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

LAU/WP/LF Source Moduels and Watershed Module
Background and Implementation for the Multimedia, Multipathway, and Multirecptor Risk Assessment (3MRA) for HWIR99 - Draft

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. 17

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

November 12, 1999

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 *LAU/WP/LF Source Modules* and *Watershed Module* 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.

Peer Review Charge
for
LAU/WP/LF Source Modules and Watershed Module

August 1999

Background/History

Source modules were developed for waste piles (WPs), land application units (LAUs), and landfills (LFs), i.e. non-wastewater waste management units (WMUs), to provide estimates of annual average contaminant concentrations in surficial soils and contaminant mass release rates to air and groundwater. These estimates are then used in an integrated, multimedia, multipathway model (FRAMES-HWIR) linking source modules with environmental fate and transport and exposure/risk models. Additionally, LAU and WP source release models have been combined with a local watershed model (a "local watershed" is a runoff sheet-flow-only watershed containing the WP or LAU) to provide estimates of contaminant mass flux rates from runoff and erosion to a downslope waterbody, as well as contaminant concentrations in surficial soils in downslope buffer areas between the WMU and the nearest waterbody.

A soil column model was developed to describe the dynamics of contaminant mass fate and transport within these non-wastewater WMUs and, optionally, in the unsaturated soil below. Because it is applied in all the WMU source models described here, it is referred to as the Generic Soil Column Model (GSCM). Governing equations for the GSCM are similar to those used by Jury et al. (1983, 1990) and Shan and Stevens (1995). However, the analytical solution techniques used by those authors were not considered applicable to the source release models developed here because of the need to consider the periodic addition of contaminant mass and enhanced contaminant mass loss rates in the surficial soil (e.g., due to runoff, erosion, wind and mechanical processes). A new solution technique has been developed for use in HWIR that is computationally efficient and sufficiently flexible to allow consideration of the unique design and operational aspects of each WMU under consideration.

The Watershed Module simulates the effects of indirect chemical contamination resulting from airborne emissions from the source and subsequent deposition on surficial soils throughout the (approximately 2 km) area surrounding the WMU, and runoff, eroded soils, and chemical loadings to waterbodies throughout this area. Once atmospheric deposition has occurred on any particular watershed, the fate and transport processes occurring both within the soil column of the watershed and at its surface are fundamentally similar to those same processes simulated for the LAU and WP in the context of their "local watersheds". Thus, the GSCM and its coupling to the watershed hydrology and surface fate and transport model are also used to simulate each watershed within the area surrounding the WMU.

Charge to Reviewers

We invite comments on any and all portions of the modules. We have made a considerable effort within the documentation to identify and describe all significant assumptions and limitations. Accordingly, we will not repeat those discussions here, but rather refer the reviewer to the appropriate sections. Those sections with explicit discussions of assumptions/limitations are:

- GSCM-specific -- Sections 2.1 and 2.5.
- Source-specific operational aspects -- Section 3.7.2 (WP), Section 3.8.2 (LAU), and Section 4.2 (LF).
- Spatial implementation of the soil erosion model with respect to the sheet-flow assumption and

flow length -- Section 3.3.

- Fixed modeling domain of the GSCM vis a vis burial/erosion processes -- Section 3.4.2.
- Steady-state assumption of the runoff compartment model -- Section 3.4.1.
- Treatment of the inner boundary conditions for multiple soil zones with varying properties (cover/waste, waste/subsoil) for the LF model -- Sections 4.3.1 and 4.4.1.

Finally, we specifically invite comments on the conceptual model and implementation of the coupling of the surface fate and transport model with the GSCM. The particular spatial scale ("local watershed") associated with some of the HWIR exposure and risk issues have, to our knowledge, not been previously addressed by extant models. For example, PRZM deals with field-scale simulations (e.g., our WMU subarea in the local watershed), while HSPF deals with much larger, regional watersheds. Indeed, EPA has reviewed extant models with respect to their applicability to the HWIR spatial scale(s) of interest and found them lacking, or at least too computationally or data intensive. The combined GSCM/surface fate and transport model represent EPA's effort to fill this technological gap and we welcome comments.

Reviewer Comments Summary Report for Source Models for Non-Wastewater Waste Manaegement Units (Land Application Units, Waste Piles, and Landfills)

General Comments:

Dr. Bahe/Dr Wyatt:
Overall the concepts underlying the development of source modules for modeling risks from non-wastewater waste management units (WMU's) is innovative and potentially quite useful for predicting, minimizing, and managing environmental impacts. The combination of a small (aerial) scale model with long term (time) simulation findings is an engaging program. Attempting the integration of WMU source modules with multi-pathway, transport, and exposure modules, thus creating a holistic analytical tool, is desirable but obviously subject to problems. In all the modelers did a good job on basic assumptions for defining LF, LAU and WP units, however, as with all basic assumptions, it precludes use in certain situations.

Upon the completion of our review we returned to the "limitations section" provided with this submittal and noted that the modelers recognize some of the limitations we may elaborate on. Their recognition of the shortfalls or trade-offs, and subsequently their choices in model parameters, further made us uncertain of the ultimate utilization goals and practical applications desired for the completed model. We believe that it would have been more beneficial to our evaluation of the modules (model) if we were provided with a clearer understanding of the specific objectives of the model, including the targeted user group for the model. Further, our review was restricted because we were not given the opportunity to actually test a working version of the model (as opposed to review of theoretical integration).

We both agreed that the most puzzling aspect of evaluating this model was the fact that there is no "model" (software) to evaluate. It would be informative to see an integrated running version, especially considering that the developers have a working version which they used to perform ground-truthing in segments, (i.e., when running the model the developers numerical modeling was not working in their design and they switched to a "quasi-analytical" approach). Also, we recognize that although this model is currently being developed to run on a particular hardware system, it should be ultimately written in a language that would allow for PC applications.

The concepts underlying the development of individual modules (LF, LAU, WP) for modeling environmental risks associated with non-wastewater management units (WMU's) in watersheds are original and are potentially useful. The model developers recognize some of the existing shortfalls in the model and make a solid attempt to explain the rationale for choice. Regardless, we were left unsure about the targeted user groups and the ultimate goals of the model from an applied and practical perspective. It was apparent however that a strong emphasis was placed on "computational efficiency" and the associated systems constraints. Technically, this model may suffice, depending on the desired outcome by EPA, but from an environmental management standpoint both utility and data were inevitably compromised.

In general each of the modules (LAU, WP, and LF) and individual models (GSCM, Watershed) seemed reasonable with respect to function as specified by the limitations on data and inputs. In particular, the GCSM and Watershed models were rooted in existing functional prototypes that were subject to modification for WMU linkage purposes. With a primary goal solely based on computational efficiency it appears that theoretically the model developers have a good protocol.

For multiple users in environmental management the limitations and practical applications pose concern. Though these have been more definitively discussed earlier we feel that the following issues, given sufficient attention, could help to rectify these concerns and provide a model with greater utility and benefits. These emerging issues include:

The use of variable time steps for different data and modules may minimize run time but compromise theconsistency of the data.

When choosing to define "soils" as either waste materials or a series of soils in the WMU subareas it is impractical to assume homogenous conditions and constant infiltration rates. For that matter, assuming that a watershed is homogenous with respect to soil, chemical concentrations, and runoff/erosion does not appear very realistic.

The model is at risk of "accounting" errors, either omitting the losses that occur from pathways not included such as photodegradation and second order losses; or by double counting contaminant transport and fate as a result of efforts to maintain a mass balance across linked models. The latter of these is exemplified by calculating mass back into the top "soil" layer following volatilization and gaseous diffusion loss, with uncertainty as to whether or not wind, leaching, and runoff/erosion events recount the input.

Regardless of the WMU type, second order losses of chemical contaminants need to be modeled in order to apply the outputs to risk evaluations. Likewise, although less complex to model perhaps, using a sheet flow only contaminant transport evaluation in a watershed limits utility of the model. Many watersheds have significant channel and streamflow impacts.

A single cell, homogenous media, no lateral migration model for a landfill simplifies the simulation but many landfills will not fit this condition. Many landfills are required to use specific soil covers and subsoil zones or layers which should be more clearly integrated into the model.

There are many good points and viable scenarios or functions presented throughout the documents. The model developers may be able to prioritize "compromises" based on end user demands and gradually build the model over time. In any event it seems prudent to develop applications that can also be run on a PC.

Dr. Brown:

The document is generally well written and reasonably easy to understand and follow. There are a variety of difficulties with the presentation which need to be improved. Concerns will also be raised about a few of the assumptions and the lack of consistency in the use of symbols in the equations in different sections.

In the title, the reader is never told that HWIR99 stands for the Hazardous Waste Identification Rule, but why 99?

Dr. Innskeep:

The draft documents described above contain detailed descriptions of calculations, assumptions, and limitations of model subroutines used to estimate contaminant fluxes out of waste-piles, landfill application units and landfills. The source models are linked to "local watershed" and "soil column" modules for predicting contaminant fate pathways (volatilization, erosion, surface water runoff, leaching) and respective fluxes. The Thorough documentation is provided for nearly all calculations, and followed by generally insightful comments regarding limitations, computational efficiency and practicality regarding intended use. The reports appear to have accomplished the primary objective of documenting and cataloging model calculations and assumptions.

That said, after several trips through the document, I found myself wanting a clearer presentation. Specifically, I felt as though the scope of the project was poorly defined, making interpretation regarding completeness difficult. Further, it was not entirely clear who was the intended primary audience. Given the presentation style and the sparing use of visuals, I would guess that the audience best suited for the report is other modelers. Perhaps that is the case; it was not explicitly stated. Regardless, I believe the relative ease with which a reader will understand the document could be improved by additional visualization, with clarity of purpose and scope.

Regarding scope, I was struck by the lack of example model simulations, which would seemingly accomplish so much toward the goal of model development, testing and evaluation. In any modeling application, it is worthwhile to show input file formats, anticipated or suggested data sources, example output file format, and sensitivity analysis concerning major model assumptions or sources of data variation. Little effort was expended discussing the potential contaminants of interest and the intended contaminant scope (other than a brief comment about metals versus organics). In addition, model development and model performance are inextricably linked with an assumed or implied temporal and spatial resolution of input parameters. In the report, it is often difficult to assess the intended model resolution and the expected source of input parameters. For example, issues of scale compatibility of input parameters and process description, and the use of average properties to represent homogeneous media, need to be addressed quantitatively and with greater clarity.

To summarize, I believe the report does an excellent job providing documentation for model assumptions and calculations. However, depending on the intended audience, the report would benefit from significant revision. The primary goals would be to (i) improve accessibility to key organizational and conceptual design and (ii) evaluate more thoroughly the effects of model assumptions and input data sources on model output and performance relative to intended use. Specific comments on each module follows; hopefully, the comments will assist in finding avenues for improving the work and the potential impact.

Mr. Norris:

A. Nomenclature - There needs to be a consistent nomenclature for program components. The GSCM is referred to as the Generalized Soil Column Model in the Acknowledgments but as the Generic Soil Column Model throughout the rest of the document.

B. Acronyms - A listing of acronyms used throughout the document should be included in the first part of the document.

C. Target Audience - The use and sometimes over-use of acronyms is prevalent throughout the documents and research materials that are being developed today. The authors of this document need to make sure that the target audience has a clear understanding of the acronyms and abbreviations used so that the product can be properly applied and utilized.

D. Terms - A definition of terms used in the document should be included in addition to the definitions of the symbols and units definitions. I would suggest a separate "definition of terms" section as well as the use of footnotes to indicate the proper definition of the terms as they occur within the document.

Section 1.0 Introduction:

Dr. Bahe/Dr Wyatt:

In the next several pages we are providing comments on each section and/or module. At the end of this comment section we provide a closing discussion, summarizing the elements in need of attention recurring throughout the proposed modeling protocol. Although comments noted in the closing section may seem redundant, they are provided to highlight issues that may deserve prioritization.

The developers of this model made the decision to model a watershed as a single, *homogenous* unit with respect to soil characteristics, chemical concentrations therein, and runoff/erosion. They indicate that in the model, the simulated chemical loads to the waterbody are sole source (aerial deposition only), as is true for buffer area receptors. Further, they indicate that the direct loadings from runoff/erosion are to be simulated in specific source modules, then added to the indirect aerial loads to provide the total surficial soil concentrations. The potential problems with this approach are twofold. First, many chemicals that may be contained in solid wastes managed on land units may be subject to vertical migration through soil and geological profiles within the watershed area - (a factor not accounted for with the omission of spatial chemical concentration gradients). Second, the model is at risk of 'double accounting' the aerial deposits that are subject to further migration within the watershed by erosion or runoff.

It is questionable whether some of the assumptions made concerning the GSCM should hold with “soil” being defined as a porous medium whether it is the actual waste in the WMU or a near-surface soil in the watershed subarea assessed. Soils and waste units have a very different range of characteristics and are certainly subject to differing climatic and temporal impacts. Additional comments on the GSCM are noted later.

We felt that the variable time steps used for different data and modules (i.e., annual averages, daily values, etc.) were selected in order to minimize run time and data limitations, but the result compromises the consistency of the model. Some good points are made as to why it seemed impractical to build annual averages without using daily rainfall/runoff events as a foundation, yet this seems to contradict the often mentioned desire to maintain ‘computational’ efficiency considering the data intensity associated with daily time steps.

Dr. Brown:

Page 1-1, Paragraph 1: I think it would help if we were told up front that the model will be applied to both organic and inorganic constituents.

Page 1-1, Paragraph 2: I think the sentence in parenthesis needs to have “or the soil under a landfill” added to be complete. Also, in this paragraph in the eleventh line we find HWIR, this time without 99, and undefined.

Page 1-1, Third Bullet, Second Arrow: One usually thinks of leaching as advection and not including diffusion. Perhaps it would be better to say “Movement of aqueous phase constituent mass by advection or diffusion .....”

Dr. Innskeep:

Introduction. I assume that the primary objective of this modeling effort is to link the source modules with environmental fate and exposure/risk models. Right from the outset, the scope of the effort is not defined or presented in a manner that makes it obvious to the reader why the report is organized as it is. A schematic could be used to show the intended linkage of source modules and fate models right from the outset. The first paragraph of the introduction should then try to give the entire perspective of the modeling efforts. This schematic should outline the primary sections that will be discussed in the report, and provide a context in which the reader can easily place the various modules and subroutines. The authors should consider reorganization where source modules are discussed first, with the clear connection and linkage of output that is used in local watershed/soil column models. As it now stands, readers are presented first with information regarding fate and transport modeling with not much understanding of how the source modules will interface. It also brings to question why Section 2 is necessary in its current location. It might be preferable to save the Generic Soil Column Model for an Appendix. Can the soil column model be introduced first when integrated with the local watershed module? For reasons that were not apparent, I did not understand why the GSCM was presented first, yet is not used in that form for integration with the local watershed module. The subject of the report (i.e. prediction of contaminant fluxes) is really addressed in Section 3 with inputs from 3.7 and 3.8. It would seem advantageous to move these discussions to an earlier location in the document or at least improve visualization of how the source modules are linked to the local watershed/soil column models.

The last paragraph in the introduction is necessary and should be retained, but as it stands, it doesn't appear consistent with the sections contained in the report. It is stated that "Sections 4 and 5 describe the specifics of the application and integration for the WP and LAU, respectively". "Section 6 describes specifics of the application of the GSCM to the landfill". These statements aren't consistent with current Section headings.

Other: Ironically, I couldn't find HWIR explicitly defined in the document. Define upon first usage in the introduction.

Dr. Munster:

The source models for non-wastewater waste management units (land application units, waste piles and landfills) as presented in the documentation are well described with good detail and references. The concept of modeling waste piles (WPs), land application units (LAUs) and landfills (LFs) as individual modules to provide annual average surface soil concentrations is an valuable innovation. These source emission modules (WPs, LAUs and LFs) are then effectively coupled with both a watershed model and a Generic Soil Column Model (GSCM). The watershed model simulates contaminate transport in solution in surface water runoff or attached to sediment in erosion. The watershed model simulation provides contaminate and erosion rates to down-slope water bodies and buffer areas. The GSCM simulates contaminate transport through the unsaturated soil profile and incorporates advection, diffusion and volatilization transport mechanisms. This system of WP, LAU and LF modules coupled with the watershed surface runoff component and the GSCM soil leaching component provides a comprehensive simulation tool for assessing contaminate transport from these non-wastewater management units (WMUs).

Mr. Norris:

1. The introductory materials are clear and concise.

2. I would suggest moving the definitions included in the text of this section to footnotes to make the

section more readable.

Section 2.0 Generic Soil Column Model:

Dr. Bahe/Dr Wyatt:

2.1 - It is critical to assume that the soil column sorptive capacity does *not* become depleted over the duration of a year, let alone for long term modeling. This is a questionable assumption to make considering the type of risk analysis this model may be used for.

Assuming an infiltration rate constant in space and time for materials such as waste piles or land applied wastes could result in misleading estimates. This assumption does not seem probable when considering that vehicle traffic, loading and removal, tillage, and climatic variables exist. It also does not seem practical to assume one dimensional chemical transport through the soil, especially if "soil" is defined as waste material and/or soils by classification. Similarly, the assumption that the waste or soil material is homogenous likely excludes use of the model for many WMU sites. (In defense, one could suppose the "homogenous" zone and properties "uniform in space and time" could be reasonable if the zone is a single waste type or a consistent soil series known to exist across the entire watershed).

- Users must realize that the model may not be accurate if their particular LAU, WP, of LF is significantly non-homogeneous or consolidated. This is a likely condition for many older waste pile or landfill units.

When utilizing the model, users must recognize that the data they input for the model will be simulated based on annual averages inherent in the model. Input data which may vary on a shorter basis (i.e. daily, weekly, monthly, etc) must still reflect an annual total for that parameter. Users should also understand the nature of the contaminant(s) being modeled to verify that the assumption of reversible linear equilibrium partitioning between adsorbed and dissolved phases (especially in the case of metals) is appropriate. Also LNAPLS and DNAPLS do not appear to be addressed in the assumptions.

The model may not be as effective in coastal regions where significant tidal influences affect groundwater elevations significantly and on a consistent basis.

2.3 - Parameter estimations are being evaluated in accordance with 'soil types' yet the integral model is targeted to waste. This estimation method assumes data for waste will be available much like it is for a soil series.

2.4 - Given the fact that the numerical solution is not effective, the "quasi-analytical" findings should be weighted accordingly. Also the model should be restricted from use in settings where the majority of known contaminants are subject to high V_E or D_E. Also, it is interesting to note that this accepted trade-off enables long-term estimations but elsewhere in the model 'short-term' daily inputs are more desirable (again a question of consistency).

We noticed that the model does not allow for the actual effect of aqueous phase diffusion loss from soil to air, nor loss and transport from the watershed via volatilization. Instead, on the basis of "computational efficiency", this mass is calculated back into the top layer of soil. This concern is also related to comments on chemical aerial deposition mentioned above in the preliminary section discussion. It is possible that this augmentation could result in greater mass than justified in the top layer. In addition, it is assumed that the contaminant mass further concentrates in the surface soil as a result which indicates the model does not allow for (or integrate) the downward migration of contaminants. Would adding a site-related constant into this equation (2.4.1-10) based on local average meteorological conditions be feasible?

The diffusivity calculations are viable but it is questionable as to whether boundaries can be assumed C_T=0, especially when integrating with LAU or complete WMU's. Also, depending on the chemistry of waste compounds assessed, the diffusive losses from the bottom boundary may or may not ultimately become a steady-state transfer, especially with respect changing temperatures.

