APPENDIX B - Individual Responses to Charge Questions

The following is a series of comments and recommendations from individual panel members for revision of the preliminary risk assessment and the associated predictive models.

The charge questions for this consultation are focused on the five primary documents. Please comment on the adequacy and readiness of the aforementioned draft documents for further scientific review.

- Do the documents provide adequate descriptions of study design, methods, conclusions, limitations and uncertainties?

General comments:

Dr. Dale Hattis

An aggregate environmental flow analysis missing. This I believe is needed to assess changes in risks especially to distant people and other receptors. Distant people of special risk include the Inuit of northern Canada who I believe have substantial PCB exposures from eating higher-trophic level marine fish and mammals. It is likely that even in a worst case analysis it can be shown that the ex-ORISKANY could only make a very minor contribution to the exposures of these people but it would show appropriate concern for the evaluation of the “unreasonable risk” criterion if the expected increments to their exposures were included by a modest additional effort at analysis of expected PCB emissions in relation to larger environmental compartment flows. It is even possible, perhaps likely, that a full analysis will show modest net reductions in PCB flows to distant receptors because the modest increase in South Atlantic biomass may well absorb and sequester more background PCBs from the ambient seawater than is expected to be emitted from the sunken aircraft carrier. The bioconcentration modules in the PRAM model could readily be used to evaluate this if the modelers supplied even very approximate estimates of overall and specific-biota biomasses to go with the concentrations predicted from the PRAM software in contact with ambient seawater PCB concentrations.

Dr. Taylor Eighmy

Overall, this is a complex and challenging subject with some significant unknowns. It is apparent that an immense amount of work has already been directed at this effort. The Navy and its partners have earnestly tackled this non-trivial subject with great enthusiasm.

From a larger perspective, the mass of PCBs on the ex-Oriskany may or may not contribute significantly to the PCB regional budget in the Gulf of Mexico (or perhaps even globally). Absent analysis of how this source relates to total loads, it is difficult to determine relative contributions and impacts. I suspect that 700 to 1200 lbs of tPCBs is not an insignificant amount.

One management solution is to remove all PCBs before reefing the ex-Oriskany and all other Navy ships in the reefing inventory. However, it this is ultimately cost prohibitive, then I believe revisions to TDM and PRAM (or perhaps, more appropriately, development of an alternative probabilistic fate and transport model) will likely be needed so their utility for risk assessments is realized and alternative management decisions can be explored.

The comments that are offered here relate specifically to the ex-Oriskany case, but also may have utility to the possible continued exploration of Navy ship reefing and the need for a comprehensive approach for looking at PCB transport and fate in these reef environments.

1. Investigation Of PCBs Release-Rates From Selected Shipboard Solid Material Under Laboratory-Simulated Shallow Ocean (Artificial Reef) Environments, June 2005 (Draft Final)…

- Please provide comments as to the adequacy of the study to support the fate and transport models (PRAM and TDM).
- Please comment on whether the nonliquid-PCB materials selected for evaluation were sufficiently representative of PCB materials on the ex-Oriskany and other vessels to provide a basis for evaluating ship sinkings.
- Please provide comments as to whether biodegradation or encrustation processes should have been considered in the study.

Dr. Dale Hattis

From the central-estimate inventory appearing in the CACI report, over 97% of the total estimated content of the Ex-ORISKANY is in the electrical cable insulation (705.5 lbs/722.6 lbs) = 97.6%). Therefore I decided to check the calculations that led to the estimates of concentration and source release rates from this component of the PCB-containing material. I first plotted the concentrations reported for 59 samples—using two different grouping intervals to make sure that the results are not dependent of the widths or starting points for the intervals I selected (see Figures 1 and 2 (on the following pages). Both plots indicate that, as reported in the PRAM documentation, even after log transformation the distribution departs appreciably from normality. It appears that there are at least two modes, with the high-concentration mode—including 5 of the 59 samples between 10,000 and 29,000 ppm—having about 92% of the total mass of PCBs that would be contained in an equal-weight combination of all 59 samples. Unfortunately, the one sample of cable studied in the leaching experiments contained only about 1200 ppm PCBs (0.12%). This means that the representativeness of the single sample used for electrical cable leaching rates is very questionable. A higher concentration electrical cable sample—which would be more representative of the material containing the vast majority of the PCBs on the ex-ORISKANY—is likely to have a larger fractional rate of release of PCBs into sea water, meaning that the principal PCB source driver for the PRAM model is likely to have been substantially underestimated—with corresponding effects on concentration levels in different media, biota, and ultimate risks.

Figure 1

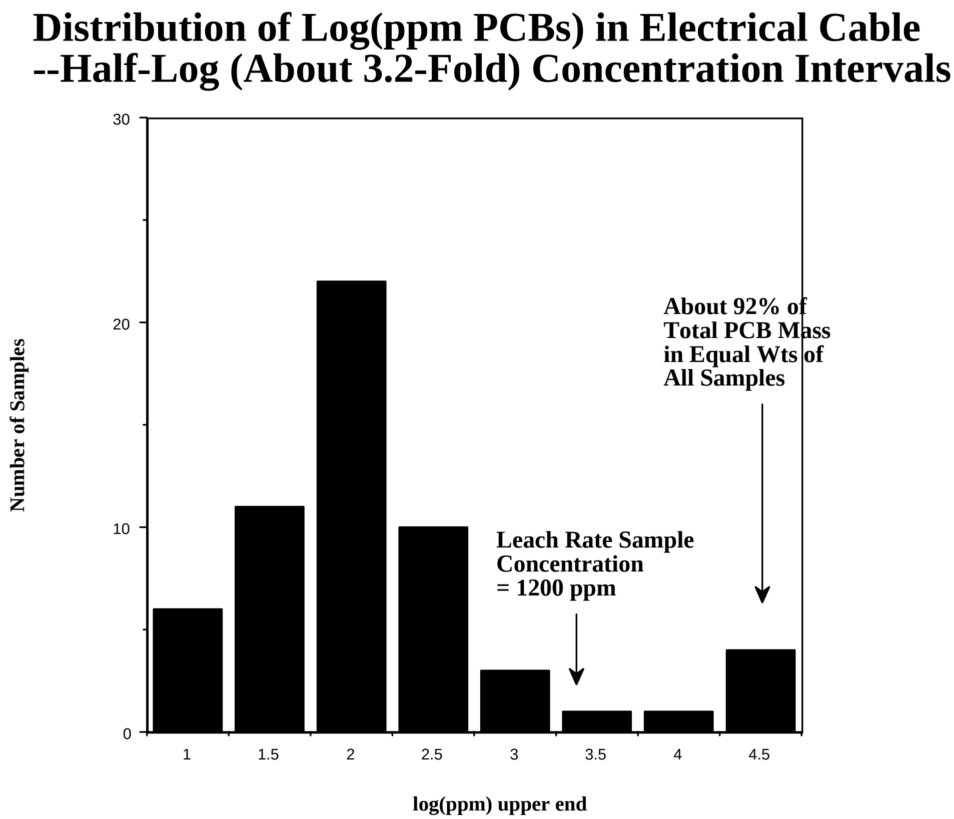
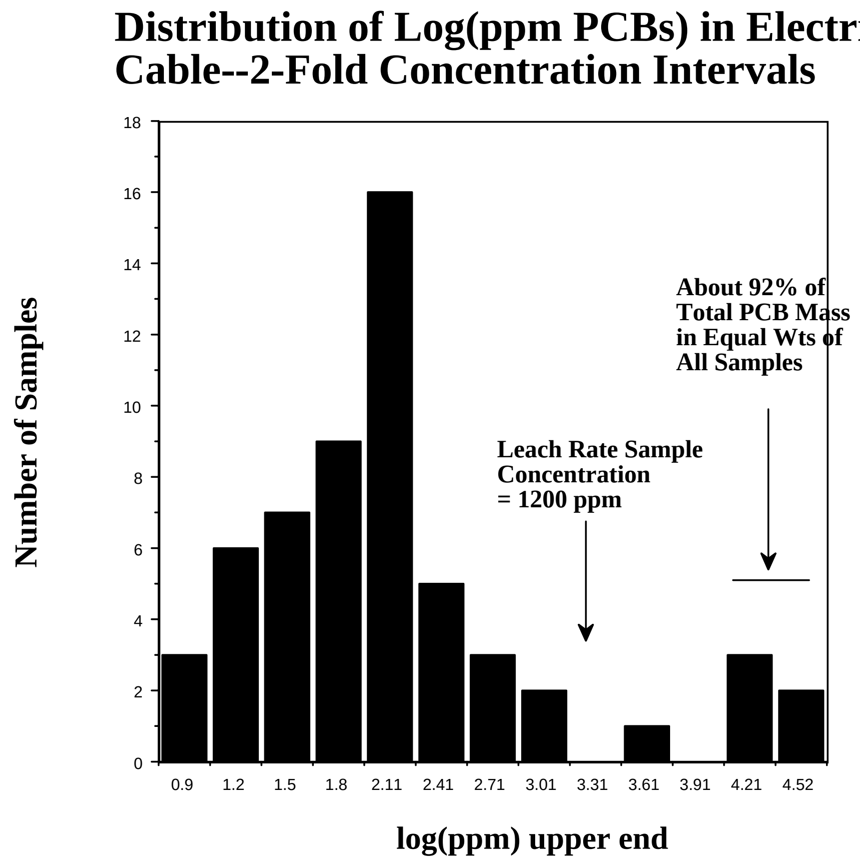


Figure 2



Dr. Taylor Eighmy

- Please provide comments as to whether biodegradation or encrustation processes should have considered in the study

Please see the comments below about leach test methodology. Mechanistic leaching studies really are needed to anchor the source term efforts.

