure should be removed. This limit is arbitrary and defeats the purpose of the spatial analysis. If the analysis indicates that the exposure is less then 10 percent, then the exposure is less than 10 percent.

- Temporal considerations were not mentioned. These considerations can be very important, particularly with regard to the assessment of the impact of persistent and bioaccumulative contaminants and migratory species. Season considerations and duration of exposure for impact should be included in the exposure module. The algorithm is straightforward, and it will only increase the size of the database by one field per contaminant.

EPA should refer to Clifford et al. (1995) and Mackay et al. (2001) for an appropriate approach to spatial analysis of chemical exposures for wildlife species.

**Dr. Sample:** Although I have no significant concerns about modifying exposure based on home ranges in general, I have concerns about their application in the TerFW model. Modeling efforts can only be as accurate as their least accurate input data. Given the large uncertainty concerning bioaccumulation and food web transfer, I think that the adjusting exposure based on habitat is questionable. Habitat-based adjustments in this case provide a false sense of precision.



US EPA ARCHIVE DOCUMENT

7. *In general, there are a number of data gaps in the terrestrial food web database. We combined field-derived and laboratory-derived data and used small mammals data as surrogate values for other types of vertebrates in the food web. Are these approaches reasonable for including information into the database for a national-scale assessment? Are there data sources or references that you are aware of that would reduce the uncertainties associated with this approach?*

**Dr. Fairbrother:** Data gaps – there are many data gaps for estimating terrestrial food web exposures. For the most part, these were appropriately acknowledged and included in the documentation. In the specific comments below, I point out a few areas where there likely are more data available and provided a few examples. I also suggested an alternative approach to using small mammal concentrations as estimates for all vertebrate prey. No matter what we do, however, all these assessments will continue to suffer from lack of data. Unfortunately, there is no national program directed towards generating the information needed. Instead, we all continue to patch together the available information, model what we can, and use default values to fill in the gaps. Then we hope that we are conservative enough to not make too many Type 2 errors (i.e., to say something is safe when it isn't) and not so conservative as to loose credibility (i.e., require clean-up targets that result in unnecessarily large expenditures of funds). This is particularly difficult for a national level assessment such as being done here, where site-specific studies cannot be done to help with filling data gaps. The authors are to be acknowledged for their efforts in wrestling with the continuing dilemma of supporting environmental management decisions through the use of sound science without sufficient data.

**Dr. Pastorok:** As noted elsewhere, EPA should use the available information on nonlinear relationships for uptake of soil contaminants by earthworms from Sample et al. (1998). An additional data issue related to biotransfer factors may exist. On p. 10-8 of Data Collection for the Hazardous Waste Identification Rule, the authors list a U.S. Department of Energy Document on biota-sediment accumulation factors (BSAFs) as a data source for the terrestrial food web model. However, sediments were not included in the model as an exposure media. Extrapolation of BSAFs to soil systems is not technically valid.

**Dr. Sample:** The use of small mammals to represent other terrestrial vertebrates is appropriate. In support of the EcoSSLs and for other projects I have conducted extensive searches for data on bioaccumulation in other vertebrate taxa. These data are extremely limited. The greatest dataset is for small mammals.

8. *A default factor of 1.0 was applied for chemical uptake into prey organisms when chemical-specific data were not available. Would you recommend a different approach rather than assuming a default value of 1.0 which may likely lead to an over- or under-estimation of uptake into prey species and which could be developed and implemented within a timeframe of from one to several months?*

**Dr. Fairbrother:** See Appendix 4 of the USEPA EcoSSL guidance (June 2000) for a detailed discussion of uptake equations for use with various classes of organic chemicals. This assumes knowledge of octinol-water partition coefficients (Log $K_{ow}$), which are readily available from the



US EPA ARCHIVE DOCUMENT

risk assessment tools of the Oak Ridge Lab web site or many other chemical databases. This methodology should be reviewed and considered for use in the 3RMA model, before settling on a default value of 1. For inorganic substances, the same appendix provides information on selected elements; for others for which there are no known or reported uptake values, a default of 1 would be acceptable.

**Dr. Pastorok:** I did not see a discussion of the default value of 1 for the biotransfer factor or a rationale for it in the main document. The assumption of a default value of 1 across all chemicals and receptors is not appropriate. Some reasonable guesses at variation in default factors could be made for at least some combinations of chemicals and prey types from data in the literature.