The model allows for first order losses entirely from the waste unit, but does not account for potential second order compounds and/or losses. This may not always be a valid condition, recognizing that many compounds decay and are transformed into other chemicals, often with greater toxicity (ex. - perchloroethelene to vinyl chloride). Users should be aware of the potential occurrence of secondary compounds (non-parent compounds) in the unit being modeled. Our concern about this omission is expressed across the various modules.

The convective transfer step is variable. Given the inherent model assumption of certain average annual values, it might be helpful to users to have guidelines for incremental data input options that would result in associated, fixed convective transfer steps fir which the model authors determine to be most accurate. This might increase the consistency of the model use, as well as help to reduce the "temporal distortion".

We agree with the limitations stated in section 2.5 of the documents provided. The discussion about shortfalls associated with precipitation, dissolution, geochemistry of the media, etc. in determining the K_d and fate and transport of metals is an issue that should be revisited (very dependent on the ultimate use of the model). The consideration of one contaminant at a time poses a problem because it is known that chemical interactions often effect transportability and persistence in a given media.

- We also question why photodegradation is ignored as a first order loss. It does not appear that the atmospheric gradient of transport has been extensively developed in the model.

Dr. Brown:

Page 2-1: The bullets are set up in pseudo-sentence style. For example: In the first bullet the statement ends with a period. Then we find "where" followed by a list of definitions and no period at the end. This is a problem throughout the text.

Another problem illustrated in this bullet is that the symbols in the equations throughout the text are typically in italics while those listed in the text are in standard font style. Notice that C and t are slanted in the equation but not in the text. Also the b in ρb should be a subscript.

I have checked the units on many of the equations, and they appear to be correct. However, it is peculiar to me that one system of units was not used throughout. For instance below equation 2.1-1 all the volumes are in m^3 except for bulk density which is given in cm^3. It might be best to put everything in m^3 to avoid confusion. Also, perhaps kg should have been used instead of g.

Page 2-1, Last bullet: We are told here that the K_d can be specified for inorganic contaminants. This implies that if we have an inorganic contaminant - say a metal, we can simply follow the equations with the appropriate K_d. That almost works, but then on Page 2-9 in the second last paragraph we are told that zero concentrations are assumed at the upper boundary.

I think that it would be good to make it clear throughout how organic and inorganic contaminants are handled so that there is no confusion.

Page 2-5, Paragraph 3: I think it would be good to show some comparisons between the explicit finite difference method and quasi-analytical solution. This should be done at least in the range where numerical dispersion is not a problem, to give the reader some feel of the validity of the approach.

Page 2-6, Paragraph 3, Line 5: "Thickness" is a better term for *dz* than width.

Page 2-7, Paragraph 1: Similarly, I would use "down" instead of "to the left" and "up" instead of "to the right". In fact, I would turn the figure 90°.

Page 2-10, Last bullet in Item 1: z_{ava} and z_{avb} are not in Appendix C. In fact, there are numerous instances where symbols are not given in the Appendix.

Page 2-10, Third bullet in Item 1: The subscript on K is difficult to read, but looks wrong. It should be K_{sat} not K_{sap}.

Page 2-10, Item 4: Infiltration rate needs to be defined. In soils it means the movement of water through the soil surface, while to a geologist it means the flux of water below the root zone.

Page 2-11, Item 15: Many of the terms here are not found in Appendix C.

Pages 2-10 and 2-11: Many of the numbered items are not written in complete sentences. Here again, this is a matter of style and should be consistent throughout the document.

Page 2-12, Second bullet, Second sentence: The statement that "with metals, the presence of a precipitate is not allowed" is bothersome to me. Precipitation is the major mechanism by which most metals are sorbed, and their movement is retarded in soils. Are you saying that the presence of elemental metals as opposed to metal compounds is not considered?

Page 2-13, First bullet: You make a good point that the outputs are not strictly applicable to individual years.

Dr. Innskeep:

Page 2-1. Generic Soil Column Model. As mentioned above in organizational suggestions, I would be inclined to discuss source modules first followed by soil column and local watershed models. I would summarize all fixed and variable input parameters and suggested data sources in Table format. Although this information is obviously redundant with the text provided, it will add immediate clarity to how the model will be used. Further, linkage to the LAU, WP and/or Landfill source modules should be explicitly shown in the summary Table.

Page 2-2. Define η upon first usage.

Page 2-3. I am not clear how dispersion is addressed in the model. The Convective (or Advective)-Dispersion Equation (CDE) is normally defined in terms of a lumped dispersion coefficient accounting for hydrodynamic dispersion as well as solute diffusion. It is unclear whether the effective diffusivity accounts for or implies dispersion. Based on the estimation techniques provided for D_E, it appears that hydrodynamic dispersion is ignored. Under many circumstances hydrodynamic dispersion is considerably more important than diffusion in soil solutions.

Page 2-4 and 2-5. How is annual average infiltration obtained, and is the parameter fragmented to smaller time steps or does the time step = annual. The method presumably allows for estimation of long-term annual average contaminant fluxes (2-5), but it is unclear based on what is presented, whether water movement is estimated annually or daily. Certainly, episodic daily events yield different patterns of water movement relative to constant daily values. This might be discussed here for clarity.

Page 2-10. The Algorithm is defined here in text, but it would be beneficial to also present this in Figure or flowchart form as was done for source modules.

Page 2-12. Section 2-5. Second sentence in first paragraph is awkward….omit "When applied to metals.." Also, somewhere in this section, it would be worthwhile to comment on limitations of the general approach where annual average infiltration is used as the primary driving variable for contaminant transport. What is the role of preferential flow in contaminant transport under natural conditions? Given expected sources of input data, what limitations might be associated with representativeness of input data to variable landscape positions?

Page 2-13. The solution technique used to solve the mass balance expression seems unnecessary. There are a number of solutions to the CDE and it's various forms that are not particularly inefficient computationally. At a minimum, it would be nice to show that the order of solution does not control the outcome. This should be done with realistic example input data.

Dr. Munster:

The GSCM uses standard contaminate transport equations to partition pollutants into three phases; adsorbed, dissolved and gaseous. The mass balance equation governing mass fate and transport is a typical equation applied to unsaturated soils that incorporates advection, dispersion and decay terms. A quasi-analytical solution technique was adopted to allow for long-term estimation of annual average contaminate transport.

The column is assumed to be an unconsolidated homogeneous porous media that is constant in space. Again, a standard assumption that simplifies the soil column equations and solution techniques. However, this assumption precludes any type of preferential flow paths in the porous media. That is, the model assumes complete mixing of water and contaminate throughout the soil column. In many soils, this is not the case, as preferential flow pathways dominate the transport process. In preferential flow, water, and any contaminates dissolved in the water, bypasses most of the soil matrix (Steenhuis et al.). The complete mixing model does not apply in this case.

Consider a clay soil with a very low saturated hydraulic conductivity (K_{sat}). Very long travel times would be calculated for water to travel through a column packed with clay soil. Even "representative" core samples taken from surface clay soils in the field and tested in the laboratory can be rather impermeable. However, on a larger scale, there are a tremendous number of cracks and fissures that extend throughout a clay soil, even to very great depths (< 20 ft) (Chakka and Munster). These cracks are most noticeable during extended dry periods, especially in soils with high shrink-swell potential. Surface runoff events that occur in cracked clay soils will quickly infiltrate, especially, early in the runoff event. As the initially dry soil begins to wet-up, the cracks tend to close somewhat. However, the "first-flush" of runoff and any contaminates present in the runoff, have already infiltrated, bypassing most of the soil matrix. This reduces or eliminates much of the sorption and degradation that may take place during complete mixing in the soil column.

However, I also understand the necessity to make simplifying assumptions for a watershed scale model. To incorporate preferential flow into a model is extremely difficulty and data intensive. The computation effort is increased by on order of magnitude, or more. However, this simplifying assumption may lead to controversy. This is a risk assessment model that makes a simplifying assumption that precludes a very real risk factor. The risk for contaminates to be quickly transported to the groundwater via preferential pathways. At the very least, I would suggest adding another bullet to section 2.5, Limitations Related to Use of the GSCM, related to the no preferential flow assumption.

Mr. Norris:

Even though the rational for the development and use of the Generic Soil Column Model has been presented in the Introduction Section, a summary paragraph repeating the key points should be included at the beginning of this section to ensure that the reader and ultimate user of this model fully understands why it was developed and why certain assumptions were necessary.

Assumptions: I find no inconsistencies with the assumptions identified with the Generic Soil Column Model (GSCM) as presented in this section.

Section 2.4/Solution Technique:

a. This is a well thought out and developed section. The limitations of the quasi-analytical approach developed for use in HWIR (i.e., the loss of the ability to evaluate short-term trends in concentration and diffusive flux profiles) should be acceptable for this use. This method does indeed seem well suited for this type of analysis.

b. The author might want to consider the inclusion of an appendix with an example of a real world situation to the "Solution Technique" so accurately presented in Section 2.4.

c. This section should be further divided into Subsections 2.4.1 Background, 2.4.1 Description of the Quasi-analytical Approach, 2.4.3 Boundary Conditions and 2.4.4 Algorithm.

4. Section 2.5/Limitations Related to Use of the Generic Soil Column Model (GSCM):

a. The first several sentences of the first bullet of Section 2.5 seem to be disjointed and fragmented. There seems to be several incomplete thoughts within this paragraph. The authors should revisit this section and make sure that the proper information is being presented.

b. Given the extent of current contamination/contaminant levels on sites and the likelihood of commingling of contaminants, further model development to account for this possibility is definitely warranted and should be actively pursued in a future phase of this effort.

c. The order of the solutions proposed (diffusive losses, first order losses and advection) seems to be a logical approach.

d. The assumption made concerning the boundary conditions at the soil/air interface seems to be consistent with and in reasonable agreement with what actually occurs in nature.

Section 3.0 Local Watershed / Soil Column Model (LAU, WP):

Dr. Bahe/Dr Wyatt:

3.1 - Again this section is nicely integrated and the conceptual basis of the model is well defined. The sheet flow only restriction is explained and, due to computational constraints and goals for the model, has merit. From a practical environmental management point of view it seems that the assumptions behind the sheet flow only decision may leave contaminant transport throughout many types of watersheds nationwide unaccounted for. It would be beneficial to create an algorithm for watersheds that are known to have channel and stream flows that also influence WMU subareas, even if it were as simple as a (0) sheet flow only/ (1) sheet flow plus variable. Overall the definition of the WMU effects within the subareas are sufficiently addressed.

3.2.1 - In many watersheds "infiltration" will not ultimately serve as the groundwater recharge source, as is assumed and modeled here. This potential shortfall should be fully described to model users along with estimation of the potential data gaps resulting thereof. It is somewhat confusing to discuss the hydrology model for waste unit subareas in the context of daily soil moisture values for the 'root zone' of the soil (or waste?) column. For buffers in the watershed or vegetated subunits, this makes complete sense – but as a link in the WMU model it does not seem appropriate (unless phytoremediation is a component).

3.2.2 - The assumptions here may make the model unreliable in areas of exaggerated topography and proximate surface water. Fortunately, in the implementation section routing and non-routing procedural methods are stated as available options. This may help gear the model to more widespread watershed applications, especially when considering that the implicit assumption behind the routing method (i.e. - subareas not hydrologically connected and non-sheet flow drainage systems present) likely depicts many urban and farm areas.

3.3 - There are some checks and balances in place throughout the model description, one of which is the use of a "sediment delivery ratio" – noted here to be used to account for eroded soil deposition associated with ditches, gullies, etc. This appears to help balance potential transport concerns relative to a sheet flow only approach as is mentioned earlier.

We believe that method 4 is reasonable in that it ties into the long-term average assumption of time in the Generic Soil Column Model. Future versions of the model would benefit from the ability to input actual terrain conditions (i.e. areas where sheet flow is not a valid assumption) both from calculation of flow, to selecting the best method for storm event data. Again, the only concern again is the shifting of time units between models and the potential loss of data consistency or relevance. It also seems that daily records could become data intensive which contradicts earlier discussions of "computational" objectives.

3.4 - The simplifying assumptions made by the model developers result in the use of a "quasi-dynamic" or "quasis-analytical" approach and the subsequent instantaneous steady-state assumed for each storm event appear valid and reasonable as long as the model remains a tool for small scale areas and long-term simulations. The "Solids in Compartment" section is clean and efficient, with good discussion and questions about known variables, mass fluxes, and transport parameters.

3.4.2 - Some of the inherent assumptions of the GSCM are based on an annual average. It is apparent that erosion of a sub basin will occur during that period of time. The assumption that the starting elevation of a unit is fixed is reasonable given the scale of the model; however, future versions should seriously consider adding an equation that adjusts the starting elevation of the unit based on the calculated volume of eroded material (the mass balance error). Otherwise, this error will become larger at the end of each simulation, and may make it difficult to identify other mass errors not related to erosion. Overall, this section provides a very nice solution to determining erosion and contaminants in run off. It might be worth evaluating the addition of an internal correction for elevation based on erosion since this value is both definable and quantifiable within the model.

The discussions about settling (vs) and resuspension (vr) parameters as processes internal to the subarea, along with burial/erosion reflected as net changes across the subarea is well thought. The only real tricky assumption subject to challenge relates to equations 3.4.1-1 and 3.4.1-2. They state that either can be represented as one equation in vs and vr unknowns, thus if one of these is a known variable the other can be solved. But they also state that (vr) would be difficult to obtain and that (vs) could be assumed as similar to "settling and compaction in sludge thickeners". Assuming similarities with 'sludge thickeners' may not prove indicative across many waste types or soil units. This poses an even greater concern considering this determination of (vs) is intended to be used to predict the resuspension velocity.

Where is equation 3.4.2-5 as referred to on page 3-24? [We assume that this is a typo and should read 3.4.2-5a.]

3.5.1 - Again we observe the assumption for constant infiltration and convection in the GSCM module that is to be linked to the Local Watershed/Soil Column Models (LWSCM). As mentioned in the review comments for GSCM, this seems impractical when considering the likelihood of variability in some wastepiles and LAU's. Consequently the modelers attempt to compensate for this shortfall in the LWSCM thus allowing for annual infiltration variability which ultimately results in irregular intervals for the convection step. These alterations resulting from attempts to integrated across multiple models challenge the 'integration' feasibility of time steps and data inputs.

3.5.3 - Taking leachate flux into account on a short term basis is an important consideration and provides a secondary solution for this module to integrate with the rest of the simulation. We agree with their evaluation of the limitation of this solution technique (i.e., convective transfer steps occur less than 1x/yr). Also, it is unclear as to whether or not this determination was made during preliminary model runs, and whether it is correlated to chemical specific leachate flux (i.e., what chemicals and substrates does this pertain to?).

Future versions of the model might benefit from consistent calculation steps as mentioned in previous comments. If those can be performed on a shorter scale increment, post-processing algorithms might no longer be necessary.

In discussing the outputs to the Vadose Module it is questionable as to whether or not the non-zero LeachFlux yearly time series from a local watershed will result in the exact same time series duration for all local watersheds. Variability in watershed subareas, and the WMU's contained therein would likely alter the time to non-zero LeachFlux. Are there additional assumptions being made that are not articulated in this section?

3.7.2 - This section provides a reasonable and workable set of assumptions and modeling parameters to describe waste piles, given the incredible diversity of sizes and shapes in existence. Though 'workable' it is highly unlikely that waste piles will have a constant height and area for any length of time, let alone across WMU's sites. Mechanical, climatic and temporal effects must be compensated for elsewhere in the model unless generalizations are expected outputs.

Although the model developers state that realistically waste is added/removed from piles in increments, they still opt to model instantaneous refills. They also seem willing to compromise the potential volatile emissions losses that would otherwise occur and be accounted for. Many of the chemicals of environmental concern contained in a waste pile would be subject to periodic volatilization, and perhaps deposition in the watershed area during pile management activities. It seems that these potential pollution events need to be addressed here or elsewhere as an add-on to the model.

The automatic assumption of anaerobic bio-degradation in all subsurface layers of the waste pile might not be accurate in all cases. Certainly contaminant chemistry, pH, moisture conditions, etc will effect the anaerobic and aerobic microbiology and ultimate degradation activity in various layers. Future versions of the model might include a 0/1, aerobic/anaerobic condition switch to allow users to input conditions per layer based on field data.

The assumptions applied to the runoff and erosion processes are reasonable for computational purposes, but this portends unrealistic implications. How often are waste piles constructed and maintained with a top slope equal to the slope of the local watershed? The assumption that no remaining contamination will exist when a waste pile is completely removed indicates that either other bioremedial activities commence in the watershed that are not accounted for or that the half-life of the compounds contained is short in comparison to storage time. We realize that these assumptions are supposed to apply only to the runoff an d erosion pathways, but temporal influences can lead to periodic release of materials in surface layers that should be accounted for in the model. As we commented earlier, it is not apparent from the text but seems likely that, the mass calculated back into the top layer of soil to simulate diffusion from soil to air in the CSCM might affect the accurate calculation of erosion and/or convection based time steps.

3.8.2 - This section provides a reasonable set of assumptions given the diverse nature of land applications units nationwide. When considering that the till zone depth modeled is fairly shallow {(dz) = 0.01 m} for the LAU, the assumption of insignificant effects of waste additions on hydraulic properties is reasonable. As far as first-order loss, volatilization should be included since the depth of till is shallow. If the modeler has a valid reason to omit this pathway it should be clearly articulated to the user as to why and, if at all, where the complete model sufficiently accounts for such potential loss.

Dr. Brown:

Page 3-1, Last line: It would be good to say “For example, in figure 3.1-2a, subarea 2 might be...”

Figure 3.1-1: The divide in this figure is called “drainage divide” while in Figure 3.1-2a it is called “watershed divide”.

Figure 3.1-1: The stream flows to the left in Figure 3.1-1 but to the right in Figure 3.1-2a.

Figure 3.1-2a: Where is the WMU? Better figures would help.

Page 3-5, Last paragraph: It is not appropriate to simply assume that the growing season is June through August. That introduces significant errors in southern parts of the country where the growing season is much longer. The best solution might be to use frost free days as the growing season. There are published maps of frost free days for the United States.

Page 3-6: It would be preferable to have the pseudo-code in a table or box rather than in the text.

Page 3-7: SM needs to be defined here.

Page 3-7: WP is given as % by volume here. However, in Appendix C, it is given as cm. It can only be cm if a depth of soil is defined. The same problem occurs with FC. The units must be dimensionless, otherwise AW is in cm^2.

Page 3-8: The use of Hargreaves equation is a reasonable choice.

Page 3-10, Paragraph 1: The units on SL should be kg/m^2/day to be consistent with most typical units in Appendix C. The hyphen used here and for the units of R_{appl} in Appendix C is poor form.

Page 3-10, Paragraph 3: K values are reported by soil series, not by soil type.

Page 3-11, Paragraph 3: Since method 4 is used here, I don't see the need to spell out the other three in detail.

Several places we slip back and forth between using no sign to indicate multiplication (equations 2.2-1 and 2.2-2a), a dot in the equation (equations 2.4.1-9 and 2.4.1-10), and an asterisk (equation 3.3-1 and item 1 on page 3-12). The equations look like they were put together by a committee. A consistent style should be selected and used throughout.

Page 3-15, Paragraph 2: The second sentence has no verb.