However, the surfaces where leaching will occur will be immediately fouled by organic conditioning layers (minutes to hours), bacteria (hours to days), eukaryotes (days to weeks), and (depending on interior currents to support sessile filter feeders), fouling invertebrates such as bryozoans, hydroids, sponges, tunicates, etc. Assessing their relative role at retarding diffusive fluxes or mobilizing PCBs from the solids into the biofouling layers really will help to frame longer term PCB availability into the ship interior and subsequent trophic transfer.

There may be ways to develop second-order leach tests that look at the influences of organic conditioning films (OC partition sinks?), biofilms (lipid sinks? PCB-degraders?), and encrusting invertebrates (mobilization into lipid sinks? diffusional barriers?).

There has been some work in the biofouling literature on forming conditioning films or biofilms on inert porous membranes and then mechanically mating the mesh with surfaces to observe impacts to subsequent fluxes from the surface. This might be an avenue to explore for such secondary tests.

- Please provide comments as to the adequacy of the study to support the fate and transport models

There is relatively little literature and methodologies about leaching of organics such as PCBs from solid matrices. Some work on solidified/stabilized phenols, chlorophenols and organo-arsenics compounds in wood, and PAHs and dioxins in stabilized soils has been conducted. However, the standardized and widely-adopted methodologies developed for inorganic leaching may have suitable applicability here.

There are methods that might be adopted that may shed light on PCB congeners (including dioxin-like congeners) and homologue mechanistic leaching behavior from solids. Monolithic tank diffusion leaching tests or granular tank diffusion leaching tests (see leaching.net or see methods MT001.1 and MT002.1 in Kosson, D.S. *et al.* (2002) An Integrated Framework for Evaluating Leaching in Waste Management and Utilization of Secondary Materials, *Environ. Eng. Sci.* **19**: 159-204 as examples) can provide information on PCB availability in the matrices (what might be leachable), effective diffusivity of the PCBs (homologues or congeners) relative to diffusivity in water, matrix tortuosity, and some inference about leaching mechanism (surface-dissolution, diffusion, matrix depletion) if cumulative release (mg/m^2) is plotted versus log time. The methods can handle large solids of know geometry (monolithic) or granular materials that are compacted in a mold (granular).

A mechanistic basis may help to interpret the noisy initial empirical leaching data that have been observed (surface dissolution?) and lend themselves to better bounded long term leaching modeling from cable materials.

Given the desire of the Navy and others to further explore ship reefing, it may be prospective to adopt more standardized leaching protocols that shed light on mechanistic leaching behavior

The use of tank leaching tests (either for larger solids with defined geometries in a monolithic test or smaller, more particulate materials in a granular test) does not require any agitation to reduce external diffusional resistances or concentration build-up in the leachant as leaching intervals are selected that prevent concentration build up. This may obviate the need for using the stainless steel mesh and fiberglass filter material to prevent loss of “fines” containing PCBs.

The amount of material that is leached can be significant (e.g., kilograms) so that biases introduced by small sample sizes or problems associated with PCB congener BDLs may not be a problem.

The adoption of such tests will require some development work (e.g., selection of the extractants used to operationally define PCB congener or homologue availability, management of headspace, selection of appropriate leaching vessel and filtration apparatus materials), but this would be a valuable contribution to the knowledge base about organics leaching.

These diffusion-based tank leaching methodologies are of interest to USEPA OSW and were presented to the SAB during a 2003 consultation. They are also widely used outside the U.S. and are being 'normalized' by CEN in the European Union.

The current tests developed by the Navy are probably amenable to interpretational methods used by the above mentioned tests and may be an appropriate starting point for mechanistic studies.

- Please comment on whether the non-liquid PCB materials selected for evaluation were sufficiently representative of PCB materials on the ex-Oriskany and other vessels to provide a basis for evaluating ship sinking.

There is an apparent wide variability in congener, homologue or Aroclor content in the materials. This is based on a limited sample size from some Navy vessels. It might be appropriate to expand the materials sampling program along with adoption of leaching methodologies to build a better PCB content, PCB availability, and PCB leaching database for shipboard materials. These data can better refine distributions of parameters for use in probabilistic models.

Dr. Randy Maddalena

- Please provide comments as to whether biodegradation or encrustation processes should have been considered in the study.

The intent of the leach rate study was to measure screening level emission rates from the material in as highly controlled environment so that the results could be extended to different scenarios. There was some modeling done by the authors to demonstrate that degradation of the source material (BHI, cable, gasket, …) would not significantly increase/alter the release rate. However, I don't quite follow the logic. Intuitively I would expect the release rate to increase proportionally to surface area and as particles erode from the surface of the source material the surface area for exchange will increase dramatically. In addition, as erosion occurs, "fresh" pcb deeper in the material will be available for release. Future work might want to look closer at the impact of aging of materials in ocean water even if only in a more detailed modeling experiment and the role of biological organisms on the release rates should be discussed in more detail. The document references experience with anti-fouling agents as an example of how incrustation can reduce release of materials from the surface. First order estimates of these processes will be necessary in the future although I'm not sure how feasible it would be to a apply to the ex-ORISKANY at this point.

My personal feeling is that an even more basic source term input should be derived from the leaching study results. What I would be interested in is the mass transfer rate out of a particular material. Most of the measurements in the leach study had no apparent change in the chemical mass (or inventory) over the course of the experiment but the release rate decreased slightly over time indicating to me that the diffusion path length may be increasing for the PCB in the contaminated media. This has been shown to happen in the literature as where mass transfer responds as a function of time because surface is depleted and diffusion path length increases. I would consider using the data provided in the study to evaluate a more mechanistic conceptual model where diffusivity is constant (based on material) but path length changes with time so that one could relate to any shipboard material and starting concentration. This conceptual model could also consider changing surface area although the particles released from the surface might already be reduced in PCB (an issue for probabilistic modeling).

- Please provide comments as to the adequacy of the study to support the fate and transport models (PRAM and TDM).

I am not entirely clear on what values were used in the steady state runs but the experiments should be more than adequate with the following single concern. The goal was to get a maximum release rate by minimizing buildup in the aqueous phase. This was apparently accomplished by changing out the bottle each week regardless of whether a sample was collected. My concern is that the bottles do not seem to have been extracted (or solvent rinsed). As a result, the mass that was sorbed to the bottle was lost from the analysis, which could significantly bias the results down so the experimental results might under predict the release rate. If 50% of the aqueous PCB is lost to glass and 6-8 bottles are used for a given leachate that could equate to a huge under prediction of the release rate. At the very least I would suggest scanning the literature for estimates of loss to glass and use that to “correct” the measured release rates.

The assumption that the raw empirical data is representative of behavior in a complete vessel is not okay. Given the variability in the material across the vessel the likelihood of a zero release rate for a given material and time step is vanishingly small. There needs to be some processing of the release rate data to account for the very large scale-up from lab to Ex-Oriskany.

- Please comment on whether the nonliquid-PCB materials selected for evaluation were sufficiently representative of PCB materials on the ex-Oriskany and other vessels to provide a basis for evaluating ship sinkings.

Having no experience with navy vessels I am not able to comment on what materials onboard may contain PCBs. My only concern is that given the age/history of the vessel and the use of PCBs over the years I would expect that all surfaces in the ship would have some levels of PCB residue. There is a large surface area and initial release from these impervious surfaces is likely to be very high (rapid) (See Marion Diamonds work on impervious surfaces).