**Dr. Sample:** Before assuming 1 as an appropriate default, I would perform an analysis of the distribution of BAFs over various soil concentrations, chemical classes, and biota types. Data already collected and used in the TerFW could be used for such an analysis. The results could then be used to support the use of a particular default value for a particular chemical class-biota type combination.

US EPA ARCHIVE DOCUMENT

## Specific Comments

**Dr. Fairbrother:** *Terrestrial Food Web Module*

1. Overview – the module is designed to calculate chemical concentrations in soil, plants, soil invertebrates, and vertebrates as input variables to the wildlife dietary module that will sum up the total mass of chemical for comparison to a wildlife threshold dose. All tropic levels are included, although some are more compressed than others. For example, vertebrate prey are lumped as a single entity, although first and second order carnivores consume different organisms within the food web and likely have significantly different body concentrations. Also, calculation of invertebrate concentrations is mostly focused on soil-dwelling organisms, while many wildlife species (especially many of the charismatic songbirds) primarily eat foliar invertebrates. The data collection module addresses some of these questions (and I have provided comments to those concerns below), but the presentation in the module layout should explicitly acknowledge any such gaps in the food web model. It is commendable that the authors titled this module a food web, rather than a food chain as is done for the farm scenario. However, the web-like nature of the terrestrial system has obviously been greatly simplified which requires some discussion.

2. Overview – this section should explicitly state that the calculations looked at the top layer(s) of soil, and did NOT include the leaf litter. Of course we know that most of the soil invertebrates (other than earthworms) actually inhabit the leaf litter and not the soil itself, but the uptake factors used to calculate their concentrations are derived from soil-invertebrate relationships. Incorporation of a detailed soil invertebrate food web, or soil ➔ leaf litter ➔ invertebrate uptake scenario, into the model is unnecessary for a screening level assessment such as being conducted here, given the paucity of data for parameterizing such a model.

3. Overview, 3rd paragraph. The statement is made that "Receptors that ingest plants and soil invertebrates... are presumed to forage only within that part of the home range that is contained within the AOI..." See Comment #6 above.

4. Overview 4th paragraph (top of page I-2). The categories of vegetation include one called "silage." Silage is, of course, fermented green forage, generally made from alfalfa or corn. What, exactly, is being referred to here in regard to wildlife consumption? I suggest that a different term be used (something other than "crops" which is equally nondescriptive).

5. Reference – the new document being produced by US EPA titled "*Methodology for Assessing Health Risks Associated with Multiple Exposure Pathways to Combustor Emissions*" is referred to with some regularity throughout this document. It is frustrating that so much reliance is put on a document that we were unable to review. However, I assume that this document was an update of the 1998 version and the basic concepts and approaches remained the same. The primary shortcoming of using this document is its reliance on crop plants (and some ornamentals) and farm animals for deriving default



US EPA ARCHIVE DOCUMENT

values.  However, the physical processes of uptake and diffusion by plants are similar regardless of species, so the approach is generally applicable.

6. Assumptions and limitations second bullet – it would be helpful to state here that the model recognizes differences in uptake between plant roots and plant shoots (i.e., that various chemicals preferentially remain in one or the other).

*Data Collection Section 10.0*

1. Page 10-10 2nd to last paragraph – the default biotransfer factor for metals other than mercury into beef and milk is 2E-05.  Why was this particular number chosen?  Certainly it depends upon whether or not the metal is an essential element.  And is it really true that the amount in milk is equivalent to the amount in muscle tissue? Furthermore, wildlife consume organ meats which tend to concentrate metals to much higher levels than does muscle (e.g., cadmium accumulates in kidney, copper and lead in liver, etc.).  Some effort should be made to incorporate this variable into the model.  This could be done using the muscle BAF and adding liver/kidney concentrations through an adjustment factor based on 1) what is known about concentration of metals in these tissues and 2) relative size of liver or kidney to total body mass (for a default animal such as a cow, I guess).  This would require some literature search time to find the appropriate values to parameterize such an equation.

2. Page 10-10 to 10-11 bioavailability.  The first paragraph of this discussion simply states that bioavailability of chemicals in soil to plants is not known, so a default of 1 is used. This is not true, it is just difficult to find the information.  A good start is:

   Kabata-Pendias, A. and H. Pendias.  2001.  *Trace elements in soils and plants*, 3rd Ed. CRC Press, Boca Raton, FL.  413pp.

   Allen, H.E. (ed.).  1995. *Metal speciation and contamination of soil*.  CRC Press, Boca Raton, FL. 358pp.