Page 3-16: The units on ρ are incorrect. It needs to be 2.65 g/cm^3 or 2.65 Mg/m^3.

Page 3-17, Paragraph 2, Line 9 and the line after equation 3.4.1-2: I think the word deposition would be better than burial. Same on Page 3-18, Paragraph 2, third last line.

Page 3-18, Paragraph 4: The origin of the 0.05 to 1.0 m/day seems a little strange. Why not apply Stoke's law to some aggregate sizes to determine if this is an appropriate range for soil? Another source of information would be the dredge spoil research efforts at the Waterways Experiment Station in Vicksburg, Mississippi.

Page 3-19, Equation 3.4.1-8 and elsewhere: Why not use ρ_b instead of m_2?

Page 3-20, Last line: "can be expressed"

Page 3-22, Paragraph 2: Again, I suggest using "deposition" instead of "burial".

Page 3-26, Paragraph 2: *j=ae*, for losses due to wind/mechanical activity. Where are these described and how are they quantified?

Page 3-27, Note at bottom of page: It is somewhat bothersome to me to describe this effort as a screening model when the note indicates that the calculations are limited by the memory of modern computers.

Page 3-32, Table 3.6-1, Variable I: Again, the infiltration rate needs to be defined as indicated above.

Page 3-32, Table 3.6-1, Variable J_{lch}: Here it says flux but in Appendix C it says flux at the lower soil boundary.

Page 3-33, Table 3.6-1, Variable C_T: Again, the definition and even the units differ between here and Appendix C.

Page 3-36, End of first bullet: It doesn't seem correct to assume no surface water pathway for metals.

Page 3-38: The illustration is for organic compounds and not metals.

Page 3-39, Paragraph 1: Change to "dry bulk density of the waste".

Page 3-39: We have seen equation 3.8.2-6 before, but every time it looks different. See equation 3.4.1-8 and ή in the GSCM model. Equation 3.8.2-6 surely does not need to be attributed to anyone.

Dr. Innskeep:

Page 3-1. Section 3. The first part of this section is spent introducing the need for source modules. As mentioned in comments above, authors may consider presentation of source modules first, identifying visible linkage to local watershed/soil column models, followed by the more detailed description of the local watershed model.

The figures used to introduce the local watershed/soil column model (Figures 3.1-1, 3.1-2) could be improved to show actual model function, complete with input parameters and primary output. The visuals do not go far enough in assisting the reader with model conceptualization and linkage with source modules.

Page 3-4. Clearly identify the calculation time step. It appears in equation 3.2.1-1 that all calculations are based on daily inputs. This is fine. It would be helpful to reiterate in subsequent sections such as runoff, ET, and recharge what the calculation time step is and how that corresponds with available data for input.

Page 3-12. Method 2. Define and cite PRZM.

Page 3-14. Last paragraph. The comment on "screening level objective" should be introduced earlier as well. It would be worthwhile to make this clear in the Introduction.

Page 3-27. First sentence is confusing. "The infiltration rate is allowed to vary from year to year". Do you mean that the *annual* 'I' is allowed to vary? Won't 'I' actually vary daily, based on the time step used for calculation?

Page 3-27. Sentence under 3.5.2, No.2 beginning "The year the simulation ceases in each local watershed…." is awkward, rewrite.

Page 3-28. Section 3.5.3

It is difficult to imagine that convective transfer steps occur so infrequently based on daily time steps. Does the convective transfer step refer to water or solute? Authors comment that leaching occurs "more-or-less continuously" and that the routine presented here provides a smoother leachate flux over time. It might be nice to point out that leaching fluxes will not generally occur 'more-or-less continuously' unless the infiltration rate is constant in time and space. Leaching events are generally episodic corresponding to water inputs, and in real systems, preferential flow paths may be very important for distributing solute to greater depths than predicted using homogeneous media. Authors may want to discuss this more thoroughly at this point.

Waste-Pile and Land Application Specifics (Section 3.7-3.8). I stated earlier that it may be preferable to outline source modules first. I can see some difficulties in accomplishing this, and it may not improve the report. At a minimum, I would put more focus on linking the source modules with the Local Watershed/Soil Column Model earlier in the report.

Page 3-39. Land Application Unit. Regarding discussion that "waste addition does not affect significantly the hydraulic properties", I can understand that this would be difficult to predict *a priori*. How sensitive is the model to hydraulic properties, and would it likely make a difference based on current inputs. Are source data varied for tilled versus non-tilled soil. If not, what affect does an assumption of constant hydraulic properties of tilled vs. untilled soil have on model output.

Page 3-40. Appropriate depth for soil column surface layer = 1 cm, or for entire till zone? It seems that 1 cm is too small to represent the till zone.

Dr. Munster:

The LAU and WP modules provide annual average contaminate mass flux rates from the top and bottom interfaces of these WMUs. The WMUs are an integral of the "local" watershed and are affected by up-slope runoff and erosion. In turn, the WMUs affect down-slope areas through surface runoff and erosion. The conceptual model for the LAU and WP and the coupling of these WMUs to the watershed is well thought out. This model accurately represents field conditions in two-dimensions (longitudinal and vertical).

The basic hydrology equation (3.2.2-1) is standard and accounts for all of the various hydrologic processes. However, the assumption that frozen precipitation is treated as rainfall is unwarranted. Typically, snowfall events, during subfreezing air temperatures, do not produce any runoff, and should be modeled as such. Other models, such as Soil Water Assessment Tool (SWAT) simulate no runoff during precipitation events with subfreezing air temperatures (Neitsch et al., 1999). This may make a big difference in contaminate transport, especially in northern locations where snow is on the ground for extended periods of the year.

Runoff estimation using the Soil Conservation Service (SCS) curve number procedure is appropriate, especially for rural areas. The curve number procedure was developed for rural areas by the Natural Resource Conservation Service (NRCS, formerly the SCS). Therefore application to urban area needs to be done very carefully. Impervious surfaces, gutters and ditches in urban areas are not easily accounted for in the curve number procedure (Haan et al., 1994).

The Hargreaves potential evapotranspiration (PET) equation for calculating evaporation and transpiration losses from the soil profile is adequate. However, several of the Hargreaves PET equations appear to be in error. I realize that there are many forms that these equations can take, and I didn't have time to research the discrepancies throughly. So the equations that I suggest may, in fact, be equivalent to the equations in the Source Models document. The standard Hargreaves equations from the American Society of Civil Engineers (ASCE) manual on Evapotranspiration and Irrigation Water Requirements (1990) provides the following equations:

$$PET = [0.0023 S_o \Delta_T^{0.5}(T + 17.8)] / \lambda \qquad (3.2.3\text{-}5)$$

where $\lambda = 2.501 - 0.002361(T)$ MJ/kg

$$S_o = 37.586 d_r(\omega_s \sin\phi \sin\theta + \cos\phi \cos\theta \sin\omega_s) \qquad (3.2.3\text{-}6)$$

$$\theta = 0.4093 \sin[(2\pi(284 + J) / 365] \qquad (3.2.3\text{-}9)$$

The infiltration equation has a feedback loop that increases the amount of surface runoff in the event infiltration exceeds the moisture capacity of the top soil layer. Previously, I had stated that a limitation of the GSCM was the inability to account for preferential flow paths in the soil column. There may be a way to bypass this feedback loop for soils that are known to exhibit preferential flow paths. That is, instead of transferring this water to surface runoff, somehow, maintain this water in the soil. Perhaps by allocating it to some of the deeper soil layer in the GSCM. If this solution is not feasible, then perhaps some other modeling mechanism could be utilized.

Soil erosion is based on a modified universal soil loss equation (MUSLE). The equations all seem appropriate. However, I believe that there is a misprint on page 3-13, the second line from the bottom. The value 30.5 m does not seem appropriate for the 0.5% slope. Perhaps it should be 305 m instead.

The conceptual model for chemical fate and transport (section 3.4) is very logical and comprehensive. The runoff compartment model incorporates diffusion, settling, re-suspension and burial and erosion. Contaminate transport in the runoff accounts for both dissolved and adsorbed pollutants. A lot of effort has gone into simulating sediment transport. Even the preferential erosion of finer soils is taken into account. Again, maybe an effort to incorporate preferential flow of water in the soil column would be appropriate also.

The assumption in section 3.5.1 that infiltration is constant in the GSCM and that convection events occur at regular intervals throughout the entire simulation is at the heart of the preferential flow issue that I've raised. It is my contention that, for many soils, infiltration is not a constant and the transport of contaminates does not occur at regular intervals. Infiltration and contaminate transport occurs primarily during and immediately after rainfall events (Lin and McInnes). A more realistic approach would be to permit higher infiltration rates during and immediately following a rainfall event. This would increase solute velocity (V_e) in the soil and result in faster transport of water and contaminate through the soil than an average infiltration rate and an average V_e rate. In turn, this would affect (maybe significantly) contaminate loading to the groundwater. For example, if a watershed experienced four major rainfall events in one year, and they all occurred one month, water and pollutants would be transported through the soil column very quickly. The contaminates may even enter the groundwater system if a shallow groundwater table was present. On the other hand, if those same four rainfall events were spread over an entire year with an average infiltration rate, the contaminates would not be transported as far through the soil column and maybe not into the groundwater system. Additionally, increased contaminate biodegradation may occur with longer residence times in the soil. Slower contaminate transport is definitely not a conservative prediction of pollution potential.

The leachate flux post-processing algorithm seems very artificial to me. The need for this algorithm seems to be a consequence of using the average infiltration rate. If a more realistic infiltration process was employed, than this post-processing algorithm would probably not be necessary.

The waste pile model is very comprehensive and well-thought out. The assumptions of insignificant side slope and top slope equal to the average watershed slope does not represent tall, rounded piles. However, simplifying assumptions must be made for all models, and these assumptions are adequate for most cases.

I have some serious concerns regarding the assumptions about the land application unit conceptual model. It is unclear to me whether a liquid waste can be applied in the absence of a soil waste fraction. Many manufacturing and agricultural operations dispose of liquid waste using land application. A typical agricultural land application practice would be to irrigate effluent from a manure waste treatment lagoon directly onto pasture land (Aldrich et al.). There is no tilling that takes place, as stated in the first LAU assumption. There is no bulk density or organic carbon fraction for the liquid waste as stated in the third assumption (p. 3-38). In the case of a pure liquid waste, equations 3.8.2-1 to 3.8.2-4 do not apply. The assumption that "the contaminate mass is concentrated in the solids portion of the waste" is null and void. This is a serious limitation that would preclude this model from being applied to many of the land application processes employed by agriculture and industry.

Mr. Norris:

Section 3.1/Introduction:

a. The numerous acronyms should be spelled out the first time that they are used in this section.

b. From a watershed perspective, yes these waste management units are integral to land area on which they are located. In fact as indicated they will or may have an impact on the watershed area in which they are located. Therefore, a holistic modeling approach is a proper one.

c. I would recommend rewording the third sentence of the first paragraph to read: "Indeed, after some period of time, during which runoff and erosion has occurred from a waste management unit (WMU), the downslope land areas will have been contaminated and their surface concentrations could approach or even exceed the residual chemical concentrations in the WMU."

d. The definition of "local watershed" could be included as a footnote to this page in order to help the flow of the text.

e. The limitation of a "local watershed" to that area in which runoff from the Waste Management Unit (WMU) occurs only as overland flow (sheet flow) is an appropriate one. This will by definition limit the size of the "local watershed" to more manageable units. All the literature that I am familiar with indicates that once sheet flow from any source approaches or exceeds 100 feet (depending on the slope, the type of cover and the erodibility of the soil) it becomes rill erosion and starts making defined channels.

f. The figures presented in this section are clear and concise.

Section 3.2/Hydrology:

a. The simulation of watershed runoff and groundwater recharge is appropriate.

b. Additional identification of Subsections might be appropriate in Section 3.2.2 (i.e., 3.2.2.1

Governing Equations and 3.2.2.2 Implementation).

c. The daily runoff method identified here is appropriate for rural areas and is widely accepted. Have the authors considered the use of this methodology in more urban or suburban settings? More and more waste management units are appearing in these more urbanized settings. It would be interesting the see a comparison of the use of the USGS Regression Analysis (Evaluation and Management of Highway Runoff Water Quality, Publication No. FHWA-PD-96-032, U.S. Department of Transportation, Federal Highway Administration, June 1996) versus the SCS method for determining runoff from these waste management units.

d. I concur with the use of the Hargreaves equation to determine daily evapotranspiration; it is less demanding and is better suited for use on the sites being examined.

Section 3.3/Soil Erosion:

a. Additional Subsections might be appropriate in Section 3.3 Soil Erosion (i.e., 3.3.1 General, 3.3.2 MUSLE Implementation, and 3.3.3 Spatial Implementation).

b. The Universal Soil Loss Equation (USLE) is an appropriate tool on which to base the Soil Erosion Model. The USLE or the MUSLE ("Modified" Universal Soil Loss Equation) has been used to successfully identify and manage soil loss in a number of rural and urbanizing localities through Erosion Control Ordinances (i.e., Albemarle County, Virginia).

c. It would be useful to include Figure 3.1 – 1 again (maybe as a separate and new figure) on or near page 3-13 since it is referenced there.

d. The fourth sentence of the first full paragraph of Section 3.3.3 Spatial Implementation on Page 3-13 should be slightly reworded for clarity. I would suggest the following: "As part of the HWIR data collection process, the delineation of the sheet-flow-only 'local watershed' is accomplished by a Geographical Information System (GIS) analysis. A key component of this analysis is the correct generation of the water body network such that the water body delineated as lying downslope of the 'local watershed' is in fact the first 'defined drainage channel' that the runoff would encounter."

e. The 700,000-m2 upper bound does present ample opportunity for sheet flow to evolve into erosive channel flow. My experience shows that flows exceeding 100 feet (depending on soil conditions, cover and slope) may develop into small-defined channels and start being an erosive force. I concur with the analysis that dividing the "local watershed" into smaller areas may be appealing and would be appropriate for a more complex evaluation. However, limiting the flow distance to a reasonable maximum may be more appropriate for the HWIR's screening level objective. For the purposes of this model, the maintenance of mass balance for the "local watershed" is of prime importance. Areas approaching this reasonable maximum should be identified as potential problem areas and marked for further evaluation at some date in the future.

Section 3.4/Chemical Fate and Transport:

a. Additional subsections might be appropriate for Section 3.4.1/Runoff Compartment (i.e., 3.4.1.1 Introduction; 3.4.1.2 Solids in Runoff Compartment; 3.4.1.3 Solids in Soil Compartment and 3.4.1.4 Contaminant in Runoff Compartment).

b. The rational for the assumption of instantaneous steady state for solids and chemical concentrations in runoff is appropriate.

c. There is a word missing in the second sentence of the last paragraph of subsection 3.4.1.1 Introduction. The sentence should read: "Development of mass balance equations for solid(s) and chemical concentrations follow."

d. The exclusion of hydrolysis, volatilization, and biodegradation processes from the runoff compartment is appropriate.

f. The rational presented for the transport of solids in the soil compartment and the subsequent use of the solids mass balance equations to determine the burial/erosion and resuspension parameters is sound and is presented in a logical progression.

g. The definition of "finer soil particles" might be more appropriate as a footnote on Page 3-21.

h. The logic and flow of the materials presented in Section 3.4.2 Soil Compartment appears to be fine. However, it appears that the materials presented on page 3-22 on the minor mass balance error created by the burial/erosion mechanism would be better presented as a footnote explanation.

i. The further refinement of the Generic Soil Column Model (GSCM) to accommodate the minor mass balance error of the burial/erosion mechanism should be part of a future effort that has a greater scope.

Section 3.5/Implementation:

a. The first paragraph of Section 3.5.1 Overview should reference Figure 3.5-1 as Figures 3.5-1a and 3.5-1b. In addition, an effort should be made to locate these figures closer to the reference location.

b. The use of the TermFrac criterion or 200 years, whichever comes first, as the determinant for the length of the "time series" seems to be logical.

c. I would recommend the placement of the note on the computer memory requirements currently in the text on page 3-27 in a footnote.

d. A space needs to be added in the next to the last sentence of the first paragraph in Section 3.5.3 Leachate Flux Processing. The space needs to be added before…"is the average infiltration rate between years a and b."

Section 3.6/Output Summary

a. It appears that all of the outputs have been included in the summary.

Section 3.7/Waste Pile (WP) Model Specifics

a. Figure 3.7.1-1. Illustration of a Waste Pile in a "Local Watershed" could be moved into Section 3.7.1 Introduction. Then Section 3.7.2 Additional Assumptions would not have to be broken by the insertion of the figure in that section.

b. The explanation of the addition and removal of waste incrementally is better suited for placement in a footnote.

c. Although the waste pile model may underestimate volatile losses it will still provide a fairly good estimate of these losses and is appropriate for this scale of modeling effort.

d. The assumption that there is no subsequent runoff/erosion transport pathway from the waste pile subarea and the assumption that there is no remaining contamination in the subarea that contained the waste pile may not apply to all real world sites. I understand the limitations imposed by the models being used and concur that if in fact no surface runoff pathway exists then there will be no surface runoff.

e. The logic and sequence of the processes outlined for Waste Piles is consistent with use in a screening level effort.

Section 3.8/Land Application Unit

a. Figure 3.8.1-1. Illustration of the Land Application Unit (LAU) in the "Local Watershed" should not be placed in such a manner as to break up the following additional assumptions section.

b. The second sentence of the third full bullet on Page 3-39 should read: "Thus, the hydraulic properties of the soil (K_{sat}, SM_b) are used in Equation 2.3-1 to determine the water content <u>of</u> the till zone."

c. I agree with the assumptions and the logic presented for the Land Application Unit.

Section 4.0 Landfill Model:

Dr. Bahe/Dr Wyatt:

The computational efficiency gained from implementing a single cell simulation makes sense with the exception of lateral cell impacts that certainly exist in older landfills and/or those located in more expansive areas with shallow groundwater tables. The conceptual design of the data grid, based on filling the landfill units in increments, is clear and reasonable based on the long term scale of the model. Although many of the assumptions will not be valid in all cases they appear reasonable for providing a model platform, especially for landfills constructed in the late 1970's and throughout the 1980's.

There does not seem to a provision for older landfills with more variable fill histories. Future versions of the model might focus on allowing for spatial diversity of parameters within units. The current model will not pertain to many newer landfills utilizing more modern liners. Also, it seems impractical to omit simulations for specified soil cover and subsoil zones, as this is often a regulatory and/or funding requirement in many states nationwide. Depending on the type of waste managed in a landfill unit, by choice, the subsoil zone and cover layers may *not* be homogenous with the cell media.

It is good that the modelers note incoming HWIR99 waste, specified to have a constant contaminant concentration, can be adjusted in accordance with other wastes added to the cell lacking the contaminant of interest. The question is, how are variable contaminant interactions influencing the chemical form and concentration of those contaminants of 'interest'? Are there assumptions concerning chemical interactions in the waste profiles that account for such potential fluctuations that simply have not been articulated in the document?

A concern stated earlier in this summary arises again for this model, that is the lack of second-order chemical and biological fate and transport simulations. Landfills, even more so than LAU's, contain many chemical by-products of degradation and contaminant mixing. Also, the constant infiltration rate assumption remains questionable here as in the other modules previously discussed.

For existing landfills already comprised of several "units" (i.e. older landfills, non-homogeneous landfills, etc.), it would be beneficial if the model could accept the total unit "to date" as the first simulation unit. Otherwise, older landfills will have to be simulated as they were constructed historically. This process would probably introduce greater error than making a unit assumption for the existing conditions. Future units could then be layered or added on in a predictive and reliable manner.