2. TIME-DYNAMIC MODEL (TDM) Documentation, May, 2005 (Draft Final)

- Please provide recommendations as to the TDM's applicability in ship reefing (i.e., is its short term fate and transport algorithm accurate and applicable).

- Please offer recommendation on the appropriateness of TDM's fate and transport outputs for input in PRAM's exposure algorithm and the resulting comparability of the short- versus long-term exposure results.

- Please comment on the sufficiency of the documentation describing the TDM approach, limitations, and uncertainties?

- Please comment on the soundness of the assumed pycnocline to bound the volume into which PCBs are initially distributed.

- Please make recommendations regarding the accuracy, and/or reasonableness, of TDM's approach, assumptions, inputs, equations, and calculations in regards to the overall prediction of direct PCB exposure to humans and marine organisms.

- Please provide specific opinion regarding TDM's transferability to other naval reefing applications including scenarios that include multiple ships reefed in close proximity.

Dr. Laura J. Steinberg

Bullet one – what the model was meant to address – clear and well-done. Particularly instructive to learn that it had its origins in a Navy model, and was not created for this specific application.

Other bullets:
Since this is a pre-existing model, has it ever been tested for its original Navy use? Sensitivity analyses? Lots of assumptions – do they matter?
e.g. Path length, internal current, eddy diffusivity, foc = .01, sediment mixed to 10 cm, (what happens in the sediment – this is not discussed anywhere!), DOC at 0.6 mg/l, TSS at 10 mg/l, no PCBs in the water above the ship. Should use a probabilistic model, not a point estimate.

Seems to me there will definitely be short-circuiting within the ship, so that there is some build-up concentrations in some areas (although I see this is "addressed" by having no internal structure to the ship).

Recommend that measurements be taken after this first ship is sunk, and the model respecified or re-calibrated for multiple ships, or additional sinkings. – could certainly measure the flow rate through the ship, for example.

The most likely value for DOC and TSS path length may be 37 m (fig 2-1c), but the expected value is what should be used, and this looks to be more than 37 m.

Adsorption assumption of 99% of equilibrium value uses some very old data. Newer data has indicated that "full" sorption may take place over much longer time periods thus 54.7% of equilibrium value may be incorrect. Furthermore, there has been much more mechanistic work done on PAHs sorption/desorption characteristics since 1982 that could inform this model.

You're dealing with very small aqueous concentrations here, and the numerics get pretty tricky. And yet the numerics are not discussed at all.

How does PCB enter the sediment? It should be available as a sink – both for sedimenting suspended solids and for sorption of dissolved PCB. Really need a diagram of the model compartments and boundary conditions. What is assumed to happen at the edge the 3000 meter ellipse?

What about major events? Is the ocean bottom subject to resuspension events during major storms? Probably, yes, these periodic events will add more PCB to the sediments.

Don't understand how PCB dispersal is "assumed to be radially symmetric around the ship" (p. 2-3) but PCB's are only released down-current from the ship – need a diagram.

How is the water moved from elliptical ring to elliptical ring? By advection, or turbulent diffusion? If advection, how fast, and are the currents really away from the ship – that seems odd.

Don't understand last paragraph of pg. 2-3. typo, 6th line of pg 2-5, made it very difficult to understand the concept being presented. In some key places, the writing does not use proper English: 5 lines from the bottom of p. 2-5 – what does "allowed" mean here?

Not sure what "flux" is referring to section 2.1.3 – flux is usually mass per unit area per time. Basically, do not understand what occurs within each bin per one-minute interval. Do not understand section 2.1.3.

Figures in Appendix C are not clear – why is mass shown along with concentration on same graph? Also, shows sediment concentrations, but there is not discussion of sediment dynamics at all. Can't tell that the total released in the ship equals the total retained + the total mass lost at the model boundaries.

Multiple ships in close proximity:
Given the low concentrations of PCB's, I expect that the partition constants would change only in a minor way with the added PCB's. However, the additional ships may change the hydrodynamics around the ship. In addition, the plumes from the ships may overlap, so that the concentrations are higher, but these could probably be considered additive.

The 0-60 outputs should not be arithmetic means but should be weighted by the volume within each bin (top of page 2-14).

Section 2.4.2
I'm not sure I believe that the assumption about the vessel hull is a conservative one. Given that the leach rates are set in Appendix C, there is no question that the same quantity of PCB will released no matter what the hull porosity assumptions are. However, a slower flow rate through the ship would result in a higher residence time and therefore higher concentrations coming out of the ship, but in less volume of water. So, it's not clear if the total mass released per day, say, would be more or less than currently.

Dr. David Dzombak

Summary of discussion of TDM charge questions [in the order we discussed them]

3. Is the documentation describing the approach, limitations, and uncertainties sufficient?
The documentation of the TDM is inadequate. A diagram showing the boundaries and spatial discretization of the flow domain needs to be provided, and the model details (differential equations, boundary conditions, initial conditions, solution technique) need to be provided.

1. Is the TDM fate and transport algorithm accurate and applicable to ship reefing?
5. Is the TDM and its approach, assumptions, inputs, equations, and calculations appropriate for the overall prediction of direct PCB exposure to humans and marine organisms?
[one response to these similar questions]

Hydrodynamic model.
The hydrodynamic model employed in the TDM is unclear and appears to have problems in its formulation. It appears that the ship is handled as a source for water flow, with water emanating in all directions from the ship. This is not physically justifiable, and even if it were a hypothetical scenario, a source function is not defined. The outer boundary of the flow domain, with a specified boundary conditions, is not defined. The hydrodynamic model apparently used is not consistent with advection in a time-varying bottom current flow direction.

Internal flow velocity. The internal-ship flow velocity assumed (1/100 of external flow velocity appears to have been selected rather arbitrarily.

PCB Sorption/Desorption Model

The use of a fixed amount of irreversible sorption for all homolog groups based on the 1982 paper by DiToro and Horzempa on sorption-desorption of hexachlorobiphenyl is difficult to justify. PCB congeners in different homolog groups exhibit very different sorption-desorption behavior as demonstrated by Oliver (Chemosphere, 14, 1087-1106, 1985) and Ortiz et al. (J. Environmental Engineering, 130, 126-135, 2004), and others. Also, the phenomenon of irreversible sorption of hydrophobic organic compounds on soils and sediments has been the subject of additional study since the work of DiToro and Horzempa. Some of the relevant published work is as follows:

Alexander (2000) "Aging, Bioavailability, and Overestimation of Risk from Environmental Pollutants." Environmental Science and Technology, 34, 4259-4265.

Chung and Alexander (1998) "Differences in Sequestration and Bioavailability of Organic Compounds Aged in Dissimilar Soils." Environmental Science and Technology, 32, 855-860.

Northcott and Jones (2001) "Partitioning, extractability, and formation of Nonextractable PAH Residues in Soil. 1. Compound Differences in Aging and Sequestration." Environmental Science and Technology, 35, 1103-1110.

Northcott and Jones (2001) "Partitioning, extractability, and formation of Nonextractable PAH Residues in Soil. 2. Effects on Compound Dissolution Behavior." Environmental Science and Technology, 35, 1103-1110.

Equilibrium partitioning. The consideration of instantaneous or rapid (within 24 hours) phase partitioning of PCBs in the model was surprising considering the objective to predict PCB concentrations as a function of time. Justification for the approach used for reaction rates in the model needs to be presented.

Sensitivity analysis. A sensitivity analysis is needed for the TDM to identify critical parameters.

4. Is the pycnocline that was included in order to bound the volume into which PCBs are initially released well justified?

While there is information to indicate the existence of a pycnocline near the depth used in the model at various times of the year, the variation of the depth and existence of the pycnocline are not addressed. A sensitivity analysis is needed. Further, it appears that the pycnocline was invoked in order to provide, for conservative analysis, a smaller water volume for the initial distribution of released PCBs. This should be stated explicitly.

2. Are the TDM fate and transport outputs appropriate for input to PRAM's exposure algorithm? Is the TDM the appropriate counterpart to PRAM for

short-term rather than long-term exposure assessment?

The manner in which the TDM time-varying output concentrations are averaged to feed into the PRAM exposure model needs to be explained more clearly. The justification for the averaging needs to be made more clear.

An annular water compartment closer to the ship should be considered.

The questions about the formulation of the coupled hydrodynamic and contaminant transport TDM need to be resolved before the adequacy of the TDM as a forerunner to the PRAM can be fully assessed.

6. Is the TDM transferable to other naval reefing operations applications, including scenarios that include multiple ships in close proximity?