   Perhaps it should be restated that these soil bioavailability values are too uncertain to be used, so a default of 1 is included.  This gets around the issue of arguing whether or not data are available.

   The next paragraph has a discussion about how rats are similar to cattle in regard to dietary bioavailability of dioxins.  This argument is hardly convincing. Rats and cattle have very different digestive physiology, with cattle carrying around a very large fermentation vat to first reduce the lignin in ingested plants into digestible protein.  Certainly the bacteria in the rumen influence the dioxin bioavailability in some manner as to differentiate it from that of rats.  Are there really no data available in the literature, or was a search just not done in an effort to save time/money?



US EPA ARCHIVE DOCUMENT

3. Page 10-11 Soil-to-Plant BAFs. Why are soil-to-shoot BAFs in dry weight and soil-to-root BAFs in wet weight? These differences are accounted for later on in the model, but it's not clear why they are different to begin with.

4 Page 10-11 last paragraph – this describes a database developed by RTI on uptake rates of metals from soils into various plant parts in both greenhouse and field studies. How were the data qualified for use? Most importantly, were the measurements made after steady-state conditions were achieved and how was biodilution accounted for? Were soil differences one of the reasons for the differential uptake (see item #2 above)? The document is not clear if the data were taken from the 4 summary documents listed, or if the primary sources (i.e., the papers cited in these documents) were retrieved, critically reviewed, and used as appropriate. Please clarify.

5 Page 10-12 top paragraph. This paragraph explains why uptake factors from sewage sludge (biosolids) studies are not applicable to the current model. However, as I do not have the exposure module available for review, I question whether or not this is completely true. Do any of the potential wildlife habitats include agricultural fields? If so, or if other habitat with high soil organic matter (OM) is available, then these uptake factors might be applicable. The fact that OM binds with many substances (particularly metals and other cationic molecules) is important, as bioavailability will be significantly reduced (as pointed out in the discussion here). Therefore, I suggest basing the argument about whether or not the biosolids uptake factors should be used on an assessment of the likelihood of high organic matter soil being in the area of the waste sites, not simply on the argument that OM reduces bioavailability.

6. Page 10-14 bottom paragraph – it is not intuitively obvious why the air-to-plant biotransfer factors for organics were divided by the empirical correction factor of 100. The discussion that follows in the next sentences suggests the amount of material estimated in plants through use of the model may be from 5 to 40 times too high (as compared to empirical measurements). Therefore, a correction factor of 100 appears to be too great; perhaps 50 would be better?

7. Page 10-17 top paragraph – small mammal uptake factors were used for all vertebrates. This does not seem particularly appropriate, as it is safe to assume that ruminants will differ from small mammals in regard to potential uptake, etc. Given the amount of discussion and data presented earlier in the document on uptake factors for beef and dairy cattle, it seems more logical to extrapolate this information to cervids, ovids, and other bovids than to use the small mammal uptake factors.

8. Page 10-17 2nd paragraph – apparently, a literature search turned up only a single study that measured concentrations of contaminants in amphibians – a 1996 study of tadpoles that measured 5 metals. I suggest that either a more comprehensive literature search be conducted, or this be left as a data gap. Tadpoles are aquatic organisms and their uptake rates are not applicable to terrestrial systems. A quick check of Biosis 1990-2001

US EPA ARCHIVE DOCUMENT

(spending only about 10 min) turned up the following 2 references, and I suspect that a more in depth search will find additional sources of applicable information.

Burger Joanna, Snodgrass Joel. Metal levels in southern leopard frogs from the Savannah River Site: Location and body compartment effects. *Environmental Research*. 86(2). June, 2001. 157-166.

Gilliland Carolyn Duda, Summer Cheryl L. Gillilland Merritt G. Kannan Kurunthachalam. Villeneuve Daniel L. Coady Katherine Kemler. Muzzall Patrick. Mehne Chuck. Giesy John P. Organochlorine insecticides, polychlorinated biphenyls, and metals in water, sediment, and green frogs from southwestern Michigan. *Chemosphere*. 44(3). July, 2001. 327-339.

9. Page 10-18 1st paragraph – this is an excellent discussion about the influence of soil composition on uptake of contaminants. It is as equally applicable to plants as it is to earthworms, and should be explored for application there as well.

The last paragraph on this page is written in the conditional tense, making it unclear whether or not this approach was applied or merely considered. Please clarify.