The discussion on boundary conditions is sensible, especially since the user can specify the boundary condition multiplier and subsequent condition to be simulated. This gives more flexibility to the model than would otherwise exist.

4.4 - The modelers note that they are aware the current model may over estimate both contaminant mass losses from the waste zone into the cover zone, and thus volatile emissions. The advective transfer of the contaminant mass allowed from the cover soil to the waste mass could provide a balance in the model estimations. This is an example of a situation where actually running different scenarios would help to determine the over estimation risks.

4.6.2 - Although computer memory constraints limit the existing model to 200 years, future attention should be given to expanding that limit (especially in the case of metals). Further, the model developers should consider developing the model for use as a PC application.

Dr. Brown:

Page 4-1, Last bullet: It seems a little odd that no liner is being considered in the landfill model. I recognize this is a conservative approach, but in this day and age it is not realistic. We have relatively good data on leakage and diffusion through liners. A paper I did on the topic is attached.

Page 4-4, Paragraph 3: It bothers me that diffusive flux is not considered. In some cases it is the major mechanism of flux.

Page 4-4, Paragraph 3, Last sentence: This sentence does not apply to metals and other inorganic contaminants.

Dr. Innskeep:

Page 4-1. Make it clear in the introduction here and in the main introduction whether the landfill model serves as a source model for the Local Watershed/Soil Column model.

Page 4-1. Assumptions. It seems potentially problematic to assume that landfill waste zone has homogeneous properties uniform in space and time. Given the type of waste that will be incorporated into the landfill waste zone, it seems problematic to assume that this zone will be homogeneous. The potential for preferential flow paths in mixed solid waste environments appears too great to warrant an assumption of homogeneous porous media in this zone. Could the authors provide some justification for this assumption, or discuss more thoroughly what the properties of this zone might actually be?

Page 4-2. Last bullet. How are concentrations of contaminant in landfill waste determined? What types of contaminants can be accurately modeled with this approach, and how do sorption properties impact solute transport (are these the same as described in the local watershed/soil column model?)

Page 4-3. Use of anaerobic rate constants may be problematic in cases where aerobic conditions prevail. Is it safe to assume that all zones in landfill will be anaerobic?

Dr. Munster:

The landfill conceptional model is a very accurate representation of actual conditions. I see no major omissions or issues to comment on. The landfill model is very well done.

Mr. Norris:

Section 4.1/Introduction

a. The introduction is clear and concise.

Section 4.2/Additional Assumptions

a. The assumptions identified are logical and seem to accurately reflect what may occur in real world situations.

b. I hope that the decision to not include a liner in the Landfill Model for HWIR99 purposes will be revisited, if not in this effort, at least for a future effort. The use of liners in landfills today is a state of the art solid waste management technique and is prolific throughout the industry. The only problem may be determining what thickness or style of liner to use in the model.

Section 4.3/Landfill Cell Simulation -- First Year

a. Additional subsection numbering should be added to Section 4.3.1 (4.3.1.1 Upper and Lower Boundaries and 4.3.1.2 Inner Waste/Subsoil Boundary).

Section 4.4/Landfill Cell Simulation -- After First Year

a. Additional subsection numbering should be added to Section 4.4.1 Boundary Conditions (4.4.1.1 Upper and Lower Boundaries; 4.4.1.2 Inner Waste/Subsoil Boundary; and 4.4.1.3 Inner Cover Soil/Waste Boundary).

Section 4.5/Calculation of Landfill Results

a. More detail is needed to flesh out this section. The summary that is presented is short and to the point, but I fear does not do the effort justice. A summary example might be appropriate.

Section 4.6/Implementation Algorithm

a. Figure 4.6-1 should be identified as Figure 4.6.1a. Landfill Model Flowchart for an Active Cell (Year 1) and Figure 4.6.1b. The Landfill Model Flowchart for a Closed Cell (Year 2+)

on page 4-6.

b. Figures 4.6-1a and 1b should be included immediately adjacent to Section 4.6.1/Overview if possible.

c. The algorithm as presented seems to be logical and the assumptions made are appropriate for the screening level model.

Section 4.7/Output Summary

a. It appears that all of the outputs have been included in the summary.

Section 5.0 References:

Dr. Brown:

The references need considerable work. There is no consistent style. It would be best to put the date in parenthesis after the author's name. I have not gone through them in detail but just a glance indicates many inconsistencies.

Dr. Innskeep:

Authors cite but don't include reference for Clapp and Hornberger, or for Penman, 1948.

Inconsistent spelling for "Stevens" in reference by Shan and Stephens, 1995.

Cite reference for PRZM.

Appendices:

Dr. Brown:

Page A-9, Last line: < 2.5 µm

Page A-15: The references here look much better.

Appendix C: Many symbols used in the text are missing.

Page C-3, Symbol ρ_b: the "2" in M2 should be subscripted

Page C-3, Symbol $\rho_{b,w}^{wet}$: Why the "wet"? On page 3-38 there is no superscript "wet" used.

Page C-3, Symbol *bcm*: Lower soil column, not coil

Page C-4, Symbol *dz*: Replace the word size with "thickness".

Page C-5, Symbol Fc_i: This term is unitless in the text but here it is defined as having units of cm.

Page C-6, Symbol M_i: Why not give subscripts instead of using bullets (squares).

Dr. Munster:

Appendix A: Particulate Emission Equations:
Not qualified to comment.

Appendix B: Determination H', Da, and D_w for organic compounds:
These are standard equations that apply to organic compounds and appear to be correct.

Appendix C: Symbols, Units, and Definitions:

There are numerous symbols referenced in the text that are not listed in appendix C. Just one example would be M_{lcha}. This is presented in the text on page 3-26 but is not found in appendix C. Many other variables are similarly missing from appendix C.

Mr. Norris:

Appendix A/Particulate Emission Equations

Section A.1/Introduction

a. It appears that all of the available release mechanisms have been considered and are included as part of the consideration for each of the Waste Management Units.

Section A.2/Particulate Emission Rate ($E30_I$) Algorithms and Particulate Size Range Mass Fractions

a. Additional subsections might be appropriate in Section A.2.1/Wind Erosion from Open Fields [$E30_{wd}$] (A.2.1.1 Step 1: Calculate U_{*t}; A.2.1.2 Step 2: Calculate U_t; A.2.1.3 Step 3: Calculate $E30_{wd}$ and A.2.1.4 Step 4: Apply Particle Size Range Mass Fractions).

b. There is an extra page (Page A-7) in the document that should be eliminated.

c. The definition of total suspended particulate matter (TSP) should be included as a footnote on Page A-9 in Section A.2.2.

d. I concur with the assumptions identified and outlined in this section.

Section A.3/Particle Size Range Mass Fractions for the Total PM30 Emission Rate

a. The term Pm30 in the title for this section should be PM30.

Section A.4/Annual Average Constituent Emission Rate (CE30) Equations

a. The term Ce30 in the title of this section should be CE30.

b. Equation 2.4.1-12 referred to here should be repeated here for clarity.

Appendix B/Determination H', D_a, and D_w for Organic Compounds

1. General Comments
    a. The assumptions and procedures identified in this section appear to be appropriate.
    b. A separate reference section (Section B.5) should be added.

Appendix C/Symbols, Units, and Definitions

1. General Comments
    a. The break points in the chart of Symbols, Units, and Definitions need to be checked, there are several continuation points in the table that should be reconciled.

Reviewer Comments Summary Report for Watershed Module

General Comments:

Dr. Bahe/Dr Wyatt:

It appears that the watershed module is fairly straightforward and based on commonly used modeling parameters. We feel that as long as the drainage basin is homogeneous with respect to soil characteristics, runoff and erosion characteristics, and chemical concentrations, this model framework appears reasonable. As was noted for the GSCM and WMU models, we remain uncertain about the practical applications of a model that links modules using very different time steps (daily, as a function of annual averages, etc.).

The transferability of model applications is somewhat challenged by the consideration of each watershed as a single, homogenous area. Many of the parameters important in estimating environmental impacts (erosion and runoff, flow, chemical and nutrient transport, atmospheric deposition, vegetation, intrusion, etc.) are highly variable within and among U.S. watersheds. Similarly, baseflow determinations are critical to successful use of the model and one important question, posed in section 3.0, is – "what single flow statistic best represents annual average baseflow for a given region, reach order, and year?". Although it appears that a well thought algorithm was derived, some of the required inputs developed specifically for this model (as part of the HWIR effort) are not readily available from traditional data sources. This may or may not prove to be problematic.

We recommend that future versions should allow for spatial variability of soil characteristics, runoff and erosion characteristics, and chemical distribution. This is critical, in that many important drainage basins cover large areas and include varied combinations and types of developed and undeveloped land.

Dr. Brown:

The module is well written and easy to understand and follow. The technical approach of using local watersheds appears appropriate.

There are several points that need clarification and perhaps a bit more thought. These include:

The reader needs to be told what HWIR stands for and the significance of 99 in the title. This is jargon which obviously the authors are very familiar with, and thus, did not tell us that it stands for the Hazardous Waste Identification Rule. I thought it was published Nov. 19, 1998, or is it that the effective date is in 1999?

Dr Munster:

The source models for non-wastewater waste management units (LAUs, WPs and LFs) and the watershed module are very well done. Conceptually, these models very accurately represent the actual, "real world" conditions. The governing equations for the models are based on well established scientific principles.

This watershed scale model fills the gap between field scale models and regional scale models very well. However, some of the simplifying assumptions that are required for a watershed scale model may lead to non-conservative simulations of contaminate transport. The assumption of complete soil and water mixing and a constant average annual infiltration rate precludes preferential flow and rapid transport of contaminates through the soil profile. This is a serious tradeoff that needs to be addressed in the risk assessment model or clearly stated in the assumptions.

The model is not able to simulate purely liquid waste applications to the land. This is a serious omission that needs to be corrected. Many agricultural and industrial land application practices could not be simulated by this model.

Several other minor issues were addressed in the review. However, on the whole, this model is very well thought out and well documented. I think that it will be a very valuable tool for assessing risks at non-wastewater waste management units.

Mr. Norris:
A. **Acronyms -** A listing of acronyms used throughout the document should be included in the first part of the document.

B. **Target Audience -** The use and sometimes over-use of acronyms is prevalent throughout the documents and research materials that are being developed today. The authors of this document need to make sure that the target audience has a clear understanding of the acronyms and abbreviations used so that the model can be properly applied and utilized.

C. **Terms -** A definition of terms used in the document should be included in addition to the definitions of the symbols and units definitions. I would suggest a separate "definition of terms" section as well as the use of footnotes to indicate the proper definition and use of terms as they occur within the document.

Section 1.0 Introduction

Dr. Brown:
Page 1-1, Paragraph 1, Line 1: Volatile emission is mentioned as being a release mechanism, yet equation 2.1-1 does not include a term to account for vapor loss from the soil. In the same line, WMU needs to be defined.

Note that in line 6 of the same paragraph, volatilization is mentioned as one of the simulated processes.

Page 1-1, Paragraph 2, Line 3: There are different meanings to both the words infiltration and recharge. To the soil scientist infiltration means the flux at the surface of the soil. To the geologist both words mean the flux toward the water table at the bottom of the root zone, as infiltration to the aquifer or recharge to the aquifer. Thus, these terms need to be defined here to avoid confusion.

Page 1-1, Paragraph 2, bullets: First, although the statements start with a capialized word and end with a period, they are not sentences; secondly, the colon indicates they should not be capitalized. This is a matter of style. Finally, the order seems a bit strange to me. The second one - chemical loading should be the last one, and the others should also be put in an order which logically leads to the stream discharge.

In the first bullet I understand that volatile losses from the watershed are lost and don't need to be considered, but in the first paragraph you say leaching is simulated in the Watershed module, now we are told that "leaching from the soil column..... are not subsequently viewed as input to the Air or Vadose Modules", or is it that leaching is recalculated in the soil column model?

In the last bullet we again encounter infiltration (recharge).

Page 1-3: Again bullets don't fit into the sentence, but here most of them are sentences.

Page 1-3: In the second bullet there is a list of parameters k_{ev} ... m, which need to be defined

Dr. Innskeep:

Page 1-1. Introduction. The purpose of this module is to presumably predict fate of chemical mass released from Waste Management Units (WMU) in the form of either volatile or particulate emissions. The Introduction states that the watershed is modeled as a single homogeneous area with respect to soil characteristics, runoff and erosion characteristics and chemical concentrations in soil. Without examples discussing limitations inherent in this approach, it appears too simplistic to be of real value. If indeed, soil, runoff and erosion characteristics are taken as averages across the watershed area, then the model overlooks the fundamental processes responsible for such fate pathways. For example, in real watersheds, erosion and leaching events are likely distributed temporally and spatially in response to locally variable conditions.

One of the purposes of the Watershed Module is to simulate the time series of annual average chemical concentrations resulting from aerial deposition in the area of interest (AOI). How is the deposition itself modeled? How do we know the amount deposited in the AOI.

Page 1-2 and 1-3. Although I can appreciate the arguments provided to justify using a longer time step in the Watershed Module compared to the Local Watershed/Soil Column Model, the authors should provide some discussion of limitations of using annual average data to estimate fate of contaminants reaching off-site locations via aerial deposition. The remainder of this module is essentially identical to that described for the local watershed/soil column module.

Dr. Munster:

The watershed module simulates the fate and transport of contaminates placed on the soil surface by aerial deposition. The processes simulated include volatilization, leaching, runoff, erosion and biological and/or chemical degradation. Erosion, runoff and contaminates are transport to adjacent surface water bodies. The watershed simulated by the module is assumed to be homogeneous with respect to soil, runoff and erosion characteristics as well as chemical concentrations in the soil.

A primary assumption for the watershed model is that contaminate concentrations in the soil due to aerial deposition will be significantly less than soil concentrations resulting from direct runoff from a source. This assumption is used as justification for a large time step (up to one year) in the GSCM. In fact, it is clearly stated in the assumptions, that this large time step is based on numerical considerations, not physical considerations. However, it is not difficult to conceive of a situation where a highly toxic compound is deposited on the soil surface. In this case, even very low concentrations of this compound may pose a risk to groundwater supplies. It may be controversial for a risk assessment model to discount this consideration.

Mr. Norris:

a. WMU, LAU and WP should all be spelled out, at least the first time that they are used in this document.

b. A definition of watershed should be included as a footnote to this section.

c. This section is very well constructed and supplies some very clear and concise information on the workings of the Watershed Model and its interaction with the Land Application and Waste Pile Modules.

d. The note included in the first bullet on Page 1-1 should be inserted as a footnote.

Section 2.0 Chemical Fate and Transport Equations

Dr. Brown:

Page 2-1: As mentioned above, equation 2.1-1 does not include vaporization.

Page 2-1, equation 2.1-1: The symbols in the equation are in italics, but in the text they are not. This is a problem throughout the document, and needs to be corrected.

Page 2-4, figure 2-1: The first step says subbasin. Shouldn't it say watershed, since we have been told that all the calculations are done on a watershed basis and that watersheds are not disaggregated into "subareas"?

Dr. Munster:

The governing equation for contaminate transport is a standard equation. The watershed module implementation flowchart is logical and efficient.

Mr. Norris:

a. The overview is clear and concise.

b. There are several references in Section 2.1.1 to Equations that are actually contained in the companion document to the Watershed Module. These equations should be repeated here or need to be better referenced. A different numbering system should also be utilized to minimize the possible confusion.

Section 2.2/Implementation Algorithm

a. Additional subsections need to be added to this section (2.2.1 Overview; 2.2.2 Simulation Stopping Criteria; and 2.2.3 End of Simulation Mass Balance Check).

b. The note contained in section 2.2.2 Simulation Stopping Criteria should be included as a footnote.

Section 3.0 Streamflow

Dr. Brown:

Page 3-1: The use of 30 day average base flow on a two year return period is a reasonable choice.

Page 3-1: Again, the bullets are not in the sentence format.

Page 3-2, Table 3-1: It would be good to have a map included to show the 18 HUC Regions since most people are not familiar with them and since you give no reference as to where to pull the map up.

Dr. Innskeep:

Page 3-1 Streamflow. Use of a single estimate to reasonably characterize annual average baseflow conditions may present limitations in model accuracy and representativeness. Can the authors provide some idea of the potential loss of information by going to a single statistic representing annual average baseflow.

Dr. Munster:

A well-conceived plan for the determination of annual baseflow values for streams throughout the U.S. is presented. However, there was no mention of water losses for streams in arid regions. Contaminate losses from "losing" streams may significantly impair groundwater supplies (Squillace et al.).

Mr. Norris:

a. A summary of the hydrology submodel from Section 3.2 of the Source Models for Non-Wastewater Waste Management Units (Land Application Units, Waste Piles, and Landfills) should be included here in addition to being referenced.

b. The definitions used throughout this section should be included as footnote to improve the readability of the materials being presented.

c. The use of the 30Q2 low flow was an appropriate choice to represent the annual average baseflow. This may prove to be a useful tool for other research and modeling efforts.

Section 4.0 USLE Length-Slope Factor

Dr. Brown:

Page 4-1, Paragraph 1, line 8: (essentially infinite number of) would be clearer.

Dr. Innskeep:

Page 4-1. Likewise, the watershed averaged length-slope factors used in the Universal Soil Loss Equation appear problematic in that averages of sheet-flow paths may have a pronounced effect on the amount of predicted erosion relative to a collection of variable flow-lengths and slopes. Can the authors suggest to what degree this may be a problem in predicting chemical flux via erosional processes?

Dr. Munster:

The procedures to determine the average watershed slope and average flow length appear to be adequate.

Mr. Norris:

a. The equation referenced here (Equation 3.3-2) should be repeated here and supporting rational summarized.

b. The note on "sheet-flow" length should be included as a footnote to this section.

c. The author is correct in his analysis that in reality stream flow will be flowing both toward the stream and along the watershed gradient. Further study should be undertaken at some point in the future to account for this sheet-flow path and its incorporation into another iteration of this modeling effort.

Section 5.0 Output Summary

Dr. Brown:

Page 5-1, Table 5-1: It is not clear to me why the units on NyrMet are years, while other number of years parameters are unitless.

Page 5-1, Table 5-1: Annual Infiltration: again the meaning of recharge needs to be made clear. In the text, infiltration is used with (recharge) in parenthesis.

Page 5-1, Table 5-1: The units on SW Load Solid R do not seem to match the description. Total suspended solids in runoff would have units of g/m^3 or should the description be "total suspended solids discharged to water body"?

Section 6.0 References

Dr. Brown:

I would put the date in parenthesis after the author's name. That is a better citation style.

ATTACHMENT A

Peer Review of LAU/WP/LF Source Moduels and Watershed Module by:

Anita R. Bahe and Robert J. Wyatt

LYNX Group Ltd.

Peer Review Summary for LAU/WP/LF Source
Modules and Watershed Module

Prepared by: Dr. Anita R. Bahe and Mr. Robert J. Wyatt
LYNX Group Ltd.

General Observations

Overall the concepts underlying the development of source modules for modeling risks from non-wastewater waste management units (WMU's) is innovative and potentially quite useful for predicting, minimizing, and managing environmental impacts. The combination of a small (aerial) scale model with long term (time) simulation findings is an engaging program. Attempting the integration of WMU source modules with multi-pathway, transport, and exposure modules, thus creating a holistic analytical tool, is desirable but obviously subject to problems. In all the modelers did a good job on basic assumptions for defining LF, LAU and WP units, however, as with all basic assumptions, it precludes use in certain situations.