The hydrodynamic formulation in the TDM needs to be checked and fixed before the usefulness of the model for naval reefing operations can be assessed.

The TDM (and the PRAM) need to be validated before their usefulness for other applications can be confirmed.

Dr. Taylor Eighmy

One overarching observation with respect to TDM is the how the source term is handled. The way the ship is scuttled (through hull openings allowing for exterior communication, interior bulk head door welded open or removed) will likely impact PCB fluxes from sources, greatly impact diffusive and advective transport within the ship, and greatly impact the degree of encrusting biofouling community formation and their inclusion in trophic webs. The way that the ship is scuttled actually becomes a management tool for controlling risk. Without better description of this source term and its attendant local transport (especially advective) processes, it is difficult to comment fully on the model.

Generally, the documentation for TDM was not sufficient to evaluate the model structure, particularly the way mass is transported and conserved.

Given the importance of TDM to examine acute and sub-acute risk in the “initial phase,” an alternative model may be needed. One possible idea is to explore a multi-dimensional finite element fate and transport model for congeners or homologues that better articulates the ship’s interior, the near ship environment and the flow field. This model would replace TDM and perhaps parts of PRAM. Such models might better depict the geometry-dependant contaminant fluxes. Such models are also amenable to the addition of sinks and reactions with the modeled domains. These models are particularly powerful when coupled to a Bayesian probabilistic approach that can incorporate repetitive simulations (e.g., Monte Carlo is one) for distributed important variables. Such methodologies would be amenable to transfer to other sites. There are models that can do this that are commercially available.

- Please provide recommendations as to the TDM's applicability in ship reefing (i.e., is its short term fate and transport algorithm accurate and applicable)

TDM attempts to take early empirical leaching data as a source term and distribute it from the ship using something like a radial plug flow transport model driven by variable leach rates. The assumption of a point source, random chord length transport time and only aqueous phase partitioning to DOC and TSS is probably too simplistic given the surface-associated PCB surface dissolution phenomena, complexity of diffusive and advective transport processes within the hull (these are really not defined and become a management decision point if the ship's 5,000 compartment interior is made more or less exterior-accessible), and the likely short term sorptive sinks on the vessel interior surface where conditioning films will form. Certain assumptions regarding time steps and time frames for equilibrium partitioning (e.g., 24 h) may be invalid.

The assumption of 1/100th flow regime for the interior is very simplistic and may or may not be conservative. It is difficult to relate this term to the way that the hull and interior bulk heads will be prepared.

The way that defining equations for mass balances and transport are not clear and need documentation and, based on presentation, may not be suitable for reliably predicting near vessel mass transport.

- Please offer recommendation on the appropriateness of TDM's fate and transport outputs for input in PRAM's exposure algorithm and the resulting comparability of the short- versus long-term exposure results.

See source term comments above.

- Please comment on the soundness of the assumed pycnocline to be bound the volume into which PCBs are initially distributed.

Pycnoclines are likely more dynamic and variable.

- Please make recommendations regarding the accuracy, and/or reasonableness, of TDM's approach, assumptions, inputs, equations, and calculations in regards to the overall prediction of direct PCB exposure to humans and marine organisms.

I think that assuming that all PCB amplification is based on diffusion first into the aqueous phase neglects direct trophic transfer through diffusion into biofilms or encrusting organisms.

- Please provide specific opinion regarding TDM's transferability to other naval reefing applications including scenarios that include multiple ships reefing in close proximity.

At this time, it is not transferable.

3. Prospective Risk Assessment Model (PRAM) Documentation Version 1.4, May 2005 (Draft Final)

Dr. Dale Hattis

- Please provide specific recommendations regarding the sufficiency of PRAM's documentation.

OK, except for decimal errors in inventory tables; also discrepancy between PCB fraction in material of .185 for electrical insulation in Table 11 and "resultant estimate" of 2560 ppm in Table 10

- Are the data used to calibrate the PRAM appropriate

- Please comment on the soundness of PRAM's approach, assumptions, equations, and calculations in predicting direct PCB exposure, uptake, and food web transfers (including bioaccumulation algorithm).

Seems OK in theory but performance of bioaccumulation factors needes to be quantitatively evaluated in relation to the experimental data presented with respect to (1) systematic differences (bias) and (2) random errors. The some sensitivity analysis should be done on the model after correcting the apprarent systematic errors to the set of experimental data that is deemed relevant.

- The choice of ZOI is paramount to accuracy of PRAM's modeled predictions. Please provide recommendations regarding its definition, basis, dimensions, and overall scientific soundness.

OK for ZOI = 2

- Please provide recommendations regarding the accuracy of the PRAM's PCB congener forecasts in water and fish.

There is very little basis provided to evaluate this in the absense of a detailed comparison with the data from the other ship

- Please make recommendation regarding sufficiency of the ship's interior flow rate assumptions. Also comment on the potential usefulness of considering catastrophic weather effects on both interior and exterior flow rates.

I have no basis to drastically change the interior flow rate assumption. Weather events might suspend PCBs bound to sediments, but the model predicts very little accumulation in the sediments relative to the amount released to the general water of the Atlantic.

- Please provide specific opinion regarding PRAM's transferability to other naval reefing applications including scenarios that include multiple ships reefed in close proximity.

The prospect of additional ships means that it is important to analyze overall global transport issues.

- Lastly, provide comments on the sufficiency of the ex-Vermillion fish tissue study in calibrating and validating the PRAM.

<u>Dr. Michael C Newman</u>
<u>Secondary Reviewer</u>

1. Please provide specific recommendations regarding the sufficiency of PRAM's documentation.

On page 1-1, several statements indicate that PRAM was developed to assess risk to human health but the last paragraph of page 1-5 states that it was developed to estimate risk to human health and the environment. Later it is used to assess ecological risk. It appears that very minor rewriting on page 1-1 might eliminate this minor inconsistency.

The PRAM application here focuses solely on PCBs due to specific and understandable regulatory mandates of PCB Bulk Product Waste (40 CFR 761.62c). A reviewer could be concerned that there are other aryl hydrocarbon receptor(AhR)-related dioxin-like chemicals (e.g., dioxins or dibenzofurans) present and that they are not included in the TEQs. It would seem that a straightforward statement about other potentially present AhR contaminants would remove any concern of a reader. To assess the effect of PCBs without some statement about other chemicals that work by a similar mode of action will not be adequate for many assessors.

2. Are the data used to calibrate the PRAM appropriate?

The leaching experiments seemed to use appropriate materials and expressed rates in appropriate ways. Use of oxygen diffusion rates to handle PCB diffusion seems a reasonable compromise. The selection of partition coefficients seemed appropriate.

On page 2-2, the statement is made that no data exist to validate the model predictions. Couldn't the model's success with predicting fish tissue study concentrations for previously sunk ships (e.g., ex-Vermillion) contribute to a level of validation? Aren't PCB data available for the ex-Agerholm and ex-Vermillion in the publication, "Assessing the ecological risk of creating artificial reefs from ex-warships"? (see http://www.sdoceans.org/programs/s2r/Assessing%20Ecological%20Risks_SPAWAR.pdf)

3. Please comment on the soundness of PRAM's approach, assumptions, equations, and calculations in predicting direct PCB exposure, uptake, and food web transfers (including bioaccumulation algorithm).

The application of fugacity-based models seems very appropriate (page 1-7).

On page 2-28, the statement is made that PRAM "has not been updated to perform probabilistic risks to assess uncertainties." I would strongly recommend that this feature be added prior to the general use of PRAM for Naval ship reef creation activities.

The estimation via PRAM is fine but I am uncertain about the reason data from other artificial reefs were not analyzed using the conventional tools of scientists quantifying food web transfer of contaminants (i.e., nitrogen isotopes to define trophic status and subsequent regression against body concentrations) were not used to also produce straightforward statistical models. The uncertainties of the many constants in the PRAM model would make such independent reassurances valuable. These statistical methods are not as data demanding as the "biouptake" models discussed on page 1-8. A clear statement of the rationale for not using such statistical models, and instead, relying heavily on the "deterministic" PRAM is needed because this exercise is stated to be precedent setting. Also, in reality, the PRAM model has major "empiric" parts. Statistical aspects already are present, including the important examples of PCB release (page 2-4), statistically (QSAR)-derived partition coefficients (page 2-13), and assimilation coefficients (Equations 114 -117 on pages 2-90 to 2-92). There are significant issues associated with assimilation efficiency predictions (see page 2-91, fourth paragraph regarding highly lipophilic PCBs.) There are other examples within the model components. It is obvious that the modelers who developed and applied PRAM here understood the value of statistical models so some statement about why less mechanistic and complicated approaches were not used also would be useful.