10. Page 10-19 – In the 2nd line of the paragraph that begins the Invertebrate section, the word "terrestrial" should be replaced by "soil" and the word "flying" by "foliar."

11. The 2nd paragraph in the invertebrate section cites some work conducted by Oak Ridge, but provides no reference to a report. Is this information in the public domain and available for review? It also is not clear if the correlation between BSAFs for aquatic inverts and terrestrial inverts is for the flying (adult) forms of both or for the larval stages? Please clarify.

12. Page 10-22 Invertebrates – there are some data on uptake of contaminants by terrestrial invertebrates in the biosolids (sewage sludge) literature that might be worth exploring as well as the general metals literature. Ma published a series of papers on uptake of metals from soils in various organisms, including earthworms and other invertebrates. Van Gestel and van Straalen also have publications that would be worth reviewing. Steve McGrath has published extensively on the topic of effects of metal contamination on soil invertebrates, and likely has some information applicable to development of uptake factors. In sum, a little more digging through the literature will turn up additional useful information. Here are some of Dr. van Straalen's articles, for example:

Van Brummelen T C [a]. Van Straalen N M. Uptake and elimination of benzo(a)pyrene in the terrestrial isopod Porcellio scaber. *Archives of Environmental Contamination & Toxicology*. 31(2). 1996. 277-285.

Janssen M P M. Ma W C. Van Straalen N M. Biomagnification of metals in terrestrial ecosystems. *Science of the Total Environment*. 0(SUPPL. PART 1). 1993. 511-524.



US EPA ARCHIVE DOCUMENT

Van Brummelen T C. Verweij R A. Van Straalen N M. DETERMINATION OF BENZO-A-PYRENE IN ISOPODS PORCELLIO-SCABER LATR. EXPOSED TO CONTAMINATED FOOD. MEETING ON PHYSIOLOGICAL AND BIOCHEMICAL APPROACHES TO THE TOXICOLOGICAL ASSESSMENT OF ENVIRONMENTAL POLLUTION HELD AT THE 12TH ANNUAL CONFERENCE OF THE EUROPEAN SOCIETY FOR COMPARATIVE PHYSIOLOGY AND BIOCHEMISTRY, UTRECHT, NETHERLANDS, AUGUST 27-31, 1990. Comparative Biochemistry & Physiology - C: *Comparative Pharmacology & Toxicology*. 100 (1-2). 1991. 21-24.

Janssen M P M. Bruins A. De Vries T H. Van Straalen N M. COMPARISON OF CADMIUM KINETICS IN FOUR SOIL ARTHROPOD SPECIES. *Archives of Environmental Contamination & Toxicology*. 20 (3). 1991. 305-312.

Janssen M P M. Joosse E N G. Van Straalen N M. SEASONAL VARIATION IN CONCENTRATION OF CADMIUM IN LITTER ARTHROPODS FROM A METAL CONTAMINATED SITE. *Pedobiologia*. 34 (4). 1990. 257-267.

13. Page 10-22 last paragraph – excellent characterization of the uncertainty! See my comment above for how to resolve this to some extent.

14. Section 3.2.1.1 – this is where the documentation gets particularly confusing. This section talks about the database compilation and processing for Farm Food Chain and Terrestrial Food Web, and this particular subsection is all about cattle. Are we to assume this is only for the Farm Food Chain? If so, where is the comparable discussion for the Terrestrial Food Web? If not, how do cattle fit into the Terrestrial Food Web? And why are only cattle considered? What about sheep and pigs, that are consumed in large amounts by people? Sheep are grazed on pasture as much as cattle are, although pigs do tend to be raised in confinement. This does, however, beg the question on page 10-25 in section 10.3.2.1.2 about where livestock feed comes from. It is assumed that it all originates on the contaminated site, either in a grazing environment (which would be applicable to the Wildlife Food Web module) or for harvest, silage, and confinement feed (not applicable for wildlife).

15. Page 10-26 Section 10.3.2.1.7 – the duration of plant exposure must be highly dependent upon the region in which the plant is growing. Can't the 3MRA model have differential inputs for this variable, that are dependent upon the index variable of Region? This also applies to Section 10.3.2.25 on Page 10-28. Incorporation of regional differences would make the estimates much more realistic.

**Dr. Pastorok:** *Terrestrial Food-Web Module*
Section 1.1
The overview of the module needs more context (although I recognize this context may be provided already in other documents in the series that I have not seen). For example, to what kinds of industrial or agricultural sites and chemicals will this module be applied? The geographic range is presumably the entire United States, which includes many different types of habitats.