Upon the completion of our review we returned to the "limitations section" provided with this submittal and noted that the modelers recognize some of the limitations we may elaborate on. Their recognition of the shortfalls or trade-offs, and subsequently their choices in model parameters, further made us uncertain of the ultimate utilization goals and practical applications desired for the completed model. We believe that it would have been more beneficial to our evaluation of the modules (model) if we were provided with a clearer understanding of the specific objectives of the model, including the targeted user group for the model. Further, our review was restricted because we were not given the opportunity to actually test a working version of the model (as opposed to review of theoretical integration).

We both agreed that the most puzzling aspect of evaluating this model was the fact that there is no "model" (software) to evaluate. It would be informative to see an integrated running version, especially considering that the developers have a working version which they used to perform ground-truthing in segments, (i.e., when running the model the developers numerical modeling was not working in their design and they switched to a "quasi-analytical" approach). Also, we recognize that although this model is currently being developed to run on a particular hardware system, it should be ultimately written in a language that would allow for PC applications.

Specific Section and Module Comments:

In the next several pages we are providing comments on each section and/or module. At the end of this comment section we provide a closing discussion, summarizing the elements in need of attention recurring throughout the proposed modeling protocol. Although comments noted in the closing section may seem redundant, they are provided to highlight issues that may deserve prioritization.

Sections 1.0 and 2.0:

The developers of this model made the decision to model a watershed as a single, *homogenous* unit with respect to soil characteristics, chemical concentrations therein, and runoff/erosion. They indicate that in the model, the simulated chemical loads to the waterbody are sole source (aerial deposition only), as is true for buffer area receptors. Further, they indicate that the direct loadings from runoff/erosion are to be simulated in specific source modules, then added to the indirect aerial loads to provide the total surficial soil concentrations. The potential problems with this approach are twofold. First, many chemicals that may be contained in solid wastes managed on land units may be subject to vertical migration through soil and geological profiles within the watershed area - (a factor not accounted for with the omission of spatial chemical concentration gradients). Second, the model is at risk of 'double accounting' the aerial deposits that are subject to further migration within the watershed by erosion or runoff.

It is questionable whether some of the assumptions made concerning the GSCM should hold with "soil" being defined as a porous medium whether it is the actual waste in the WMU or a near-surface soil in the watershed subarea assessed. Soils and waste units have a very different range of characteristics and are certainly subject to differing climatic and temporal impacts. Additional comments on the GSCM are noted later.

We felt that the variable time steps used for different data and modules (i.e., annual averages, daily values, etc.) were selected in order to minimize run time and data limitations, but the result compromises the consistency of the model. Some good points are made as to why it seemed impractical to build annual averages without using daily rainfall/runoff events as a foundation, yet this seems to contradict the often mentioned desire to maintain 'computational' efficiency considering the data intensity associated with daily time steps.

Section Two:

2.1

It is critical to assume that the soil column sorptive capacity does *not* become depleted over the duration of a year, let alone for long term modeling. This is a questionable assumption to make considering the type of risk analysis this model may be used for.

Assuming an infiltration rate constant in space and time for materials such as waste piles or land applied wastes could result in misleading estimates. This assumption does not seem probable when considering that vehicle traffic, loading and removal, tillage, and climatic variables exist. It also does not seem practical to assume one dimensional chemical transport through the soil, especially if "soil" is defined as waste material and/or soils by classification. Similarly, the assumption that the waste or soil material is homogenous likely excludes use of the model for many WMU sites. (In defense, one could suppose the "homogenous" zone and properties "uniform in space and time" could be reasonable if the zone is a single waste type or a consistent soil series known to exist across the entire watershed).

- Users must realize that the model may not be accurate if their particular LAU, WP, of LF is significantly non-homogeneous or consolidated. This is a likely condition for many older waste pile or landfill units.

When utilizing the model, users must recognize that the data they input for the model will be simulated based on annual averages inherent in the model. Input data which may vary on a shorter basis (i.e. daily, weekly, monthly, etc) must still reflect an annual total for that parameter. Users should also understand the nature of the contaminant(s) being modeled to verify that the assumption of reversible linear equilibrium partitioning between adsorbed and dissolved phases (especially in the case of metals) is appropriate. Also LNAPLS and DNAPLS do not appear to be addressed in the assumptions.

The model may not be as effective in coastal regions where significant tidal influences affect groundwater elevations significantly and on a consistent basis.

2.3

Parameter estimations are being evaluated in accordance with 'soil types' yet the integral model is targeted to waste. This estimation method assumes data for waste will be available much like it is for a soil series.

2.4

Given the fact that the numerical solution is not effective, the "quasi-analytical" findings should be weighted accordingly. Also the model should be restricted from use in settings where the majority of known contaminants are subject to high V_E or D_E. Also, it is interesting to note that this accepted trade-off enables long-term estimations but elsewhere in the model 'short-term' daily inputs are more desirable (again a question of consistency).

We noticed that the model does not allow for the actual effect of aqueous phase diffusion loss from soil to air, nor loss and transport from the watershed via volatilization. Instead, on the basis of "computational efficiency", this mass is calculated back into the top layer of soil. This concern is also related to comments on chemical aerial deposition mentioned above in the preliminary section discussion. It is possible that this augmentation could result in greater mass than justified in the top layer. In addition, it is assumed that the contaminant mass further concentrates in the surface soil as a result which indicates the model does not allow for (or integrate) the downward migration of contaminants. Would adding a site-related constant into this equation (2.4.1-10) based on local average meteorological conditions be feasible?

The diffusivity calculations are viable but it is questionable as to whether boundaries can be assumed C_T=0, especially when integrating with LAU or complete WMU's. Also, depending on the chemistry of waste compounds assessed, the diffusive losses from the bottom boundary may or may not ultimately become a steady-state transfer, especially with respect changing temperatures.

The model allows for first order losses entirely from the waste unit, but does not account for potential second order compounds and/or losses. This may not always be a valid condition, recognizing that many compounds decay and are transformed into other chemicals, often with greater toxicity (ex. - perchloroethelene to vinyl chloride). Users should be aware of the potential occurrence of secondary compounds (non-parent compounds) in the unit being modeled. Our concern about this omission is expressed across the various modules.

The convective transfer step is variable. Given the inherent model assumption of certain average annual values, it might be helpful to users to have guidelines for incremental data input options that would result in associated, fixed convective transfer steps fir which the model authors determine to be most accurate. This might increase the consistency of the model use, as well as help to reduce the "temporal distortion".

We agree with the limitations stated in section 2.5 of the documents provided. The discussion about shortfalls associated with precipitation, dissolution, geochemistry of the media, etc. in determining the K_d and fate and transport of metals is an issue that should be revisited (very dependent on the ultimate use of the model). The consideration of one contaminant at a time poses a problem because it is known that chemical interactions often effect transportability and persistence in a given media.

â We also question why photodegradation is ignored as a first order loss. It does not appear that the atmospheric gradient of transport has been extensively developed in the model.

Section Three:

3.1

Again this section is nicely integrated and the conceptual basis of the model is well defined. The sheet flow only restriction is explained and, due to computational constraints and goals for the model, has merit. From a practical environmental management point of view it seems that the assumptions behind the sheet flow only decision may leave contaminant transport throughout many types of watersheds nationwide unaccounted for. It would be beneficial to create an algorithm for watersheds that are known to have channel and stream flows that also influence WMU subareas, even if it were as simple as a (0) sheet flow only/ (1) sheet flow plus variable. Overall the definition of the WMU effects within the subareas are sufficiently addressed.

3.2.1

In many watersheds "infiltration" will not ultimately serve as the groundwater recharge source, as is assumed and modeled here. This potential shortfall should be fully described to model users along with estimation of the potential data gaps resulting thereof. It is somewhat confusing to discuss the hydrology model for waste unit subareas in the context of daily soil moisture values for the 'root zone' of the soil (or waste?) column. For buffers in the watershed or vegetated subunits, this makes complete sense – but as a link in the WMU model it does not seem appropriate (unless phytoremediation is a component).

3.2.2

The assumptions here may make the model unreliable in areas of exaggerated topography and proximate surface water. Fortunately, in the implementation section routing and non-routing procedural methods are stated as available options. This may help gear the model to more widespread watershed applications, especially when considering that the implicit assumption behind the routing method (i.e. - subareas not hydrologically connected and non-sheet flow drainage systems present) likely depicts many urban and farm areas.

3.3

There are some checks and balances in place throughout the model description, one of which is the use of a "sediment delivery ratio" – noted here to be used to account for eroded soil deposition associated with ditches, gullies, etc. This appears to help balance potential transport concerns relative to a sheet flow only approach as is mentioned earlier.

We believe that method 4 is reasonable in that it ties into the long-term average assumption of time in the Generic Soil Column Model. Future versions of the model would benefit from the ability to input actual terrain conditions (i.e. areas where sheet flow is not a valid assumption) both from calculation of flow, to selecting the best method for storm event data. Again, the only concern again is the shifting of time units between models and the potential loss of data consistency or relevance. It also seems that daily records could become data intensive which contradicts earlier discussions of "computational" objectives.

3.4

The simplifying assumptions made by the model developers result in the use of a "quasi-dynamic" or "quasis-analytical" approach and the subsequent instantaneous steady-state assumed for each storm event appear valid and reasonable as long as the model remains a tool for small scale areas and long-term simulations. The "Solids in Compartment" section is clean and efficient, with good discussion and questions about known variables, mass fluxes, and transport parameters.

3.4.2

Some of the inherent assumptions of the GSCM are based on an annual average. It is apparent that erosion of a sub basin will occur during that period of time. The assumption that the starting elevation of a unit is fixed is reasonable given the scale of the model; however, future versions should seriously consider adding an equation that adjusts the starting elevation of the unit based on the calculated volume of eroded material (the mass balance error). Otherwise, this error will become larger at the end of each simulation, and may make it difficult to identify other mass errors not related to erosion. Overall, this section provides a very nice solution to determining erosion and contaminants in run off. It might be worth evaluating the addition of an internal correction for elevation based on erosion since this value is both definable and quantifiable within the model.

The discussions about settling (vs) and resuspension (vr) parameters as processes internal to the subarea, along with burial/erosion reflected as net changes across the subarea is well thought. The only real tricky assumption subject to challenge relates to equations 3.4.1-1 and 3.4.1-2. They state that either can be represented as one equation in vs and vr unknowns, thus if one of these is a known variable the other can be solved. But they also state that (vr) would be difficult to obtain and that (vs) could be assumed as similar to "settling and compaction in sludge thickeners". Assuming similarities with 'sludge thickeners' may not prove indicative across many waste types or soil units. This poses an even greater concern considering this determination of (vs) is intended to be used to predict the resuspension velocity.

Where is equation 3.4.2-5 as referred to on page 3-24? [We assume that this is a typo and should read 3.4.2-5a.]

3.5.1

Again we observe the assumption for constant infiltration and convection in the GSCM module that is to be linked to the Local Watershed/Soil Column Models (LWSCM). As mentioned in the review comments for GSCM, this seems impractical when considering the likelihood of variability in some wastepiles and LAU's. Consequently the modelers attempt to compensate for this shortfall in the LWSCM thus allowing for annual infiltration variability which ultimately results in irregular intervals for the convection step. These alterations resulting from attempts to integrated across multiple models challenge the 'integration' feasibility of time steps and data inputs.

3.5.3

Taking leachate flux into account on a short term basis is an important consideration and provides a secondary solution for this module to integrate with the rest of the simulation. We agree with their evaluation of the limitation of this solution technique (i.e., convective transfer steps occur less than 1x/yr). Also, it is unclear as to whether or not this determination was made during preliminary model runs, and whether it is correlated to chemical specific leachate flux (i.e., what chemicals and substrates does this pertain to?).

Future versions of the model might benefit from consistent calculation steps as mentioned in previous comments. If those can be performed on a shorter scale increment, post-processing algorithms might no longer be necessary.

In discussing the outputs to the Vadose Module it is questionable as to whether or not the non-zero LeachFlux yearly time series from a local watershed will result in the exact same time series duration for all local watersheds. Variability in watershed subareas, and the WMU's contained therein would likely alter the time to non-zero LeachFlux. Are there additional assumptions being made that are not articulated in this section?

3.7.2

This section provides a reasonable and workable set of assumptions and modeling parameters to describe waste piles, given the incredible diversity of sizes and shapes in existence. Though 'workable' it is highly unlikely that waste piles will have a constant height and area for any length of time, let alone across WMU's sites. Mechanical, climatic and temporal effects must be compensated for elsewhere in the model unless generalizations are expected outputs.

Although the model developers state that realistically waste is added/removed from piles in increments, they still opt to model instantaneous refills. They also seem willing to compromise the potential volatile emissions losses that would otherwise occur and be accounted for. Many of the chemicals of environmental concern contained in a waste pile would be subject to periodic volatilization, and perhaps deposition in the watershed area during pile management activities. It seems that these potential pollution events need to be addressed here or elsewhere as an add-on to the model.

The automatic assumption of anaerobic bio-degradation in all subsurface layers of the waste pile might not be accurate in all cases. Certainly contaminant chemistry, pH, moisture conditions, etc will effect the anaerobic and aerobic microbiology and ultimate degradation activity in various layers. Future versions of the model might include a 0/1, aerobic/anaerobic condition switch to allow users to input conditions per layer based on field data.

The assumptions applied to the runoff and erosion processes are reasonable for computational purposes, but this portends unrealistic implications. How often are waste piles constructed and maintained with a top slope equal to the slope of the local watershed? The assumption that no remaining contamination will exist when a waste pile is completely removed indicates that either other bioremedial activities commence in the watershed that are not accounted for or that the half-life of the compounds contained is short in comparison to storage time. We realize that these assumptions are supposed to apply only to the runoff and erosion pathways, but temporal influences can lead to periodic release of materials in surface layers that should be accounted for in the model.

As we commented earlier, it is not apparent from the text but seems likely that, the mass calculated back into the top layer of soil to simulate diffusion from soil to air in the CSCM might affect the accurate calculation of erosion and/or convection based time steps.

3.8.2

This section provides a reasonable set of assumptions given the diverse nature of land applications units nationwide. When considering that the till zone depth modeled is fairly shallow {(dz) = 0.01 m} for the LAU, the assumption of insignificant effects of waste additions on hydraulic properties is reasonable. As far as first-order loss, volatilization should be included since the depth of till is shallow. If the modeler has a valid reason to omit this pathway it should be clearly articulated to the user as to why and, if at all, where the complete model sufficiently accounts for such potential loss.

<u>Section Four</u>:

The computational efficiency gained from implementing a single cell simulation makes sense with the exception of lateral cell impacts that certainly exist in older landfills and/or those located in more expansive areas with shallow groundwater tables. The conceptual design of the data grid, based on filling the landfill units in increments, is clear and reasonable based on the long term scale of the model. Although many of the assumptions will not be valid in all cases they appear reasonable for providing a model platform, especially for landfills constructed in the late 1970's and throughout the 1980's.

There does not seem to a provision for older landfills with more variable fill histories. Future versions of the model might focus on allowing for spatial diversity of parameters within units. The current model will not pertain to many newer landfills utilizing more modern liners. Also, it seems impractical to omit simulations for specified soil cover and subsoil zones, as this is often a regulatory and/or funding requirement in many states nationwide. Depending on the type of waste managed in a landfill unit, by choice, the subsoil zone and cover layers may *not* be homogenous with the cell media.

It is good that the modelers note incoming HWIR99 waste, specified to have a constant contaminant concentration, can be adjusted in accordance with other wastes added to the cell lacking the contaminant of interest. The question is, how are variable contaminant interactions influencing the chemical form and concentration of those contaminants of 'interest'? Are there assumptions concerning chemical interactions in the waste profiles that account for such potential fluctuations that simply have not been articulated in the document?

A concern stated earlier in this summary arises again for this model, that is the lack of second-order chemical and biological fate and transport simulations. Landfills, even more so than LAU's, contain many chemical by-products of degradation and contaminant mixing. Also, the constant infiltration rate assumption remains questionable here as in the other modules previously discussed.

For existing landfills already comprised of several "units" (i.e. older landfills, non-homogeneous landfills, etc.), it would be beneficial if the model could accept the total unit "to date" as the first simulation unit. Otherwise, older landfills will have to be simulated as they were constructed historically. This process would probably introduce greater error than making a unit assumption for the existing conditions. Future units could then be layered or added on in a predictive and reliable manner.

The discussion on boundary conditions is sensible, especially since the user can specify the boundary condition multiplier and subsequent condition to be simulated. This gives more flexibility to the model than would otherwise exist.

4.4

The modelers note that they are aware the current model may over estimate both contaminant mass losses from the waste zone into the cover zone, and thus volatile emissions. The advective transfer of the contaminant mass allowed from the cover soil to the waste mass could provide a balance in the model estimations. This is an example of a situation where actually running different scenarios would help to determine the over estimation risks.

4.6.2

Although computer memory constraints limit the existing model to 200 years, future attention should be given to expanding that limit (especially in the case of metals). Further, the model developers should consider developing the model for use as a PC application.

Watershed Module

It appears that the watershed module is fairly straightforward and based on commonly used modeling parameters. We feel that as long as the drainage basin is homogeneous with respect to soil characteristics, runoff and erosion characteristics, and chemical concentrations, this model framework appears reasonable. As was noted for the GSCM and WMU models, we remain uncertain about the practical applications of a model that links modules using very different time steps (daily, as a function of annual averages, etc.).

The transferability of model applications is somewhat challenged by the consideration of each watershed as a single, homogenous area. Many of the parameters important in estimating environmental impacts (erosion and runoff, flow, chemical and nutrient transport, atmospheric deposition, vegetation, intrusion, etc.) are highly variable within and among U.S. watersheds. Similarly, baseflow determinations are critical to successful use of the model and one important question, posed in section 3.0, is – "what single flow statistic best represents annual average baseflow for a given region, reach order, and year?". Although it appears that a well thought algorithm was derived, some of the required inputs developed specifically for this model (as part of the HWIR effort) are not readily available from traditional data sources. This may or may not prove to be problematic.

We recommend that future versions should allow for spatial variability of soil characteristics, runoff and erosion characteristics, and chemical distribution. This is critical, in that many important drainage basins cover large areas and include varied combinations and types of developed and undeveloped land.

Closing Remarks

The concepts underlying the development of individual modules (LF, LAU, WP) for modeling environmental risks associated with non-wastewater management units (WMU's) in watersheds are original and are potentially useful. The model developers recognize some of the existing shortfalls in the model and make a solid attempt to explain the rationale for choice. Regardless, we were left unsure about the targeted user groups and the ultimate goals of the model from an applied and practical perspective. It was apparent however that a strong emphasis was placed on "computational efficiency" and the associated systems constraints. Technically, this model may suffice, depending on the desired outcome by EPA, but from an environmental management standpoint both utility and data were inevitably compromised.

In general each of the modules (LAU, WP, and LF) and individual models (GSCM, Watershed) seemed reasonable with respect to function as specified by the limitations on data and inputs. In particular, the GCSM and Watershed models were rooted in existing functional prototypes that were subject to modification for WMU linkage purposes. With a primary goal solely based on computational efficiency it appears that theoretically the model developers have a good protocol.