Related to the above comment, the PRAM model assigns biota to discrete "trophic chain" categorys/levels yet biota usually interact trophically in a "web." Such a web of trophic interactions would be extremely, and perhaps unnecessarily, difficult to model, i.e., inclusion of all actual sources in Equation 95 (page 2-78) with PRAM. Perhaps a relatively simple model based on the relationship between * ^{15}N vs $[PCB_i]$ might be helpful in verifying or augmenting this complex model that must make simplifications. Note that, like fugacity models, the statistical models do not require knowledge of biomass. At the very least, post-decision monitoring using * ^{15}N information and $[PCB_i]$ might provide further insight with which to assess the appropriateness of assigning different species to "Trophic Levels" and also the risk associated with future reef building.

Does the recent Hurricane Dennis flipping of the USS Spiegel Grove (July 11, 2005) change any assumptions about the stability of the carrier once it is sunk? The 510 foot long Spiegel was sunk in 130 feet of water about 6 miles off Key Largo in 2002. It settled in the wrong position but, despite predictions, was flipped into the right position recently by Hurricane Dennis. Perhaps the larger size (888 feet long) of the carrier Oriskany and the planned deeper final depth (210 feet) makes this extremely unlikely but the ex-Oriskany will have an "extreme vertical profile"(page 3-4 of Document 3. Storm stability analyses provided estimates if the bow was into the storm generated waves. Given recent events, it might be reassuring to have more explicit details relative to the Spiegel Grove event.

4. **The choice of ZOI is paramount to accuracy of PRAM's modeled predictions. Please provide recommendations regarding its definition, basis, dimensions, and overall scientific soundness.**

The ex-Oriskany is documented on various web releases to be the largest Navy ship to be sunk for reef creation, therefore, there is uncertainty associated with this activity. Regardless, the reasoning given for the ZOI dimensions are sound, and a 2 to 5 multiplier (of the ex-Oriskany's volume) is adequate. A factor of 2-2.5 for near-field foragers and 4-5 for less reef-related fish are reasonable.

Because there are plans for additional artificial reefs in the area, any future assessments will need to include the influence of nearby reefs, e.g., fish within an immediate zone may have received a significant previous dose while residing/feeding at another nearby reef. Treatment of a mosaic of zones-of-influence similar to those commonly applied in metapopulation models will be warranted.

5. **Please provide specific opinion regarding PRAM's transferability to other naval reefing applications including scenarios that include multiple ships reefed in close proximity.**

Please see comments above relative to multiple-reef scenario.

6. **Lastly, provide comments on the sufficiency of the ex-Vermillion fish tissue study in calibrating and validating the PRAM.**

According to Johnston et al.'s "Assessing the ecological risk of creating artificial reefs from ex-warships", there was a difference in liver size and lipid content for ex-Vermillion reef fish and those from natural reefs, resulting in higher PCB concentrations. (Lipid content is important in predictions, e.g., Equation 93 on page 2-77.) Some of the variability was attributed to differences in feeding and behavior. This issue needs to be addressed more before tissue data from this reef can be generally applied.

Dr. Taylor Eighmy

Please see some of the comments about TDM about ship preparation and how that influences the source term.

PRAM is a fugacity-based model. However, it does incorporate inter-compartmental transport processes (advective and diffusive). These may be overly simplistic with respect to how prevailing currents near the vessel are handled. Further, and dependent on how the ship is scuttled, the transport processes within the ship are perhaps too simplistic. The reliance on one current value from a NOAA observation buoy far a field may not provide the best bounds for prevailing current directions and strengths through out the water column. Prevailing Gulf current flow (e.g., the Gulf Loop and eddies) around a sunken vessel is undoubtedly complex. Is there a way to get more site-specific current data from physical oceanographic models that NOAA maintains?

Superimposed tidal currents and storm-based elliptical wave currents make this more complex. For instance (albeit simplistically and worthy of some examination), absent effects from a submerged ship, for long duration wind stress factors on the Gulf's surface water's greater than 20 m/s with long fetches (greater 200 km), resultant waves will likely have heights greater than 10m. Such waves can have periods of 15s. This approximately translates into wavelengths of hundreds of meters. Since the ratio of depth (d) to wavelength (L) is less than 0.5 (meaning the system is a shallow-water or transitional-water wave phenomena), elliptical water particle orbits are expected throughout the water column and will produce significant back and forth movement throughout the reef depth during each wave period.

Given the importance of PRAM to risk assessments, an alternative model may be needed. As noted for TDM, one idea is to explore a multi-dimensional finite element fate and transport model for congeners or homologues that better articulates the ship's interior, the near ship environment and the flow field. Such models might better depict the geometry-dependant contaminant fluxes. Monte Carlo simulations using the myriad variables that are needed for such a model would provide an appropriate and useful probabilistic framework. Such methodologies would be amenable to transfer to other sites.

- Please provide specific recommendations regarding the sufficiency of PRAM's documentation.

I found PRAM to be well documented.

- Are the data used to calibrate the PRAM appropriate?

A probabilistic approach would be better, especially for primary variables.

- Please comment on the soundness of PRAM's approach, assumptions, equations, and calculations in predicting direct PCB exposure uptake, and food web transfers (including bioaccumulation algorithm).

In relation to PRAM constructs, on any surface where macroinvertebrate biofouling will occur, there may site-specific forcing functions that dictate the timing of reef community establishment. For early colonizers, when do predominant larvae appear in the water column? Is there a strong seasonal basis? A two year time frame for establishment of exterior and interior communities may not be appropriate or may partially miss seasonally-induced cycles. The Navy's Marine Corrosion Test Facility in Key West may be able to help here.

I think one trophic transfer pathway has been ignored that does involve transfer via PCB dissolution into the aqueous phase.

This pathway would be based on PCB transfer to fouling biofilms or fouling invertebrates that is based on diffusion into the fouling organisms.

This pathway may be dependent on the precise means (a management decision) whereby the ex-Oriskany prepared for scuttling and use as a reef. If significant interiors surfaces are colonized by encrusting invertebrates, then transfer by surface wash-off or diffusion into the lipid fraction of the encrusting organisms may allow for PCB transfer to fish that graze on the encrusting organisms (e.g. White Grunts).

- The choice of ZOI is paramount to accuracy of PRAM's modeled predictions. Please provide recommendations regarding its definition, basis, dimensions, and overall scientific soundness.

I actually found ZOI to be a useful construct.

- Please provide recommendations regarding the accuracy of the PRAM's PCB congener forecasts in water and fish.

There is likely a balance between a focus on tPCBs, homologues, and specific congeners (including dioxin-like congeners). Specific congeners will likely drive HRAs and ERAs and thus may end up being the areas where models will help with management decisions.

- Please make recommendation regarding sufficiency of the ship's interior flow rate assumptions. Also comment on the potential usefulness of considering catastrophic weather effects on both interior and exterior flow rates.

As noted above, the description of the source term is directly affected by how the ship is prepared for use as a reef and scuttled. PRAM does not describe this process well. I also believe that wave induced directional currents and local cyclical currents associated with large storms are significant transport phenomena.

- Please provide specific opinion regarding PRAM's transferability to other naval reefing applications including scenarios that include multiple ships reefed in close proximity.

If PRAM is used (as opposed to a fate and transport probabilistic model), then its careful validation will be required before transferability.

- Lastly, provide comments on the sufficiency of the ex-Vermillion fish tissue study and validating the PRAM.

If a fate and transport probabilistic approach is not adopted and PRAM needs to be validated, then one part of the validation process (or plan) would be to use the ex-Vermillion data (target and background). Some additional sampling may be required. Other reefs might be samples at various trophic levels and be amenable to PRAM validation. Conservatism aside, I found it interesting that PRAM actually generated fish tissue concentrations in sentinel species on the same order as those measured from similar tropic levels seen at the ex-Vermillion site.

Timothy Thompson

Primary Reviewer

This was a challenging and complex undertaking by the Navy and it's team. A considerable amount of work and thought has gone into this project, and I commend this team for their focused and thoughtful exercise. While I believe that the case can be made from the information presented that the ex-ORISKANY PCB leach rates will not pose unreasonable risks to human health or the immediate environment, the PRAM model construct, supporting leach data, and lack of calibration are not suitable for a use as a long-term predictive model for future application. While the biological and human health modules may be appropriate for this, and future applications, PRAM should abandon the fugacity-based approach and incorporate field-based dynamic transfer components based on physical measurements made at the ex-ORISKANY after placement. My recommendations are made with this in mind.