US EPA ARCHIVE DOCUMENT

Which kinds of habitats and receptors can be addressed with this module? Are there any habitats and receptors that cannot be adequately addressed? The model is intended to be used in a generic fashion to evaluate exit concentrations, but will there also be a potential use by EPA or responsible parties at individual sites to develop site-specific exit values? What are the receptors for the generic model? And what method was used to select them?

This section jumps into describing the details of the conceptual approach without defining the components of the system being modeled and their relationships. The context of the entire 3MRA model is needed as well as a description of ecological receptor types, environmental media, and processes addressed in the model. A flow diagram of an example modeled system would be helpful.

p. 1-1, 2nd para, last 2 sentences– The text starts to describe some details of what appears to be an interval analysis, a Monte Carlo analysis, or some other approach to quantifying uncertainty. Without further detail, it is unclear what the approach really is. For example, it appears to describe use of a uniform distribution for concentrations of chemicals in prey, but this constraint may not be intended. Later in the document, a uniform distribution is mentioned, but no mention of Monte Carlo analysis or another distributional approach is made. Is an interval approach to be used? Monte Carlo or probability bounds analysis would be better approaches.

p. 1-1, 3rd para –As in the comment just made, I note here that the third paragraph describes modification of exposure algorithms by a home range or area-use factor in a superficial way. If the reader were familiar with such factors, this would be appropriate. However, the naïve reader would most likely not be able to understand the conceptual approach with this limited description. In addition, what about accounting for migration? Some species partition their time into on-site and off-site seasons.

p. 1-3, Section 1.2, 2nd bullet – The reference to "four home range areas" is unclear. Are these home ranges corresponding to four different receptors? Or are they four different kinds of home ranges for each receptor? In either case, how can the number be consistent for each and every habitat? Some of the receptors will not occur in some of the habitat types? The document hasn't explained which receptors are being modeled, so the reference to "four home range areas" at this point makes no sense.

p. 1-3, Section 1.2, order of bullets – If I understand the text, then it makes more sense to change the order of the 2nd and 3rd bullets.

p. 1-3, Section 1.2, 3rd bullet – The reference to "range of potential exposure concentrations" may not be clear. Don't you really want to refer to the distribution? Or maybe you really mean the range and are intending to use only an interval analysis.

p. 1.3, after 2nd set of bullets – What about air concentrations of chemicals? On p. 1-1, the text suggests that the model estimates average air concentrations of chemicals.



US EPA ARCHIVE DOCUMENT

Section 2

p. 2-1 1st assumption – This assumption has absolutely no basis unless you provide a rationale related to the kind of source, the kind of chemicals and the transport, dispersion, and degradation mechanisms involved. The area of influence of a source in terms of ecologically significant concentrations of contaminants could be more than 2 km. Moreover, the assumption that concentrations are zero outside the study area is unwarranted (and irrelevant to the current model structure). Ambient background concentrations should be assumed in areas assumed to be beyond the influence of the source. Otherwise, the risk estimates provided by the model are incremental risks only. Incremental risks would be very difficult to interpret because their ecological significance depends on the risk related to background exposures.

p. 2-1 2nd assumption – EPA should review the literature on plant uptake of chemicals and provide data to support this assumption. For a generic, national-level model, this assumption may be reasonable, but I would like to see some support for it. In addition, if appropriate, it should be made clear that this assumption applies only to plant uptake, not to aerial deposition.

p. 2-1, 3rd assumption – For a conservative analysis, this assumption is reasonable. However, the results of running the model without making this assumption should also be given in an uncertainty analysis when the model is used.

p. 2-1, 4th assumption – The 5-cm depth is too shallow for soil fauna such as earthworms, which can easily burrow to 30 cm or more. Later in the document, plants are identified as receptors. Clearly, the depth of the relevant soil layer for estimating risk to plants depends on the plant type. Trees will have root structures considerably deeper than 10 cm. The model is too simplistic in its treatment of the depth strata of soil. The selection of the depth strata needs to be linked closely to the receptor.

p. 2-1 – What is the depth of the soil horizon assumed for calculations of plant uptake?

p. 2-2, 3rd bullet – Random placement of a single home range is not an appropriate approach for a spatially heterogeneous system. For technically valid approaches, see Clifford et al. (1995) and Mackay et al. (2002).

p. 2-2, 4th bullet – This is not an important limitation because incidental soil ingestion by each receptor accounts for this pathway indirectly. If you were to add in the resuspension/redeposition pathway and include incidental soil ingestion, then you would be double-counting a portion of the exposure.