For multiple users in environmental management the limitations and practical applications pose concern. Though these have been more definitively discussed earlier we feel that the following issues, given sufficient attention, could help to rectify these concerns and provide a model with greater utility and benefits. These emerging issues include:

The use of variable time steps for different data and modules may minimize run time but compromise the consistency of the data.

When choosing to define “soils” as either waste materials or a series of soils in the WMU subareas it is impractical to assume homogenous conditions and constant infiltration rates. For that matter, assuming that a watershed is homogenous with respect to soil, chemical concentrations, and runoff/erosion does not appear very realistic.

The model is at risk of “accounting” errors, either omitting the losses that occur from pathways not included such as photodegradation and second order losses; or by double counting contaminant transport and fate as a result of efforts to maintain a mass balance across linked models. The latter of these is exemplified by calculating mass back into the top “soil” layer following volatilization and gaseous diffusion loss, with uncertainty as to whether or not wind, leaching, and runoff/erosion events recount the input.

Regardless of the WMU type, second order losses of chemical contaminants need to be modeled in order to apply the outputs to risk evaluations. Likewise, although less complex to model perhaps, using a sheet flow only contaminant transport evaluation in a watershed limits utility of the model. Many watersheds have significant channel and streamflow impacts.

A single cell, homogenous media, no lateral migration model for a landfill simplifies the simulation but many landfills will not fit this condition. Many landfills are required to use specific soil covers and subsoil zones or layers which should be more clearly integrated into the model.

There are many good points and viable scenarios or functions presented throughout the documents. The model developers may be able to prioritize “compromises” based on end user demands and gradually build the model over time. In any event it seems prudent to develop applications that can also be run on a PC.

ATTACHMENT B

Peer Review of LAU/WP/LF Source Moduels and Watershed Module by:

Kirk Brown

Texas A & M University

Review of "Source Models for Non-Wastewater Waste Management Units"

Draft dated Oct. 13, 1999

The document is generally well written and reasonably easy to understand and follow. There are a variety of difficulties with the presentation which need to be improved. Concerns will also be raised about a few of the assumptions and the lack of consistency in the use of symbols in the equations in different sections.

In the title, the reader is never told that HWIR99 stands for the Hazardous Waste Identification Rule, but why 99?

Page 1-1, Paragraph 1: I think it would help if we were told up front that the model will be applied to both organic and inorganic constituents.

Page 1-1, Paragraph 2: I think the sentence in parenthesis needs to have "or the soil under a landfill" added to be complete. Also, in this paragraph in the eleventh line we find HWIR, this time without 99, and undefined.

Page 1-1, Third Bullet, Second Arrow: One usually thinks of leaching as advection and not including diffusion. Perhaps it would be better to say "Movement of aqueous phase constituent mass by advection or diffusion ....."

Page 2-1: The bullets are set up in pseudo-sentence style. For example: In the first bullet the statement ends with a period. Then we find "where" followed by a list of definitions and no period at the end. This is a problem throughout the text.

Another problem illustrated in this bullet is that the symbols in the equations throughout the text are typically in italics while those listed in the text are in standard font style. Notice that C and t are slanted in the equation but not in the text. Also the b in ρb should be a subscript.

I have checked the units on many of the equations, and they appear to be correct. However, it is peculiar to me that one system of units was not used throughout. For instance below equation 2.1-1 all the volumes are in m^3 except for bulk density which is given in cm^3. It might be best to put everything in m^3 to avoid confusion. Also, perhaps kg should have been used instead of g.

Page 2-1, Last bullet: We are told here that the K_d can be specified for inorganic contaminants. This implies that if we have an inorganic contaminant - say a metal, we can simply follow the equations with the appropriate K_d. That almost works, but then on Page 2-9 in the second last paragraph we are told that zero concentrations are assumed at the upper boundary.

I think that it would be good to make it clear throughout how organic and inorganic contaminants are handled so that there is no confusion.

Page 2-5, Paragraph 3: I think it would be good to show some comparisons between the explicit finite difference method and quasi-analytical solution. This should be done at least in the range where numerical dispersion is not a problem, to give the reader some feel of the validity of the approach.

Page 2-6, Paragraph 3, Line 5: "Thickness" is a better term for *dz* than width.

Page 2-7, Paragraph 1: Similarly, I would use "down" instead of "to the left" and "up" instead of "to the right". In fact, I would turn the figure 90°.

Page 2-10, Last bullet in Item 1: z_{ava} and z_{avb} are not in Appendix C. In fact, there are numerous instances where symbols are not given in the Appendix.

Page 2-10, Third bullet in Item 1: The subscript on K is difficult to read, but looks wrong. It should be K_{sat} not K_{sap}.

Page 2-10, Item 4: Infiltration rate needs to be defined. In soils it means the movement of water through the soil surface, while to a geologist it means the flux of water below the root zone.

Page 2-11, Item 15: Many of the terms here are not found in Appendix C.

Pages 2-10 and 2-11: Many of the numbered items are not written in complete sentences. Here again, this is a matter of style and should be consistent throughout the document.

Page 2-12, Second bullet, Second sentence: The statement that "with metals, the presence of a precipitate is not allowed" is bothersome to me. Precipitation is the major mechanism by which most metals are sorbed, and their movement is retarded in soils. Are you saying that the presence of elemental metals as opposed to metal compounds is not considered?

Page 2-13, First bullet: You make a good point that the outputs are not strictly applicable to individual years.

Page 3-1, Last line: It would be good to say "For example, in figure 3.1-2a, subarea 2 might be..."

Figure 3.1-1: The divide in this figure is called "drainage divide" while in Figure 3.1-2a it is calle d "watershed divide".

Figure 3.1-1: The stream flows to the left in Figure 3.1-1 but to the right in Figure 3.1-2a.

Figure 3.1-2a: Where is the WMU? Better figures would help.

Page 3-5, Last paragraph: It is not appropriate to simply assume that the growing season is June through August. That introduces significant errors in southern parts of the country where the growing season is much longer. The best solution might be to use frost free days as the growing season. There are published maps of frost free days for the United States.

Page 3-6: It would be preferable to have the pseudo-code in a table or box rather than in the text.

Page 3-7: SM needs to be defined here.

Page 3-7: WP is given as % by volume here. However, in Appendix C, it is given as cm. It can only be cm if a depth of soil is defined. The same problem occurs with FC. The units must be dimensionless, otherwise AW is in cm^2.

Page 3-8: The use of Hargreaves equation is a reasonable choice.

Page 3-10, Paragraph 1: The units on SL should be $kg/m^2/day$ to be consistent with most typical units in Appendix C. The hyphen used here and for the units of R_{appl} in Appendix C is poor form.

Page 3-10, Paragraph 3: K values are reported by soil series, not by soil type.

Page 3-11, Paragraph 3: Since method 4 is used here, I don't see the need to spell out the other three in detail.

Several places we slip back and forth between using no sign to indicate multiplication (equations 2.2-1 and 2.2-2a), a dot in the equation (equations 2.4.1-9 and 2.4.1-10), and an asterisk (equation 3.3-1 and item 1 on page 3-12). The equations look like they were put together by a committee. A consistent style should be selected and used throughout.

Page 3-15, Paragraph 2: The second sentence has no verb.

Page 3-16: The units on ρ are incorrect. It needs to be 2.65 g/cm^3 or 2.65 Mg/m^3.

Page 3-17, Paragraph 2, Line 9 and the line after equation 3.4.1-2: I think the word deposition would be better than burial. Same on Page 3-18, Paragraph 2, third last line.

Page 3-18, Paragraph 4: The origin of the 0.05 to 1.0 m/day seems a little strange. Why not apply Stoke's law to some aggregate sizes to determine if this is an appropriate range for soil? Another source of information would be the dredge spoil research efforts at the Waterways Experiment Station in Vicksburg, Mississippi.

Page 3-19, Equation 3.4.1-8 and elsewhere: Why not use ρ_b instead of m_2?

Page 3-20, Last line: "can be expressed"

Page 3-22, Paragraph 2: Again, I suggest using "deposition" instead of "burial".

Page 3-26, Paragraph 2: *j=ae*, for losses due to wind/mechanical activity. Where are these described and how are they quantified?

Page 3-27, Note at bottom of page: It is somewhat bothersome to me to describe this effort as a screening model when the note indicates that the calculations are limited by the memory of modern computers.

Page 3-32, Table 3.6-1, Variable I: Again, the infiltration rate needs to be defined as indicated above.

Page 3-32, Table 3.6-1, Variable J_{lch}: Here it says flux but in Appendix C it says flux at the lower soil boundary.

Page 3-33, Table 3.6-1, Variable C_T: Again, the definition and even the units differ between here and Appendix C.

Page 3-36, End of first bullet: It doesn't seem correct to assume no surface water pathway for metals.

Page 3-38: The illustration is for organic compounds and not metals.

Page 3-39, Paragraph 1: Change to "dry bulk density of the waste".

Page 3-39: We have seen equation 3.8.2-6 before, but every time it looks different. See equation 3.4.1-8 and ή in the GSCM model. Equation 3.8.2-6 surely does not need to be attributed to anyone.

Page 4-1, Last bullet: It seems a little odd that no liner is being considered in the landfill model. I recognize this is a conservative approach, but in this day and age it is not realistic. We have relatively good data on leakage and diffusion through liners. A paper I did on the topic is attached.

Page 4-4, Paragraph 3: It bothers me that diffusive flux is not considered. In some cases it is the major mechanism of flux.

Page 4-4, Paragraph 3, Last sentence: This sentence does not apply to metals and other inorganic contaminants.

References: The references need considerable work. There is no consistent style. It would be best to put the date in parenthesis after the author's name. I have not gone through them in detail but just a glance indicates many inconsistencies.

Page A-9, Last line: $\leq 2.5\ \mu m$

Page A-15: The references here look much better.

Appendix C: Many symbols used in the text are missing.

Page C-3, Symbol ρ_b: the "2" in M2 should be subscripted

Page C-3, Symbol $\rho_{b,w}^{wet}$: Why the "wet"? On page 3-38 there is no superscript "wet" used.

Page C-3, Symbol *bcm*: Lower soil column, not coil

Page C-4, Symbol *dz*: Replace the word size with "thickness".

Page C-5, Symbol Fc_i: This term is unitless in the text but here it is defined as having units of cm.

Page C-6, Symbol M_i: Why not give subscripts instead of using bullets (squares).

Review of Watershed Module

The module is well written and easy to understand and follow. The technical approach of using local watersheds appears appropriate.

There are several points that need clarification and perhaps a bit more thought. These include:

The reader needs to be told what HWIR stands for and the significance of 99 in the title. This is jargon which obviously the authors are very familiar with, and thus, did not tell us that it stands for the Hazardous Waste Identification Rule. I thought it was published Nov. 19, 1998, or is it that the effective date is in 1999?

Page 1-1, Paragraph 1, Line 1: Volatile emission is mentioned as being a release mechanism, yet equation 2.1-1 does not include a term to account for vapor loss from the soil. In the same line, WMU needs to be defined.

Note that in line 6 of the same paragraph, volatilization is mentioned as one of the simulated processes.

Page 1-1, Paragraph 2, Line 3: There are different meanings to both the words infiltration and recharge. To the soil scientist infiltration means the flux at the surface of the soil. To the geologist both words mean the flux toward the water table at the bottom of the root zone, as infiltration to the aquifer or recharge to the aquifer. Thus, these terms need to be defined here to avoid confusion.

Page 1-1, Paragraph 2, bullets: First, although the statements start with a capialized word and end with a period, they are not sentences; secondly, the colon indicates they should not be capitalized. This is a matter of style. Finally, the order seems a bit strange to me. The second one - chemical loading should be the last one, and the others should also be put in an order which logically leads to the stream discharge.

In the first bullet I understand that volatile losses from the watershed are lost and don't need to be considered, but in the first paragraph you say leaching is simulated in the Watershed module, now we are told that "leaching from the soil column..... are not subsequently viewed as input to the Air or Vadose Modules", or is it that leaching is recalculated in the soil column model?

In the last bullet we again encounter infiltration (recharge).

Page 1-3: Again bullets don't fit into the sentence, but here most of them are sentences.

Page 1-3: In the second bullet there is a list of parameters k_{ev} ... m, which need to be defined

Page 2-1: As mentioned above, equation 2.1-1 does not include vaporization.

Page 2-1, equation 2.1-1: The symbols in the equation are in italics, but in the text they are not. This is a problem throughout the document, and needs to be corrected.

Page 2-4, figure 2-1: The first step says subbasin. Shouldn't it say watershed, since we have been told that all the calculations are done on a watershed basis and that watersheds are not disaggregated into "subareas"?

Page 3-1: The use of 30 day average base flow on a two year return period is a reasonable choice.

Page 3-1: Again, the bullets are not in the sentence format.

Page 3-2, Table 3-1: It would be good to have a map included to show the 18 HUC Regions since most people are not familiar with them and since you give no reference as to where to pull the map up.

Page 4-1, Paragraph 1, line 8: (essentially infinite number of) would be clearer.

Page 5-1, Table 5-1: It is not clear to me why the units on NyrMet are years, while other number of years parameters are unitless.

Page 5-1, Table 5-1: Annual Infiltration: again the meaning of recharge needs to be made clear. In the text, infiltration is used with (recharge) in parenthesis.

Page 5-1, Table 5-1: The units on SW Load Solid R do not seem to match the description. Total suspended solids in runoff would have units of g/m^3 or should the description be "total suspended solids discharged to water body"?

References: I would put the date in parenthesis after the author's name. That is a better citation style.

ATTACHMENT C

Peer Review of LAU/WP/LF Source Moduels and Watershed Module by:

William P. Inskeep

Montana State University

November 8, 1999

Eastern Research Group, Inc.
110 Hartwell Avenue
Lexington, MA 02421-3136

To Whom It May Concern:

As per the Consulting Agreement between William P. Inskeep (myself) and Eastern Research Group, Inc., I have enclosed a hardcopy and disc copy (Word document) of my peer review of the documents entitled:

Land Application Unit (LAU), Waste Pile (WP), Landfill (LF) Source Modules and Watershed Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99

Watershed Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99

A summary and specific comments are contained in my peer review. Briefly, the document describes a modeling procedure where source modules for wastepiles, land application units and landfills are coupled with local watershed/soil column models to predict contaminant fluxes controlled by processes including volatilization , erosion, leaching, degradation and runoff. The document does a thorough job outlining model calculations and assumptions.

Revision and reorganization could improve the document. The scope and intended purpose of the modeling exercise could be clarified. Further, model development and evaluation would be improved if example model runs were included to give readers a better sense of how assumptions in the model, or sources of input data may influence model outcome. Although limitations of various model assumptions are discussed, it is difficult to appreciate the potential limitations without evaluating variations in model output based on different assumptions. The document would benefit from a greater use of visuals to support model conceptualization and sensitivity analysis regarding primary controlling input parameters. Additional suggestions for improving the document are provided in the enclosed comments.

If you have any questions concerning my review, please do not hesitate to contact me at binskeep@montana.edu or 406-994-5077.

Sincerely,

William P. Inskeep, Ph.D.
Consulting Soil Scientist

November 5, 1999

Peer Review: Land Application Unit (LAU), Waste Pile (WP), Landfill (LF) Source Modules and Watershed Module:

Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99

Preface

The comments contained here are intended to serve as a peer review of documents prepared by the Research Triangle Institute, October 13, 1999, entitled:

Source Models for Non-Wastewater Waste Management Units (Land Application units, Waste Piles and Landfills): Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99

Watershed Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99

Summary Comments

The draft documents described above contain detailed descriptions of calculations, assumptions, and limitations of model subroutines used to estimate contaminant fluxes out of waste-piles, landfill application units and landfills. The source models are linked to "local watershed" and "soil column" modules for predicting contaminant fate pathways (volatilization, erosion, surface water runoff, leaching) and respective fluxes. The Thorough documentation is provided for nearly all calculations, and followed by generally insightful comments regarding limitations, computational efficiency and practicality regarding intended use. The reports appear to have accomplished the primary objective of documenting and cataloging model calculations and assumptions.

That said, after several trips through the document, I found myself wanting a clearer presentation. Specifically, I felt as though the scope of the project was poorly defined, making interpretation regarding completeness difficult. Further, it was not entirely clear who was the intended primary audience. Given the presentation style and the sparing use of visuals, I would guess that the audience best suited for the report is other modelers. Perhaps that is the case; it was not explicitly stated. Regardless, I believe the relative ease with which a reader will understand the document could be improved by additional visualization, with clarity of purpose and scope.

Regarding scope, I was struck by the lack of example model simulations, which would seemingly accomplish so much toward the goal of model development, testing and evaluation. In any modeling application, it is worthwhile to show input file formats, anticipated or suggested data sources, example output file format, and sensitivity analysis concerning major model assumptions or sources of data variation. Little effort was expended discussing the potential contaminants of interest and the intended contaminant scope (other than a brief comment about metals versus organics). In addition, model development and model performance are inextricably linked with an assumed or implied temporal and spatial resolution of input parameters. In the report, it is often difficult to assess the intended model resolution and the expected source of input parameters. For example, issues of scale compatibility of input parameters and process description, and the use of average properties to represent homogeneous media, need to be addressed quantitatively and with greater clarity.

To summarize, I believe the report does an excellent job providing documentation for model assumptions and calculations. However, depending on the intended audience, the report would benefit from significant revision. The primary goals would be to (i) improve accessibility to key organizational and conceptual design and (ii) evaluate more thoroughly the effects of model assumptions and input data sources on model output and performance relative to intended use. Specific comments on each module follows; hopefully, the comments will assist in finding avenues for improving the work and the potential impact.

<u>Specific Comments</u>

Introduction. I assume that the primary objective of this modeling effort is to link the source modules with environmental fate and exposure/risk models. Right from the outset, the scope of the effort is not defined or presented in a manner that makes it obvious to the reader why the report is organized as it is. A schematic could be used to show the intended linkage of source modules and fate models right from the outset. The first paragraph of the introduction should then try to give the entire perspective of the modeling efforts. This schematic should outline the primary sections that will be discussed in the report, and provide a context in which the reader can easily place the various modules and subroutines. The authors should consider reorganization where source modules are discussed first, with the clear connection and linkage of output that is used in local watershed/soil column models. As it now stands, readers are presented first with information regarding fate and transport modeling with not much understanding of how the source modules will interface. It also brings to question why Section 2 is necessary in its current location. It might be preferable to save the Generic Soil Column Model for an Appendix. Can the soil column model be introduced first when integrated with the local watershed module? For reasons that were not apparent, I did not understand why the GSCM was presented first, yet is not used in that form for integration with the local watershed module. The subject of the report (i.e. prediction of contaminant fluxes) is really addressed in Section 3 with inputs from 3.7 and 3.8. It would seem advantageous to move these discussions to an earlier location in the document or at least improve visualization of how the source modules are linked to the local watershed/soil column models.

The last paragraph in the introduction is necessary and should be retained, but as it stands, it doesn't appear consistent with the sections contained in the report. It is stated that "Sections 4 and 5 describe the specifics of the application and integration for the WP and LAU, respectively". "Section 6 describes specifics of the application of the GSCM to the landfill". These statements aren't consistent with current Section headings.

Other: Ironically, I couldn't find HWIR explicitly defined in the document. Define upon first usage in the introduction.