Please provide specific recommendations regarding the sufficiency of PRAM's documentation.

1. The PRAM model documentation is adequate for the bioaccumulation and human health modules, but there are gaps in the discussion, presentation of algorithms, and presentation of physical transfer processes that are critically missing from the F&T module.
2. The readability of the document would be improved by having a technical editing. There are typos and references cited in the text that are not provided in the reference section and odd phrasing/syntax issues in other places within the document.
3. Parameterization of the model requires better documentation. Model parameters are presented in Tables 4 – 7 & 9, but are not complete in the sense of citing sources and/or justification why a specific parameter was selected. Transparency is important in this process – this could be improved on.
4. Recommend that the description of the physical system in Section 3 be moved to the front of the document. This is needed so the reader is presented with an understanding of the system boundaries before a description of the modeling

algorithms and parameterization occurs.

Are the data used to calibrate the PRAM appropriate?

PRAM has not been calibrated, in the true sense of the term. There are comparisons of the bioaccumulation factors calculated by PRAM, relative to those derived in other food web models, but that does not constitute calibration.

Please comment on the soundness of PRAM's approach, assumptions, equations, and calculations in predicting direct PCB exposure, uptake, and food web transfers (including bioaccumulation algorithm).

Fate and Transport Module (F&T)

1. The use of fugacity-based transport functions are not an appropriate choice for this specific use. The use of a fugacity-based model for PRAM appears to have been driven by two primary factors: conservatism and ease of construct. Fugacity-based models are conservative in the sense that transport and uptake functions are defined by simple boxes and that a slow diffusion between the boxes allows homogenous distribution and exposure of receptor groups to PCBs within the boxes. Within the context of a screening-level risk assessment (see comments on the ecological risk assessment), this may be appropriate for this specific use for this specific purpose only.

Since model construction is based upon diffusive transfer between the boxes, the algorithms used are based upon fairly simple physical properties of the system and PCBs (e.g., temperature, K_{ow}s). The need for site-specific information on hydrodynamics (currents, waves, temperatures) and predicting how that system might change in the presence of a sunken vessel need not be addressed in a fugacity model. However, it is likely that fugacity will be the exception, not the rule, in this high current environment.

The National Resource Council's recent recommendations concerning PCB modeling and risk assessment in *A Risk-Management Strategy for PCB-Contaminated Sediments* (NRC 2001) are relevant to the PRAM and attendant risk assessments. The NRC described the need for careful, site-specific hydrodynamics, transport, organic carbon behavior, and bioaccumulation modeling to support management decisions. F&T models based upon careful construction of site-specific hydrodynamic models have been the choice for evaluating specific PCB risks the bay of Green Bay, WI, the Hudson River, the Housatonic River, and DDT-related risks off the Palos Verde shelf in southern California. The PRAM documentation cites the use of fugacity modeling in CalTox and the Great Lakes Water Quality Initiative program as a justification for application here. However, both of those programs use the fugacity algorithms to develop water quality values in an open system. They were not intended for site-specific risk assessment.

PRAM may be able to provide input into whether PCBs will accumulate to unacceptable risk levels under low/no flow conditions for the ex-ORSIKANY, but is not suited to effectively forecast with any confidence what concentrations may be expected (for use in a long term monitoring program), how might concentrations change as a result of large storm events, or how PCBs would accumulate in an aggregate of similar sunken vessels

PRAM has limited, but probably acceptable application for the ex-ORISKANY. Recommend that fugacity-based transfer functions be replaced with more explicit accounting of hydrodynamic properties that will occur once the ex-ORISKANY is in place on the ocean floor. This will require collection of site-specific current data (including storm and wind function data), and some calibration from other sunk vessels such as the ex-VERMILLION.

Reference: NRC, 2001. *A Risk-Management Strategy for PCB-Contaminated Sediments.* Committee on Remediation of PCB-Contaminated Sediments. National Research Council. National Academy Press. Washington, D.C.

2.   **Hydrodynamic properties of the coastal system are not appropriately represented in the model.** As noted above, PRAM uses principally diffusive functions for the transfer of PCBs between the four water compartments in the model.. This is a function of selecting a compartmental transfer/fugacity-based model. However, except in quiescent lakes or deep ocean systems, physical transport processes are the dominant function. Given the relatively shallow site location in the Gulf, the current data that were provided, and that the resting substrate is course sand -- advection will likely be the principal mechanism by which PCBs move. Advection is poorly documented and under-represented in the model as it currently exists.

The inclusion in PRAM of a permanent pycnocline as a barrier to PCB transport is inconsistent with both fact and good science. PRAM describes four distinct water components; upper water column, lower water column, ship interior and porewater. The upper water column and lower water column are bounded by the pyconocline, listed as 15 m on page 3-2 (ca. 49.2 ft). In the PRAM V 1.4, the pycnocline is treated as boundary/barrier to PCB transfer; transfer functions in PRAM between the upper and lower water column are treated solely as diffusive functions. However, the resting depth of the ex-Orsiskansy is expected to be within 5 ft. of the pycnocline.

Depth of site (ft)	212	(text)
Height of vessel (ft)	157	(text)
Depth to top of vessel	55	(calculation)
Depth of pynocline	49.2	(text)

The depth to the pycnocline is listed in Table 9 as “consensus of the TWG”, which is likely an estimate; and it could be less or greater. Furthermore, the forcing of a boundary layer is apparently the result of requirement of the Navy to build in a higher degree of conservatism in the model by bounding the lower water layer. This is a decision based on policy, and not science. The model documentation should be clear that the rationale for confining the lower water column is based upon a policy decision, and not attempt to cloak it in a hydrodynamic/thermal feature.

PRAM in it's construction does not account for any flow from PCB sources directly into the pycnocline -- much less account for changes in current direction as a result of a "wall" of reef (i.e., bottom currents hitting the ship's surface and being carried upwards and through the pycnoline.   A corollary to the above, Figure 13 should be modified as pictorially it suggests that the sunken vessel will be well below the upper water column.

In addition to recommendation #1, the documentation should reflect that the restriction on the volume in the lower water column is a policy decision, and not a function of a hydrodynamic property (pycnolclime). PRAM should also explicitly consider direct inputs of PCBs into the upper water column from the ex-ORISKANY.

3.   **The investigations of PCB release-rates from selected shipboard solid material, as used in PRAM, do not provide sufficient information to adequately characterize exposure for human health and ecological risk assessment beyond the specific decision concerning the ex-Oriskany**. The Navy and its team are commended for a solid and thorough investigation into sources and levels of PCBs in the on-board materials of the ex-ORISKANY. The design of the leach-rate experiments is well-thought out and documented. However, as noted by other Panel members, the relatively small number of samples and the lack of replication are of concern. In preparing for the upcoming SAB Peer Review Panel, it is recommended that the Navy focus on using these data to gain approval for reef-placement of the ex-ORISKANY, and forgo seeking approval for PRAM for broader application in the Navy's overall ship-to-reef program. This Advisory believes that while it will be possible to gain approval for risk characterization for the ex-ORISKANY, the limited information on leach rate will not allow for a broader application of PRAM.

4.   **The exposure concentration scenarios for evaluating risks from the ex - ORISKANY can only be done as simple, bounded box models. As constructed, PRAM cannot support risk-based decision making for the broader Navy reef-vessel program.**

The fugacity-based construct of PRAM, coupled with the constraints introduced with the Zone of Influence and the artificial bounding of the lower water column with the pycnocline, means that PRAM functions as a simple bounded box model. Rather than attempt to define the model as a mimic of hydrodynamic conditions at the sink-site, it would be helpful if the model documentation acknowledge that PRAM is constructed as a series of boxes that in the absence of site-specific hydrodynamic information, only diffusive flux is incorporated as a transfer function. This would include the Zone of Influence.

To support the ex-Oriskany risk assessment, additional modeling runs are recommended that incorporate:

- Explicit acknowledgement of box-model nature of PRAM
- Probabilistic (Monte Carlo or Hypercube) analysis with at least following variables:
    - Mass of PCBs on the ex-Oriskany (bias sampling)
    - PCB release rates
    - Flow Rates (currents)

- Re-expressing the human health risks (for fish consumption only) as a probability of exceeding the cancer risk factors (probability of exceeding a 10^{-6} lifetime cancer risk)
- Re-expressing the ecological risk as a probability of exceeding an HQ of 1, 5 and 10.