Section 3

p. 3-1, 1st para –The value for the depth-averaged soil horizon (~ 10 cm) is different from the value cited elsewhere in the document (5 cm). The statement that the surficial soil layer is relevant to foraging animals depends on the receptors. Burrowing mammals would certainly be expected to ingest soil from layers beneath the surface.

US EPA ARCHIVE DOCUMENT

p. 3-2 – Terms need to be defined. What do you mean by "regional watershed"? Depending on the scale, a regional watershed could always be large enough to contain all of the home range of the receptors of interest, thereby making the FracWSi Home Range term always equal to 1. The position or the "i" index in the terms in the equation is different from that in the definition of the terms.

Equations 3-1 thru 3-3 – These equations are poorly related. First, the definitions of the regional and local watersheds are inadequate to determine where these specific variables are parameterized. If the local watershed were not contained within the regional watershed, then the average concentration for the home range would be an area-weighted average of the two concentrations (not a sum). It is suggested that the term "watershed" not be used in this context because although it may be technically correct, it is misleading since the term is usually reserved to describe major drainages. Furthermore, these algorithms can be visualized as a spatially weighted summation is a few personal assumptions are made. However, this would require that the left hand variables in equations 3-1 and 3-2 are not average soil concentrations, but rather spatially weighted proportions. Since they are unobservable, they cannot be defined as "soil concentrations" until equation 3-3 is executed. It is recommended that the equations be reorganized and simplified as follows:

A graphic representation of the home range spatial approach and of the depth-averaged soil strata would also be helpful.

pp. 3-3 ff. – The use of BAFs can lead to substantial artifacts (e.g., treating the tissue-to-soil relationship as a linear function when no relationship exists at all; see Exponent 1998). The text acknowledges that BAFs are a major source of uncertainty. Researchers at Oak Ridge National Laboratory have summarized tissue-soil relationships for earthworms (Sample 1998) and the 3MRA modeling approach should follow their recommendations. The Data Collection document does refer to Sample et al. (1998) as the source for "median uptake factors" for metals in earthworms. EPA should ensure that a linear relationship between tissue and soil is appropriate by examining the information in Sample et al. (1998). In some cases, use of a nonlinear regression developed by Sample et al. (1998) may be appropriate. For other prey groups and plants, EPA should evaluate the data to determine whether a linear relationship (i.e., constant BAF) is appropriate. At the least, the range of concentrations over which a constant BAF is considered applicable should be stated.

Equation 3-5 – The $HabitatFrac_{HomeRange}$ term should not enter into this equation because the degree of overlap between the contaminated area and the home range has already been taken into account in the equations used to determine the average soil concentration.

Equation 3-6 – This equation is unclear. It would help to add a statement after the definition of terms that explains that the plant concentration terms for the individual pathways are expressed in terms of the mass of the entire plant, not in terms of the mass of the pathway-specific plant parts involved directly in the given pathway.



US EPA ARCHIVE DOCUMENT

Equation 3-7 – As written, the plant surface-loss part of the equation and the interception fraction apply only to wet deposition, but they should apply to both wet and dry deposition. The definition of the kpParxxx implies that surface loss applies to both dry and wet deposition. A statement should be added to explain that the m2 term represents a unit of ground surface, not the surface area of the plant. The derivation of the equation is not entirely clear because I don't understand why the term for plant surface loss of particle-bound constituents occurs in the denominator. A reference to another paper that explains the derivation of the equation relative to the mechanisms would help. A similar comment applies to the next few equations.

Equation 3-10 – The need for the empirical correction factor in this equation should be explained. To what term does the empirical correction factor apply? A different correction factor is used in the next equation. Somehow, modeling the transfer then using an empirical correction factor seems unnecessary. If you have empirical data to derive a correction factor, just use the data to estimate an empirical biotransfer factor. These kinds of correction factors and their derivation need to be discussed in detail. Otherwise, it appears that you are just using "fudge factors."

Equation 3-16 – EPA has recently abandoned the use of equilibrium partitioning theory to derive sediment quality guidelines. Isn't the use of equilibrium partitioning theory for soil-water partitioning inconsistent with the EPA position for sediments? If a technical rationale exists for why this theory would apply to soil systems but not sediments, it should be presented here. If no technical justification exists, then an empirical approach should be used in place of partitioning theory. If numerous data gaps exist, then more research should be done.

p. 3-10 – Cooking and cleaning don't apply to ecological receptors.