Page 2-1. Generic Soil Column Model. As mentioned above in organizational suggestions, I would be inclined to discuss source modules first followed by soil column and local watershed models. I would summarize all fixed and variable input parameters and suggested data sources in Table format. Although this information is obviously redundant with the text provided, it will add immediate clarity to how the model will be used. Further, linkage to the LAU, WP and/or Landfill source modules should be explicitly shown in the summary Table.

Page 2-2. Define η upon first usage.

Page 2-3. I am not clear how dispersion is addressed in the model. The Convective (or Advective)-Dispersion Equation (CDE) is normally defined in terms of a lumped dispersion coefficient accounting for hydrodynamic dispersion as well as solute diffusion. It is unclear whether the effective diffusivity accounts for or implies dispersion. Based on the estimation techniques provided for D_E, it appears that hydrodynamic dispersion is ignored. Under many circumstances hydrodynamic dispersion is considerably more important than diffusion in soil solutions.

Page 2-4 and 2-5. How is annual average infiltration obtained, and is the parameter fragmented to smaller time steps or does the time step = annual. The method presumably allows for estimation of long-term annual average contaminant fluxes (2-5), but it is unclear based on what is presented, whether water movement is estimated annually or daily. Certainly, episodic daily events yield different patterns of water movement relative to constant daily values. This might be discussed here for clarity.

Page 2-10. The Algorithm is defined here in text, but it would be beneficial to also present this in Figure or flowchart form as was done for source modules.

Page 2-12. Section 2-5. Second sentence in first paragraph is awkward….omit "When applied to metals.." Also, somewhere in this section, it would be worthwhile to comment on limitations of the general approach where annual average infiltration is used as the primary driving variable for contaminant transport. What is the role of preferential flow in contaminant transport under natural conditions? Given expected sources of input data, what limitations might be associated with representativeness of input data to variable landscape positions?

Page 2-13. The solution technique used to solve the mass balance expression seems unnecessary. There are a number of solutions to the CDE and it's various forms that are not particularly inefficient computationally. At a minimum, it would be nice to show that the order of solution does not control the outcome. This should be done with realistic example input data.

Page 3-1. Section 3. The first part of this section is spent introducing the need for source modules. As mentioned in comments above, authors may consider presentation of source modules first, identifying visible linkage to local watershed/soil column models, followed by the more detailed description of the local watershed model.

The figures used to introduce the local watershed/soil column model (Figures 3.1-1, 3.1-2) could be improved to show actual model function, complete with input parameters and primary output. The visuals do not go far enough in assisting the reader with model conceptualization and linkage with source modules.

Page 3-4. Clearly identify the calculation time step. It appears in equation 3.2.1-1 that all calculations are based on daily inputs. This is fine. It would be helpful to reiterate in subsequent sections such as runoff, ET, and recharge what the calculation time step is and how that corresponds with available data for input.

Page 3-12. Method 2. Define and cite PRZM.

Page 3-14. Last paragraph. The comment on "screening level objective" should be introduced earlier as well. It would be worthwhile to make this clear in the Introduction.

Page 3-27. First sentence is confusing. "The infiltration rate is allowed to vary from year to year". Do you mean that the *annual* 'I' is allowed to vary? Won't 'I' actually vary daily, based on the time step used for calculation?

Page 3-27. Sentence under 3.5.2, No.2 beginning “The year the simulation ceases in each local watershed….” is awkward, rewrite.

Page 3-28. Section 3.5.3

It is difficult to imagine that convective transfer steps occur so infrequently based on daily time steps. Does the convective transfer step refer to water or solute? Authors comment that leaching occurs “more-or-less continuously” and that the routine presented here provides a smoother leachate flux over time. It might be nice to point out that leaching fluxes will not generally occur ‘more-or-less continuously’ unless the infiltration rate is constant in time and space. Leaching events are generally episodic corresponding to water inputs, and in real systems, preferential flow paths may be very important for distributing solute to greater depths than predicted using homogeneous media. Authors may want to discuss this more thoroughly at this point.

Waste-Pile and Land Application Specifics (Section 3.7-3.8). I stated earlier that it may be preferable to outline source modules first. I can see some difficulties in accomplishing this, and it may not improve the report. At a minimum, I would put more focus on linking the source modules with the Local Watershed/Soil Column Model earlier in the report.

Page 3-39. Land Application Unit. Regarding discussion that “waste addition does not affect significantly the hydraulic properties”, I can understand that this would be difficult to predict *a priori*. How sensitive is the model to hydraulic properties, and would it likely make a difference based on current inputs. Are source data varied for tilled versus non-tilled soil. If not, what affect does an assumption of constant hydraulic properties of tilled vs. untilled soil have on model output.

Page 3-40. Appropriate depth for soil column surface layer = 1 cm, or for entire till zone? It seems that 1 cm is too small to represent the till zone.

Section 4. Landfill Model

Page 4-1. Make it clear in the introduction here and in the main introduction whether the landfill model serves as a source model for the Local Watershed/Soil Column model.

Page 4-1. Assumptions. It seems potentially problematic to assume that landfill waste zone has homogeneous properties uniform in space and time. Given the type of waste that will be incorporated into the landfill waste zone, it seems problematic to assume that this zone will be homogeneous. The potential for preferential flow paths in mixed solid waste environments appears too great to warrant an assumption of homogeneous porous media in this zone. Could the authors provide some justification for this assumption, or discuss more thoroughly what the properties of this zone might actually be?

Page 4-2. Last bullet. How are concentrations of contaminant in landfill waste determined? What types of contaminants can be accurately modeled with this approach, and how do sorption properties impact solute transport (are these the same as described in the local watershed/soil column model?)

Page 4-3. Use of anaerobic rate constants may be problematic in cases where aerobic conditions prevail. Is it safe to assume that all zones in landfill will be anaerobic?

Page 5-1. References.

Authors cite but don't include reference for Clapp and Hornberger, or for Penman, 1948.

Inconsistent spelling for "Stevens" in reference by Shan and Stephens, 1995.

Cite reference for PRZM.

Watershed Module: Background and Implementation for the Multimedia, multipathway and multireceptor Risk Assessment (3MRA) for HWIR99

Page 1-1. Introduction. The purpose of this module is to presumably predict fate of chemical mass released from Waste Management Units (WMU) in the form of either volatile or particulate emissions. The Introduction states that the watershed is modeled as a single homogeneous area with respect to soil characteristics, runoff and erosion characteristics and chemical concentrations in soil. Without examples discussing limitations inherent in this approach, it appears too simplistic to be of real value. If indeed, soil, runoff and erosion characteristics are taken as averages across the watershed area, then the model overlooks the fundamental processes responsible for such fate pathways. For example, in real watersheds, erosion and leaching events are likely distributed temporally and spatially in response to locally variable conditions.

One of the purposes of the Watershed Module is to simulate the time series of annual average chemical concentrations resulting from aerial deposition in the area of interest (AOI). How is the deposition itself modeled? How do we know the amount deposited in the AOI.

Page 1-2 and 1-3. Although I can appreciate the arguments provided to justify using a longer time step in the Watershed Module compared to the Local Watershed/Soil Column Model, the authors should provide some discussion of limitations of using annual average data to estimate fate of contaminants reaching off-site locations via aerial deposition. The remainder of this module is essentially identical to that described for the local watershed/soil column module.

Page 3-1 Streamflow. Use of a single estimate to reasonably characterize annual average baseflow conditions may present limitations in model accuracy and representativeness. Can the authors provide some idea of the potential loss of information by going to a single statistic representing annual average baseflow.

Page 4-1. Likewise, the watershed averaged length-slope factors used in the Universal Soil Loss Equation appear problematic in that averages of sheet-flow paths may have a pronounced effect on the amount of predicted erosion relative to a collection of variable flow-lengths and slopes. Can the authors suggest to what degree this may be a problem in predicting chemical flux via erosional processes?

ATTACHMENT D

Peer Review of LAU/WP/LF Source Moduels and Watershed Module by:

Clyde Munster

Texas A & M University

Peer Review

EPA's Hazardous Waste Identification Rule Risk Assessment

by

Dr. Clyde Munster, P.E.

November 1999

Submitted to:

Eastern Research Group, Inc.
110 Hartwell Avenue
Lexington, MA 02421-3136

SOURCE MODELS FOR NON-WASTEWATER WASTE MANAGEMENT UNITS (LAND APPLICATION UNITS, WASTE PILES, AND LANDFILLS)

1.0 Introduction

The source models for non-wastewater waste management units (land application units, waste piles and landfills) as presented in the documentation are well described with good detail and references. The concept of modeling waste piles (WPs), land application units (LAUs) and landfills (LFs) as individual modules to provide annual average surface soil concentrations is an valuable innovation. These source emission modules (WPs, LAUs and LFs) are then effectively coupled with both a watershed model and a Generic Soil Column Model (GSCM). The watershed model simulates contaminate transport in solution in surface water runoff or attached to sediment in erosion. The watershed model simulation provides contaminate and erosion rates to down-slope water bodies and buffer areas. The GSCM simulates contaminate transport through the unsaturated soil profile and incorporates advection, diffusion and volatilization transport mechanisms. This system of WP, LAU and LF modules coupled with the watershed surface runoff component and the GSCM soil leaching component provides a comprehensive simulation tool for assessing contaminate transport from these non-wastewater management units (WMUs).

2.0 Generic Soil Column Model

The GSCM uses standard contaminate transport equations to partition pollutants into three phases; adsorbed, dissolved and gaseous. The mass balance equation governing mass fate and transport is a typical equation applied to unsaturated soils that incorporates advection, dispersion and decay terms. A quasi-analytical solution technique was adopted to allow for long-term estimation of annual average contaminate transport.

The column is assumed to be an unconsolidated homogeneous porous media that is constant in space. Again, a standard assumption that simplifies the soil column equations and solution techniques. However, this assumption precludes any type of preferential flow paths in the porous media. That is, the model assumes complete mixing of water and contaminate throughout the soil column. In many soils, this is not the case, as preferential flow pathways dominate the transport process. In preferential flow, water, and any contaminates dissolved in the water, bypasses most of the soil matrix (Steenhuis et al.). The complete mixing model does not apply in this case.

Consider a clay soil with a very low saturated hydraulic conductivity (K_{sat}). Very long travel times would be calculated for water to travel through a column packed with clay soil. Even “representative” core samples taken from surface clay soils in the field and tested in the laboratory can be rather impermeable. However, on a larger scale, there are a tremendous number of cracks and fissures that extend throughout a clay soil, even to very great depths (< 20 ft) (Chakka and Munster). These cracks are most noticeable during extended dry periods, especially in soils with high shrink-swell potential. Surface runoff events that occur in cracked clay soils will quickly infiltrate, especially, early in the runoff event. As the initially dry soil begins to wet-up, the cracks tend to close somewhat. However, the “first-flush” of runoff and any contaminates present in the runoff, have already infiltrated, bypassing most of the soil matrix. This reduces or eliminates much of the sorption and degradation that may take place during complete mixing in the soil column.

However, I also understand the necessity to make simplifying assumptions for a watershed scale model. To incorporate preferential flow into a model is extremely difficulty and data intensive. The computation effort is increased by on order of magnitude, or more. However, this simplifying assumption may lead to controversy. This is a risk assessment model that makes a simplifying assumption that precludes a very real risk factor. The risk for contaminates to be quickly transported to the groundwater via preferential pathways. At the very least, I would suggest adding another bullet to section 2.5, Limitations Related to Use of the GSCM, related to the no preferential flow assumption.

3.0 Local Watershed / Soil Column Model (LAU, WP)

The LAU and WP modules provide annual average contaminate mass flux rates from the top and bottom interfaces of these WMUs. The WMUs are an integral of the "local" watershed and are affected by up-slope runoff and erosion. In turn, the WMUs affect down-slope areas through surface runoff and erosion. The conceptual model for the LAU and WP and the coupling of these WMUs to the watershed is well thought out. This model accurately represents field conditions in two-dimensions (longitudinal and vertical).

The basic hydrology equation (3.2.2-1) is standard and accounts for all of the various hydrologic processes. However, the assumption that frozen precipitation is treated as rainfall is unwarranted. Typically, snowfall events, during subfreezing air temperatures, do not produce any runoff, and should be modeled as such. Other models, such as Soil Water Assessment Tool (SWAT) simulate no runoff during precipitation events with subfreezing air temperatures (Neitsch et al., 1999). This may make a big difference in contaminate transport, especially in northern locations where snow is on the ground for extended periods of the year.

Runoff estimation using the Soil Conservation Service (SCS) curve number procedure is appropriate, especially for rural areas. The curve number procedure was developed for rural areas by the Natural Resource Conservation Service (NRCS, formerly the SCS). Therefore application to urban area needs to be done very carefully. Impervious surfaces, gutters and ditches in urban areas are not easily accounted for in the curve number procedure (Haan et al., 1994).

The Hargreaves potential evapotranspiration (PET) equation for calculating evaporation and transpiration losses from the soil profile is adequate. However, several of the Hargreaves PET equations appear to be in error. I realize that there are many forms that these equations can take, and I didn't have time to research the discrepancies throughly. So the equations that I suggest may, in fact, be equivalent to the equations in the Source Models document. The standard Hargreaves equations from the American Society of Civil Engineers (ASCE) manual on Evapotranspiration and Irrigation Water Requirements (1990) provides the following equations:

$$PET = [0.0023 S_o \Delta_T^{0.5} (T + 17.8)] / \lambda \qquad (3.2.3\text{-}5)$$

where $\lambda = 2.501 - 0.002361(T)$ MJ/kg

$$S_o = 37.586 d_r (\omega_s \sin\phi \sin\theta + \cos\phi \cos\theta \sin\omega_s) \qquad (3.2.3\text{-}6)$$

$$\theta = 0.4093 \sin[(2\pi(284 + J) / 365] \qquad (3.2.3\text{-}9)$$

The infiltration equation has a feedback loop that increases the amount of surface runoff in the event infiltration exceeds the moisture capacity of the top soil layer. Previously, I had stated that a limitation of the GSCM was the inability to account for preferential flow paths in the soil column. There may be a way to bypass this feedback loop for soils that are known to exhibit preferential flow paths. That is, instead of transferring this water to surface runoff, somehow, maintain this water in the soil. Perhaps by allocating it to some of the deeper soil layer in the GSCM. If this solution is not feasible, then perhaps some other modeling mechanism could be utilized.

Soil erosion is based on a modified universal soil loss equation (MUSLE). The equations all seem appropriate. However, I believe that there is a misprint on page 3-13, the second line from the bottom. The value 30.5 m does not seem appropriate for the 0.5% slope. Perhaps it should be 305 m instead.

The conceptual model for chemical fate and transport (section 3.4) is very logical and comprehensive. The runoff compartment model incorporates diffusion, settling, re-suspension and burial and erosion. Contaminate transport in the runoff accounts for both dissolved and adsorbed pollutants. A lot of effort has gone into simulating sediment transport. Even the preferential erosion of finer soils is taken into account. Again, maybe an effort to incorporate preferential flow of water in the soil column would be appropriate also.

The assumption in section 3.5.1 that infiltration is constant in the GSCM and that convection events occur at regular intervals throughout the entire simulation is at the heart of the preferential flow issue that I've raised. It is my contention that, for many soils, infiltration is not a constant and the transport of contaminates does not occur at regular intervals. Infiltration and contaminate transport occurs primarily during and immediately after rainfall events (Lin and McInnes). A more realistic approach would be to permit higher infiltration rates during and immediately following a rainfall event. This would increase solute velocity (V_e) in the soil and result in faster transport of water and contaminate through the soil than an average infiltration rate and an average V_e rate. In turn, this would affect (maybe significantly) contaminate loading to the groundwater. For example, if a watershed experienced four major rainfall events in one year, and they all occurred one month, water and pollutants would be transported through the soil column very quickly. The contaminates may even enter the groundwater system if a shallow groundwater table was present. On the other hand, if those same four rainfall events were spread over an entire year with an average infiltration rate, the contaminates would not be transported as far through the soil column and maybe not into the groundwater system. Additionally, increased contaminate biodegradation may occur with longer residence times in the soil. Slower contaminate transport is definitely not a conservative prediction of pollution potential.

The leachate flux post-processing algorithm seems very artificial to me. The need for this algorithm seems to be a consequence of using the average infiltration rate. If a more realistic infiltration process was employed, than this post-processing algorithm would probably not be necessary.

The waste pile model is very comprehensive and well-thought out. The assumptions of insignificant side slope and top slope equal to the average watershed slope does not represent tall, rounded piles. However, simplifying assumptions must be made for all models, and these assumptions are adequate for most cases.

I have some serious concerns regarding the assumptions about the land application unit conceptual model. It is unclear to me whether a liquid waste can be applied in the absence of a soil waste fraction. Many manufacturing and agricultural operations dispose of liquid waste using land application. A typical agricultural land application practice would be to irrigate effluent from a manure waste treatment lagoon directly onto pasture land (Aldrich et al.). There is no tilling that takes place, as stated in the first LAU assumption. There is no bulk density or organic carbon fraction for the liquid waste as stated in the third assumption (p. 3-38). In the case of a pure liquid waste, equations 3.8.2-1 to 3.8.2-4 do not apply. The assumption that "the contaminate mass is concentrated in the solids portion of the waste" is null and void. This is a serious limitation that would preclude this model from being applied to many of the land application processes employed by agriculture and industry.

4.0 Landfill Model

The landfill conceptional model is a very accurate representation of actual conditions. I see no major omissions or issues to comment on. The landfill model is very well done.

Appendix A: Particulate Emission Equations

Not qualified to comment.

Appendix B: Determination H', Da, and D_w for organic compounds

These are standard equations that apply to organic compounds and appear to be correct.

Appendix C: Symbols, Units, and Definitions

There are numerous symbols referenced in the text that are not listed in appendix C. Just one example would be M_{lcha}. This is presented in the text on page 3-26 but is not found in appendix C. Many other variables are similarly missing from appendix C.

WATERSHED MODULE

1.0 Introduction

The watershed module simulates the fate and transport of contaminates placed on the soil surface by aerial deposition. The processes simulated include volatilization, leaching, runoff, erosion and biological and/or chemical degradation. Erosion, runoff and contaminates are transport to adjacent surface water bodies. The watershed simulated by the module is assumed to be homogeneous with respect to soil, runoff and erosion characteristics as well as chemical concentrations in the soil.

A primary assumption for the watershed model is that contaminate concentrations in the soil due to aerial deposition will be significantly less than soil concentrations resulting from direct runoff from a source. This assumption is used as justification for a large time step (up to one year) in the GSCM. In fact, it is clearly stated in the assumptions, that this large time step is based on numerical considerations, not physical considerations. However, it is not difficult to conceive of a situation where a highly toxic compound is deposited on the soil surface. In this case, even very low concentrations of this compound may pose a risk to groundwater supplies. It may be controversial for a risk assessment model to discount this consideration.

2.0 Chemical Fate and Transport Equations

The governing equation for contaminate transport is a standard equation. The watershed module implementation flowchart is logical and efficient.

3.0 Streamflow

A well-conceived plan for the determination of annual baseflow values for streams throughout the U.S. is presented. However, there was no mention of water losses for streams in arid regions. Contaminate losses from “losing” streams may significantly impair groundwater supplies (Squillace et al.).

4.0 USLE Length-Slope Factor

The procedures to determine the average watershed slope and average flow length appear to be adequate.

SUMMARY

The source models for non-wastewater waste management units (LAUs, WPs and LFs) and the watershed module are very well done. Conceptually, these models very accurately represent the actual, “real world” conditions. The governing equations for the models are based on well established scientific principles.