5. **PRAM must undergo a careful, documented QC check.** While there is a reference in the PRAM documentation concerning independent third-party review by Dr. Keith Little of RTI International on the TDM and the PRAM models, these are listed as "personal communication". When the supporting documentation was requested, the Panel learned that "There are no formal reports from Dr. Keith Little, interface with Dr. Little has been informal via email and verbal communication between himself and other risk assessment/modeling team members." All of the derived equations in the documentation should be checked for accuracy, as well as transposing of the equations into the spreadsheet, and cross-checks within the spreadsheet.

Recommend a thorough, third-party QA check of the PRAM equations and spreadsheet functions.

6. **PRAM should be the subject of an external peer review.** Page 1-2 describes various reviews that were undertaken, but is unclear what components of PRAM were reviewed. For example, the PRAM documentation lists reviews in 2000 and 2001 by the Navy Environmental Health Center – those were probably associated with the human health modules in PRAM. Likewise, a review is listed by the RTI, but that appears to be associated with the draft leachate rates. Appendix G provides review comments by the TWG – those are principally associated with biological exposure assumptions used by the bioaccumulation module. Finally, the Second International Conference of Contaminated Sediments in Venice does not constitute peer review. There appears to be no formal, serious review of the F&T module in PRAM.

Recommend that the modeling peer-review panel be convened to review at a minimum the F&T and bioaccumulation modules in PRAM.

7. **A better explanation is required as to why PRAM undertakes the modeling of homologs, as opposed to total PCBs. Likewise, the omission of dioxin-like congeners is of concern for both the human health and ecological risk assessments.** The model definition on page 1-1 is very clear on the purpose – predict the concentrations of PCBs in reef fish and the subsequent human health risks to recreational anglers. Human (and ecological) health risks are evaluated based upon tPCBs, and dioxin-like congeners, so the reason for modeling homolog groups separately is puzzling.

Multiple studies have tried to derive determine specific PCB-congener uptake patterns from field-derived sediment, water, and tissue data. To date, the ability to define this mathematically has eluded the scientific community (see especially the work of Ross Norstrom with the Canada Ministry of Environment). Various explanations have included selective uptake, metabolism and excretion, internal dechlorination, and other factors (EPA 2005). The PRAM documentation addresses this issue in the assimilation efficiency discussion on page 2-91. Figure 9 attempts to use an Excel function to derive a parabolic representation of the scattered data; I doubt this would stand up to rigorous statistical analysis.

The major risk assessment/modeling efforts that have been conducted on the Hudson, the Fox River/Green Bay, the Housatonic, the Saginaw River, and more recently Willamette River in Oregon, the Lower Duwamish River in Washington, and False Bay in Vancouver BC have all observed this same problem. Modeling efforts at all of these sites have focused on total PCBs, and have either derived statistical relationships between tPCBs and the dioxin-like congeners, or have modeled those 6 – 8 specific congeners separately.

Recommend that future bioaccumulation modeling focus on total PCBs, but track specific dioxin-like congeners.

Reference: EPA 2005. Memorandum: Response to Ecological Risk Assessment Forum Request for Information on the Benefits of PCB Congener-Specific Analyses. United States Environmental Protection Agency, Office of Research and Development, National Center for Environmental Assessment, Washington, DC. NCEA-C-1315, ERASC-002F. March 2005. Available on the web at
http://www.epa.gov/oswer/riskassessment/pdf/1315-erasc-002f.pdf

Bioaccumulation Module

The bioaccumulation module appears to be adequately documented, and is consistent with accepted and current practice. With the caveat cited above concerning dioxin-like PCB congeners, this module is directly applicable to the decision concerning the ex-ORISKANY, and could have general applicability other systems. However, the Panel recommends that additional efforts be expended towards calibrating the module.

The choice of ZOI is paramount to accuracy of PRAM's modeled predictions. Please provide recommendations regarding its definition, basis, dimensions, and overall scientific soundness.

The choice of ZOI is only related to the degree of conservatism required in the assessment, and not on a rigorous evaluation of the hydrodynamic conditions or transport functions that will occur once the ex-ORISKANY is in-place on the ocean floor. Therefore, it is inappropriate to discuss it in terms of "accuracy". See previous comments on PRAM as a simple box model.

Please provide specific opinion regarding PRAM's transferability to other naval reefing applications including scenarios that include multiple ships reefed in close proximity.

As discussed above, in its current configuration, PRAM is not applicable beyond the specific decision for the ex-ORISKANY.

Lastly, provide comments on the sufficiency of the ex-Vermillion fish tissue study in calibrating and validating the PRAM.

The current representation of the ex-VERMILLION fish tissue calibration effort is insufficient to support PRAM. By the Navy's own presentation, there is no confidence in the estimates of PCB mass in the ex-VERMILLION. The inability of PRAM to adequately forecast fish tissue concentrations limits its utility. How the calibration effort was conducted is not clear in the documentation. Calibration of the food web component is done by modifying the various parameters including growth, dietary ingestion rates (prey percentage), lipid content, assimilation efficiencies, bioenergetics, and depuration rates. These are modified within the range of literature-reported values and run through the model until the predicted rates match the observed rates as closely as possible. If this occurred, it was not documented.

Please provide recommendations regarding the accuracy of the PRAM's PCB congener forecasts in water and fish.

As noted previously, PRAM can only be used to forecast how PCBs will transfer under quiescent conditions that support the fugacity assumptions. While it may be useful in the specific context related to the ex-ORISKANY, because of the uncertainty in PCB mass, leach rates, and by not incorporating hydrodynamic properties into the model, PRAM will likely not predict with any accuracy or confidence PCB bioaccumulation.

Please make recommendation regarding sufficiency of the ship's interior flow rate assumptions. Also comment on the potential usefulness of considering catastrophic weather effects on both interior and exterior flow rates.

It is hard to judge the sufficiency of the ship's interior flow rate assumptions without at least some field-measurements for comparison. While this may be sufficiently conservative, I recommend that the scientific literature be consulted to see if there are any values that can be used for comparison.

As noted previously, PRAM's overall utility is restricted by the absence of advective transfer algorithms – which are likely to be the dominant transfer functions in this system. Any reconstruct of the abiotic module should include both temperature and wind-driven currents and waves. Catastrophic weather events are consistently built into contaminant-transfer models; including PCBs and DDTs in the Great Lakes and off the Palos Verde shelf.

4. Ex-ORISKANY Artificial Reef Project Human Health Risk Assessment, June 2005 (Draft Final)

<u>Dr. Randy Maddalena</u>

As a general comment:

It would help to see one more paragraph in the executive summary that identifies the most important factors in the overall assessment (based on both sensitivity and uncertainty) and why the reader should believe that the values used for these factors in the model are correct (or conservative). This information is provided in some detail in the various documents and appendixes but as part of the Executive Summary the authors should bring these findings forward.

Specific charge questions:

- Please provide specific recommendations on the completeness of the exposure scenarios with regard to selecting the maximally exposed receptor and the length of the chronic exposure duration (i.e., should the initial 2-year pulse PCB release period be considered in chronic exposures).

Missing exposure pathways - Pregnant/nursing mothers and breast feeding infants. It is not clear to what extent this pathway has been considered but I would expect exposure for nursing infant to be greater than that of the parent at least for the period of time that an infant is nursing and assuming the mother has been eating fish from the reef for some time. This same concern also applies to embryo/fetus prior to birth. The toxicology may be very different for infants (and fetuses).

Near vessel compartment – I think the level III fugacity model is a good choice for this application given the level of uncertainty in key factors such as source term and PCB inventory on the vessel. I am not as concerned about the hydrodynamics as much as whether all necessary compartments are included. The authors used information about fish range to identify the inner ZOI but the way the model is structured with the elliptical compartments they may need to re-visit the width of that first compartment so it dilutes emissions as if all flow was one direction from the vessel. My feeling is that there is a near vessel zone where emissions will be more concentrated (i.e., a plume) on the down-current side of the vessel and that fish will reside in this zone at lease some part of there life. However, this may be a moot point if the assumption that the grouper spends 20% of its life in the vessel interior results in the main exposure for humans.

- Please provide specific recommendations regarding the exposure parameter selection for the diver scenario and whether its qualitative assessment is sufficient.

I don't have any comments at this point. It looks like exposures external to the vessel will be small so the diver is probably not an important pathway although there may be a transfer to the vessels delivering divers to the reef over long periods.

- Please provide recommendations regarding the accuracy of the PRAM's PCB congener forecasts in water and fish.

On page 4-10 there is discussion of an "adsorption out-stripped brake" that is used to make the mass balance work. I don't understand what this is. If a mass balance type model is used this will be accounted for by transfer factors so I don't understand why a correct factor is needed.