*Data Collection Section 10.0*
p. 10-16, Section 10.3.1.2.1 - The use of field data for bioaccumulation factors in preference to laboratory data is appropriate.

p. 10-17, top – Because of data gaps, EPA will extrapolate uptake factors for small mammals to other categories of vertebrates. The text acknowledges the substantial uncertainty involved in this extrapolation and calls the extrapolated values "placeholders." It is technically invalid to make these kinds of extrapolations. If EPA is unable to present a good technical rationale for an extrapolation of an uptake factor, then the extrapolation should not be made. Data gaps should be acknowledged and filled. Making up numbers just to fill a data table with "placeholders" is not appropriate. A footnote about a data gap (or a zero value with the meaning "data gap") can be used as a placeholder.

Table 10-6 - The extrapolation of sediment uptake factors for amphibians to soil systems is technically unjustified. Indeed, it seems quite unlikely that uptake by a tadpole in a sediment/water system would be similar to that of an adult amphibian in a terrestrial soil system. Again, data gaps should be acknowledged and filled. The extrapolation of the uptake factors for tadpoles to reptiles is also unjustified.

p. 10-17, earthworms – See comment on pp. 3-3 ff. of main document.



US EPA ARCHIVE DOCUMENT

p. 10-18, Equation 10-9 – See comments on Equation 3-16 of the main document.

p. 10-19, para 4, depuration – If the organism for which the concentration term is being estimated is a prey item, then data for nondepurated organisms should be used. If the concentration term is being estimated for a receptor (and the toxicity reference value in the risk module is expressed as a body or tissue burden), then data for depurated organisms should be used. The text states that uptake factors based on depurated organisms are preferred but does not give a rationale for why this is so. A preference for data for depurated organisms in both cases is not technically justified.

**Dr. Sample:**

*Terrestrial Food Web Module*

1) Page 1-2, 1st para, lines 10-11: how do 'empirically-derived algorithms' differ from 'biouptake data from field or greenhouse studies' differ? In my view they are the same.

2) Page 1-2, para 2: It states that regression methods were used to estimate concentrations in biota. This is not supported by all subsequent text. BCFs and BAFs are always used despite available regression models that have been shown to produce more accurate results. If the goal is to be conservative and a policy decision was made to use BAFs and BCFs preferentially, that should be stated clearly. This would be the location to do that.

3) Page 2-1, 4th bullet: 5 cm is a very shallow averaging depth. Most plants and animals will be exposed to depths greater than this. If a shallow depth was selected to be conservative (i.e., to prevent dilution of chemicals by averaging concentrations over a larger mass of soil associated with a more realistic depth), that is OK - the report simply needs to reflect that policy decision.

4) Page 2-2, 2nd bullet - the point here is unclear - aren't empirical data always preferred? Maybe this limitation should be restated that bioaccumulation data are lacking for many chemical/biota type combinations, requiring the use of default values or estimated BAFs.

5) Page 3-1, Sect 3.1, 1st para: depth of 10 cm is inconsistent with what is stated in Sect. 2.1

6) Page 3-3, Eqn 3-3: this equation does not seem right. These concentrations should not be simply additive. Isn't a weighted average (weighted by the proportional contribution to home range) needed?

7) Page 3-3, Sect. 3.2, 1st para: shouldn't 'invertebrates' be 'terrestrial invertebrates'? Also, I do not like the creation of new terms (i.e., omniverts and herbiverts) that are likely to be confusing to readers.

8) Page 3-4, Eqn 3-4: Note that reliance on BAFs does not account for the non-linear nature of bioaccumulation and will overestimate uptake at high concentrations and underestimate uptake at low concentrations. I would recommend the use of existing log-linear models when they are available.



US EPA ARCHIVE DOCUMENT

9) Page 3-9, footnote 7: I am glad to see that errors in the Travis and Arms models acknowledged and the use of the revised models from the EcoSSLs is considered.

10) Page 3-11, Eqn 3-18: here and elsewhere in report - sources for models need to be cited. This model is from Briggs et al. (1983).

*Section 10.0 Farm Food Chain and Terrestrial Food Web Data*

1) Page 10-1, 1st Para, 3rd line: Inconsistent nomenclature - TFW is TerFW in previous document.