This watershed scale model fills the gap between field scale models and regional scale models very well. However, some of the simplifying assumptions that are required for a watershed scale model may lead to non-conservative simulations of contaminate transport. The assumption of complete soil and water mixing and a constant average annual infiltration rate precludes preferential flow and rapid transport of contaminates through the soil profile. This is a serious tradeoff that needs to be addressed in the risk assessment model or clearly stated in the assumptions.

The model is not able to simulate purely liquid waste applications to the land. This is a serious omission that needs to be corrected. Many agricultural and industrial land application practices could not be simulated by this model.

Several other minor issues were addressed in the review. However, on the whole, this model is very well thought out and well documented. I think that it will be a very valuable tool for assessing risks at non-wastewater waste management units.

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APPENDIX

JOURNAL ARTICLES

ATTACHMENT E

Peer Review of LAU/WP/LF Source Moduels and Watershed Module by:

William Norris

Virginia Department of Environmental Quality

2150 Meadowfield Way
Charlottesville, VA 22911

November 3, 1999

Ms. Nicole Schubert
Peer Review Coordinator
Eastern Research Group, Inc.
110 Hartwell Avenue
Lexington, MA 02421

SUBJECT: Consulting Agreement Number: 0120.00.017.002-08/ERG Task No. 17/Contract No. 68-W-99-001

Dear Ms. Schubert:

Enclosed please find a completed Peer-Review of the documents provided by ERG on EPA's HWIR Risk Assessment Methodology under EPA Contract No. 68-W-99-001, Work Assignment No. 17, Consulting Agreement Number: 0120.00.017.002-08. I have also enclosed a disk copy of the review in Microsoft Word format.

As per my conversation with you today, I am also submitting my review by email to you at nschube@erg.com.

Also enclosed you will find an invoice for services and work completed under this agreement. I would appreciate your prompt attention to this Invoice for Payment.

Thank you for this opportunity to participate in this peer review. This is a well thought out document and will have great use in the waste management, watershed management and risk assessment arenas. Please let me know if I can be of further assistance in your peer review efforts.

Please contact me if you have any questions about this review or if I can be of further assistance. I can be reached at work at (804) 698-4022 or (804) 293-3031 or at home at (804) 974-1955. My email address at work is wknorris@deq.state.va.us and at home is norriswk@aol.com.

Sincerely,

William K. Norris

PEER REVIEW

OF

EPA's HWIR RISK ASSESSMENT METHODOLGY

Source Models for Non-Wastewater Waste Management Units (Land Application Units, Waste Piles, and Landfills

And

Watershed Module

Background and Implementation for the Multimedia, Multipathway, and Multirecptor Risk Assessment (3MRA) for HWIR99

FOR

EASTERN RESEARCH GROUP, INC.
WORK ASSIGNMENT NO. 17
EPA CONTRACT NO. 68-W-99-001

BY

William K. Norris
2150 Meadowfield Way
Charlottesville, VA 22911

Agreement Number: 0120.00.017.002-08

Peer Review by William K. Norris

Part I: Source Models for the Non-Wastewater Waste Management Units (Land Application Units, Waste Piles, and Landfills: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99/October 13, 1999 – DRAFT

I. General Comments

A. **Nomenclature** - There needs to be a consistent nomenclature for program components. The GSCM is referred to as the Generalized Soil Column Model in the Acknowledgments but as the Generic Soil Column Model throughout the rest of the document.

B. **Acronyms -** A listing of acronyms used throughout the document should be included in the first part of the document.

C. **Target Audience -** The use and sometimes over-use of acronyms is prevalent throughout the documents and research materials that are being developed today. The authors of this document need to make sure that the target audience has a clear understanding of the acronyms and abbreviations used so that the product can be properly applied and utilized.

D. **Terms -** A definition of terms used in the document should be included in addition to the definitions of the symbols and units definitions. I would suggest a separate "definition of terms" section as well as the use of footnotes to indicate the proper definition of the terms as they occur within the document.

II. Specific Comments

A. Introduction/Section 1.0:

1. The introductory materials are clear and concise.

2. I would suggest moving the definitions included in the text of this section to footnotes to make the section more readable.

B. **Generic Soil Column Model (GSCM)/Section 2.0:**

1. **Section 2.0 – Specific:**

a. Even though the rational for the development and use of the Generic Soil Column Model has been presented in the Introduction Section, a summary paragraph repeating the key points should be included at the beginning of this section to ensure that the reader and ultimate user of this model fully understands why it was developed and why certain assumptions were necessary.

2. **Section 2.1/Assumptions:**

a. I find no inconsistencies with the assumptions identified with the Generic Soil Column Model (GSCM) as presented in this section.

3. **Section 2.4/Solution Technique:**

a. This is a well thought out and developed section. The limitations of the quasi-analytical approach developed for use in HWIR (i.e., the loss of the ability to evaluate short-term trends in concentration and diffusive flux profiles) should be acceptable for this use. This method does indeed seem well suited for this type of analysis.

b. The author might want to consider the inclusion of an appendix with an example of a real world situation to the "Solution Technique" so accurately presented in Section 2.4.

c. This section should be further divided into Subsections 2.4.1 Background, 2.4.1 Description of the Quasi-analytical Approach, 2.4.3 Boundary Conditions and 2.4.4 Algorithm.

4. **Section 2.5/Limitations Related to Use of the Generic Soil Column Model (GSCM):**

   a. The first several sentences of the first bullet of Section 2.5 seem to be disjointed and fragmented. There seems to be several incomplete thoughts within this paragraph. The authors should revisit this section and make sure that the proper information is being presented.

   b. Given the extent of current contamination/contaminant levels on sites and the likelihood of commingling of contaminants, further model development to account for this possibility is definitely warranted and should be actively pursued in a future phase of this effort.

   c. The order of the solutions proposed (diffusive losses, first order losses and advection) seems to be a logical approach.

   d. The assumption made concerning the boundary conditions at the soil/air interface seems to be consistent with and in reasonable agreement with what actually occurs in nature.

C. Local Watershed/Soil Column Model (Land Application Unit, Waste Pile)/Section 3.0:

Section 3.1/Introduction:

   a. **T**he numerous acronyms should be spelled out the first time that they are used in this section.

   b. From a watershed perspective, yes these waste management units are integral to land area on which they are located. In fact as indicated they will or may have an impact on the watershed area in which they are located. Therefore, a holistic modeling approach is a proper one.

   c. I would recommend rewording the third sentence of the first paragraph to read: "Indeed, after some period of time, during which runoff and erosion has occurred from a waste management unit (WMU), the downslope land areas will have been contaminated and their surface concentrations could approach or even exceed the residual chemical concentrations in the WMU."

d. The definition of "local watershed" could be included as a footnote to this page in order to help the flow of the text.

e. The limitation of a "local watershed" to that area in which runoff from the Waste Management Unit (WMU) occurs only as overland flow (sheet flow) is an appropriate one. This will by definition limit the size of the "local watershed" to more manageable units. All the literature that I am familiar with indicates that once sheet flow from any source approaches or exceeds 100 feet (depending on the slope, the type of cover and the erodibility of the soil) it becomes rill erosion and starts making defined channels.

f. The figures presented in this section are clear and concise.

2. Section 3.2/Hydrology:

a. The simulation of watershed runoff and groundwater recharge is appropriate.

b. Additional identification of Subsections might be appropriate in Section 3.2.2 (i.e., 3.2.2.1 Governing Equations and 3.2.2.2 Implementation).

c. The daily runoff method identified here is appropriate for rural areas and is widely accepted. Have the authors considered the use of this methodology in more urban or suburban settings? More and more waste management units are appearing in these more urbanized settings. It would be interesting the see a comparison of the use of the USGS Regression Analysis (Evaluation and Management of Highway Runoff Water Quality, Publication No. FHWA-PD-96-032, U.S. Department of Transportation, Federal Highway Administration, June 1996) versus the SCS method for determining runoff from these waste management units.

d. I concur with the use of the Hargreaves equation to determine daily evapotranspiration; it is less demanding and is better suited for use on the sites being examined.

3. Section 3.3/Soil Erosion:

a. Additional Subsections might be appropriate in Section 3.3 Soil Erosion (i.e., 3.3.1 General, 3.3.2 MUSLE Implementation, and 3.3.3 Spatial Implementation).

b. The Universal Soil Loss Equation (USLE) is an appropriate tool on which to base the Soil Erosion Model. The USLE or the MUSLE ("Modified" Universal Soil Loss Equation) has been used to successfully identify and manage soil loss in a number of rural and urbanizing localities through Erosion Control Ordinances (i.e., Albemarle County, Virginia).

c. It would be useful to include Figure 3.1 – 1 again (maybe as a separate and new figure) on or near page 3-13 since it is referenced there.

d. The fourth sentence of the first full paragraph of Section 3.3.3 Spatial Implementation on Page 3-13 should be slightly reworded for clarity. I would suggest the following: "As part of the HWIR data collection process, the delineation of the sheet-flow-only 'local watershed' is accomplished by a Geographical Information System (GIS) analysis. A key component of this analysis is the correct generation of the water body network such that the water body delineated as lying downslope of the 'local watershed' is in fact the first 'defined drainage channel' that the runoff would encounter."

e. The 700,000-m2 upper bound does present ample opportunity for sheet flow to evolve into erosive channel flow. My experience shows that flows exceeding 100 feet (depending on soil conditions, cover and slope) may develop into small-defined channels and start being an erosive force. I concur with the analysis that dividing the "local watershed" into smaller areas may be appealing and would be appropriate for a more complex evaluation.

However, limiting the flow distance to a reasonable maximum may be more appropriate for the HWIR's screening level objective. For the purposes of this model, the maintenance of mass balance for the "local watershed" is of prime importance. Areas approaching this reasonable maximum should be identified as potential problem areas and marked for further evaluation at some date in the future.

4. Section 3.4/Chemical Fate and Transport:

a. Additional subsections might be appropriate for Section 3.4.1/Runoff Compartment (i.e., 3.4.1.1 Introduction; 3.4.1.2 Solids in Runoff Compartment; 3.4.1.3 Solids in Soil Compartment and 3.4.1.4 Contaminant in Runoff Compartment).

b. The rational for the assumption of instantaneous steady state for solids and chemical concentrations in runoff is appropriate.

c. There is a word missing in the second sentence of the last paragraph of subsection 3.4.1.1 Introduction. The sentence should read: "Development of mass balance equations for solid(s) and chemical concentrations follow."

d. The exclusion of hydrolysis, volatilization, and biodegradation processes from the runoff compartment is appropriate.

f. The rational presented for the transport of solids in the soil compartment and the subsequent use of the solids mass balance equations to determine the burial/erosion and resuspension parameters is sound and is presented in a logical progression.

g. The definition of "finer soil particles" might be more appropriate as a footnote on Page 3-21.

h. The logic and flow of the materials presented in Section 3.4.2 Soil Compartment appears to be fine. However, it appears that the materials presented on page 3-22 on the minor mass balance error created by the burial/erosion mechanism would be better presented as a footnote explanation.

i. The further refinement of the Generic Soil Column Model (GSCM) to accommodate the minor mass balance error of the burial/erosion mechanism should be part of a future effort that has a greater scope.

5. Section 3.5/Implementation:

a. The first paragraph of Section 3.5.1 Overview should reference Figure 3.5-1 as Figures 3.5-1a and 3.5-1b. In addition, an effort should be made to locate these figures closer to the reference location.

b. The use of the TermFrac criterion or 200 years, whichever comes first, as the determinant for the length of the "time series" seems to be logical.

c. I would recommend the placement of the note on the computer memory requirements currently in the text on page 3-27 in a footnote.

d. A space needs to be added in the next to the last sentence of the first paragraph in Section 3.5.3 Leachate Flux Processing. The space needs to be added before…"is the average infiltration rate between years a and b."

6. Section 3.6/Output Summary

a. It appears that all of the outputs have been included in the summary.

7. **Section 3.7/Waste Pile (WP) Model Specifics**

a. Figure 3.7.1-1. Illustration of a Waste Pile in a "Local Watershed" could be moved into Section 3.7.1 Introduction. Then Section 3.7.2 Additional Assumptions would not have to be broken by the insertion of the figure in that section.

b. The explanation of the addition and removal of waste incrementally is better suited for placement in a footnote.

c. Although the waste pile model may underestimate volatile losses it will still provide a fairly good estimate of these losses and is appropriate for this scale of modeling effort.

d. The assumption that there is no subsequent runoff/erosion transport pathway from the waste pile subarea and the assumption that there is no remaining contamination in the subarea that contained the waste pile may not apply to all real world sites. I understand the limitations imposed by the models being used and concur that if in fact no surface runoff pathway exists then there will be no surface runoff.

e. The logic and sequence of the processes outlined for Waste Piles is consistent with use in a screening level effort.

8. **Section 3.8/Land Application Unit**

a. Figure 3.8.1-1. Illustration of the Land Application Unit (LAU) in the "Local Watershed" should not be placed in such a manner as to break up the following additional assumptions section.

b. The second sentence of the third full bullet on Page 3-39 should read: "Thus, the hydraulic properties of the soil (K_{sat}, SM_b) are used in Equation 2.3-1 to determine the water content <u>of</u> the till zone."

c. I agree with the assumptions and the logic presented for the Land Application Unit.

D. **Landfill Model/Section 4.0**

1. **Section 4.1/Introduction**

a. The introduction is clear and concise.

2. **Section 4.2/Additional Assumptions**

a. The assumptions identified are logical and seem to accurately reflect what may occur in real world situations.

b. I hope that the decision to not include a liner in the Landfill Model for HWIR99 purposes will be revisited, if not in this effort, at least for a future effort. The use of liners in landfills today is a state of the art solid waste management technique and is prolific throughout the industry. The only problem may be determining what thickness or style of liner to use in the model.

3. **Section 4.3/Landfill Cell Simulation -- First Year**

a. Additional subsection numbering should be added to Section 4.3.1 (4.3.1.1 Upper and Lower Boundaries and 4.3.1.2 Inner Waste/Subsoil Boundary).

4. **Section 4.4/Landfill Cell Simulation -- After First Year**

a. Additional subsection numbering should be added to Section 4.4.1 Boundary Conditions (4.4.1.1 Upper and Lower Boundaries; 4.4.1.2 Inner Waste/Subsoil Boundary; and 4.4.1.3 Inner Cover Soil/Waste Boundary).

5. **Section 4.5/Calculation of Landfill Results**

a. More detail is needed to flesh out this section. The summary that is presented is short and to the point, but I fear does not do the effort justice. A summary example might be appropriate.

6. Section 4.6/Implementation Algorithm

a. Figure 4.6-1 should be identified as Figure 4.6.1a. Landfill Model Flowchart for an Active Cell (Year 1) and Figure 4.6.1b. The Landfill Model Flowchart for a Closed Cell (Year 2+) on page 4-6.

b. Figures 4.6-1a and 1b should be included immediately adjacent to Section 4.6.1/Overview if possible.

c. The algorithm as presented seems to be logical and the assumptions made are appropriate for the screening level model.

7. Section 4.7/Output Summary

a. It appears that all of the outputs have been included in the summary.

E. Appendix A/Particulate Emission Equations

1. Section A.1/Introduction

a. It appears that all of the available release mechanisms have been considered and are included as part of the consideration for each of the Waste Management Units.

2. Section A.2/Particulate Emission Rate ($E30_l$) Algorithms and Particulate Size Range Mass Fractions

a. Additional subsections might be appropriate in Section A.2.1/Wind Erosion from Open Fields [$E30_{wd}$] (A.2.1.1 Step 1: Calculate U_{*t}; A.2.1.2 Step 2: Calculate U_t; A.2.1.3 Step 3: Calculate $E30_{wd}$ and A.2.1.4 Step 4: Apply Particle Size

Range Mass Fractions).

b. There is an extra page (Page A-7) in the document that should be eliminated.

c. The definition of total suspended particulate matter (TSP) should be included as a footnote on Page A-9 in Section A.2.2.

d. I concur with the assumptions identified and outlined in this section.

3. Section A.3/Particle Size Range Mass Fractions for the Total PM30 Emission Rate

a. The term Pm30 in the title for this section should be PM30.

4. Section A.4/Annual Average Constituent Emission Rate (CE30) Equations

a. The term Ce30 in the title of this section should be CE30.

b. Equation 2.4.1-12 referred to here should be repeated here for clarity.

F. Appendix B/Determination H', D_a, and D_w for Organic Compounds

1. General Comments

a. The assumptions and procedures identified in this section appear to be appropriate.

b. A separate reference section (Section B.5) should be added.

G. Appendix C/Symbols, Units, and Definitions

1. General Comments

a. The break points in the chart of Symbols, Units, and Definitions need to be checked, there are several continuation points in the table that should be reconciled.

Peer Review by William K. Norris

Part II: Watershed Module: Background and Implementation for the Multimedia, Multipathway, and Multireceptor Risk Assessment (3MRA) for HWIR99/October 13, 1999 – DRAFT

I. General Comments

A. Acronyms - A listing of acronyms used throughout the document should be included in the first part of the document.

B. **Target Audience -** The use and sometimes over-use of acronyms is prevalent throughout the documents and research materials that are being developed today. The authors of this document need to make sure that the target audience has a clear understanding of the acronyms and abbreviations used so that the model can be properly applied and utilized.

C. **Terms -** A definition of terms used in the document should be included in addition to the definitions of the symbols and units definitions. I would suggest a separate "definition of terms" section as well as the use of footnotes to indicate the proper definition and use of terms as they occur within the document.

II. Specific Comments

A. Introduction/Section 1.0

1. General Comments

a. WMU, LAU and WP should all be spelled out, at least the first time that they are used in this document.

b. A definition of watershed should be included as a footnote to this section.

c. This section is very well constructed and supplies some very clear and concise information on the workings of the

Watershed Model and its interaction with the Land Application and Waste Pile Modules.

d. The note included in the first bullet on Page 1-1 should be inserted as a footnote.

B. Chemical Fate and Transport Equations/Section 2.0

1. Section 2.1/Overview

a. The overview is clear and concise.

b. There are several references in Section 2.1.1 to Equations that are actually contained in the companion document to the Watershed Module. These equations should be repeated here or need to be better referenced. A different numbering system should also be utilized to minimize the possible confusion.

2. Section 2.2/Implementation Algorithm

a. Additional subsections need to be added to this section (2.2.1 Overview; 2.2.2 Simulation Stopping Criteria; and 2.2.3 End of Simulation Mass Balance Check).

b. The note contained in section 2.2.2 Simulation Stopping Criteria should be included as a footnote.

C. Streamflow/Section 3.0

1. Comments

a. A summary of the hydrology submodel from Section 3.2 of the Source Models for Non-Wastewater Waste Management Units (Land Application Units, Waste Piles, and Landfills) should be included here in addition to being referenced.

b. The definitions used throughout this section should be included as footnote to improve the readability of the materials being presented.

c. The use of the 30Q2 low flow was an appropriate choice to represent the annual average baseflow. This may prove to be a useful tool for other research and modeling efforts.

D. USLE Length-Slope Factor/Section 4.0

1. Comments

a. The equation referenced here (Equation 3.3-2) should be repeated here and supporting rational summarized.

b. The note on "sheet-flow" length should be included as a footnote to this section.

c. The author is correct in his analysis that in reality stream flow will be flowing both toward the stream and along the watershed gradient. Further study should be undertaken at some point in the future to account for this sheet-flow path and its incorporation into another iteration of this modeling effort.