2) Page 10-11, last para before bullets: statement that uptake factors are preferred over regression models for plants is erroneous and misleading. All current data indicate that regression models provide better estimates of bioaccumulation than do uptake factors.

3) Page 10-16, last para: How do omnivorous and herbivorous vertebrates differ from small mammals, birds, and herpetofauna? Aren't they just specialized categories of the latter?

4) Page 10-16, last para: Although Sample et al. (1998) is cited as the source of BAF used for small mammals, all regression models that were recommended for use based on the validation analysis are avoided. Unless a justification for not using them is provided, I recommend using regression model to estimate bioaccumulation whenever they are available.

5) Table 10-6: More information concerning the amphibian BAFs is needed. What is the sample size? Was a BAF distribution reported? If so, provide min and max values. Should state that these BAFs are based on coal ash and may not accurately represent soil.

6) Page 10-18, Eqn 10-9: description of $F_{water}$ is incorrect. This is not the constituent concentration in porewater but rather the fraction of water in worm tissue. BCF should be identified as being wet weight. Values for $F_{water}$ (0.84), $F_{lipid}$ (0.01), and $P_{worm}$ (1 kg/L) should all be provided.

7) Page 10-18, last para: equation for estimating pore-water concentrations from soil concentrations should be provided. In addition, these should be integrates with equation 10-9 to provide the overall model for worm BCFs based on soil.

8) Page 10-19, 1st para, last sentence: although the ORNL report (DOE 1998b) is based mostly on aquatic insects, other aquatic invertebrates are also included in the dataset. This needs to be mentioned and described in the uncertainty section.

9) Page 10-19, 2nd para, 6th sentence: The ORNL report does not indicate any correlation between aquatic and terrestrial insects. Rather the correlation is between aquatic juvenile stages of insects and the emergent adult stages of the same insects. This is very different.



US EPA ARCHIVE DOCUMENT

10) Page 10-20, Section 10.3.1.3: If BAFs are to be relied on, the document needs to point out that uptake is non-linear and BAFs overestimate at high concentrations and underestimate at low concentrations.

11) Page 10-20, Biotransfer Factors: As these models are derived from Travis and Arms, there are questions concerning their validity.

12) Page 10-20, Bioavailable Fraction: also don't know how this relates to conditions in the field, form of chemical, site conditions, etc.

13) Page 10-21, 3rd para: as part of the Benzo(a)pyrene example should consider that PAHs are readily metabolized by many species. BAFs generally do not take this into account.

14) Page 10-22, 1st para: should clarify what the vertebrate categories are.

15) Page 10-22, 2nd para: also need to point out that the uptake data you are using is based on different taxa (i.e., aquatic as opposed to terrestrial), all are juveniles, and database include non-arthropods.

16) Page 10-22, 4th para: should state that amphibian data is likely to overestimate uptake, esp. for reptiles.

17) Appendix 10A:
A) need to include references for all values reported so that they can be verified.
B) It does not appear that Kow-based BAFs for plants and animals were generated and included in this table - parameter values of "1" are listed for almost all organic chemicals. Regardless, the question arises about the utility of the model if many or most of the parameters are simply guestimates for default values.

US EPA ARCHIVE DOCUMENT

US EPA ARCHIVE DOCUMENT

# References

**Dr. Pastorok:**

Clifford, P.A., D.E. Barchers, D.F. Ludwig, R.L. Sielken, J.S. Klingensmith, R.V. Graham, and M.I. Banton. 1995. An approach to quantifying spatial components of exposure for ecological risk assessment. Environ. Toxic. Chem. 14, (5) 895–906.

Exponent. 1998. Review of bioaccumulation methods for fish and shellfish. Prepared for Washington State Department of Ecology, Olympia, Washington. Exponent, Bellevue, Washington. December 1998. 54 pp.

Mackay, C.E., J.A. Colton, and G. Bigham. 2001. Structuring population-based ecological risk assessments in a dynamic landscape. pp. 273-296. In: M.C. Newman, M.H. Roberts, Jr., and R.C. Hale (eds.). Coastal and Estuarine Risk Assessment. Lewis Publishers, Boca Raton, FL.

Sample, B.E., J.J. Beauchamp, R.A. Efroymson, G.W. Suter, II, and T.L. Ashwood. 1998. Development and validation of bioaccumulation models for earthworms. Report No. ES/ER/TM-220. Oak Ridge National Laboratory, Oak Ridge, TN. Prepared for U.S. Department of Energy, Office of Environmental Management, Washington, DC.