The numbers provided for mass of PCB remaining onboard the ex-ORISKANY are inconsistent. Page 2-2 indicates 330 kg of PCBs while Tables 4-1 and 4-2 indicate that the mean value is around 5000 kg with a "95% upper confidence limit for the mean" of about 56,000 kg. The larger number may actually be reporting the UCL of the data not the UCL of the mean but it is not clear.

What is the sediment burial rate in this region and would that result in a significant loss mechanism. I ask because one of my overall concerns is the addition to global inventory of PCBs but I don't have a sense of the amount that would get into global circulation from a release 200 feet down in the ocean. In the current PRAM setup almost all of the PCB emissions are lost from the model domain so the question would be whether that is a significant release to the environment or is the release likely to be sequestered in the deep ocean sediment?

It appears that release of PCB is on the order of 10 pounds per year while the threshold for reporting emission to the Toxics Release Inventory is at 1 pound per year. This further supports the need to assess potential regional/global impacts. The intake fraction (see D.H. Bennett et al) and/or global emission term (i.e., fraction reaching Great Lakes see MacLeod et.al. and BETR model) should be assessed as part of the overall *hazard* assessment. These chemicals are not your ordinary Superfund Site bad guys. They are listed as part of the global "dirty dozen" and as such should probably be assessed beyond the local exposure pathways but this might be a policy issue.

On specific modeling details, The exchange between inside and outside the ship is probably appropriately set at about 0.01 of external current but that should be a bit more rigorously supported because my sense is that the exchange rate is very important because it drives the concentration in the grouper. I do not see a need for more complex compartmentalized modeling of the interior of the vessel. The inclusion of wet/dry deposition for such a small air compartment is probably not necessary relative to advection out of the air compartment but I guess it does not hurt to include.

- Please comment on the selected risk assessment parameters used in the study including toxicity values and risk calculations. Are quantitative risk considerations missing (e.g., dioxin-like PCB risks)?

I have a concern about not including the dioxin-like pcbs in the risk calculation. I know this has received a lot of consideration but I'm not entirely clear why the dioxin-like PCBs were not specifically considered. From what I gather from the reports, I think it has something to do with the lack of tox data and the PRAM not providing congener specific predictions but those are both technical reasons. Excluding a chemical or pathway should be health-based not technical-based so I would recommend that these particular congeners be included in the assessment (as they were and for the reasons specified in the leach rate study).

On page 9-21 I do not understand why two exposure frequency numbers are presented. Is it two events per month for three months per year?

5. Ex-ORISKANY Artificial Reef Project Ecological Risk Assessment, June 2005 (Draft Final)

Dr. Michael C Newman

Primary Reviewer

1. Please provide specific recommendations regarding the sufficiency of PRAM's documentation.

On page 1-1, several statements indicate that PRAM was developed to assess risk to human health but the last paragraph of page 1-5 then indicates that it was developed to estimate risk to human health and the environment. Later it is actually used to assess ecological risk. It appears that very minor rewording on page 1-1 might eliminate this minor inconsistency.

The PRAM application here focuses solely on PCBs due to specific and understandable regulatory mandates of PCB Bulk Product Waste (40 CFR 761.62c). A reviewer could be concerned that there are other aryl hydrocarbon receptor(AhR)-related dioxin-like chemicals (e.g., dioxins or dibenzofurans) present and that they are not included in the TEQs. It would seem that a straightforward statement about other potentially present AhR contaminants would remove any concern of a reader. To assess the effect of PCBs without some statement about other chemicals that work by a similar mode of action will not be adequate for many assessors. As an extreme example to make the point that some discussion of other AhR-mediated toxicants should be included, would this same focus only on PCBs be adequate if a ship used during the Vietnam era to transport Agent Orange was to be sunk?

2. Are the data used to calibrate the PRAM appropriate?

The leaching experiments seemed to use appropriate materials and expressed rates in appropriate ways. The issue of material representativeness (given the high variability in materials) is a concern. Some estimation of and inclusion of variability would be useful. Use of oxygen diffusion rates to handle PCB diffusion seems a reasonable compromise. The selection of partition coefficients seemed appropriate.

On page 2-2, the statement is made that no data exist to validate the model predictions. Couldn't the model's success with predicting fish tissue study concentrations for previously sunk ships (e.g., ex-Vermillion or ships already sunk in the area of the proposed ex-Oriskany sinking) contribute to a level of validation? Aren't PCB data available for the ex-Agerholm and ex-Vermillion in the publication, "Assessing the ecological risk of creating artificial reefs from ex-warships"? (see http://www.sdoceans.org/programs/s2r/Assessing%20Ecological%20Risks_SPAWAR.pdf)

3. Please comment on the soundness of PRAM's approach, assumptions, equations, and calculations in predicting direct PCB exposure, uptake, and food web transfers (including bioaccumulation algorithm).

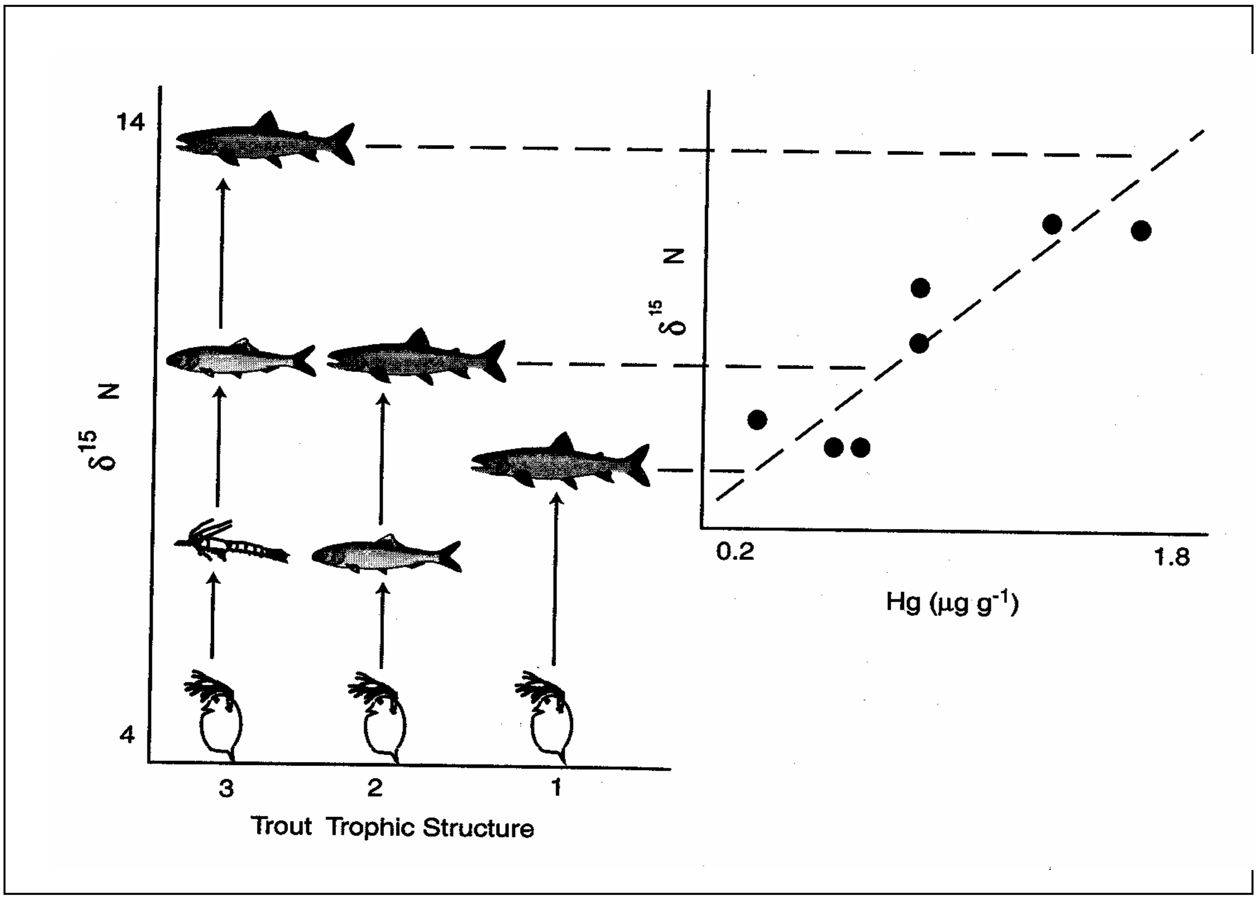
The application of fugacity-based models seems very appropriate (page 1-7).

On page 2-28, the statement is made that PRAM "has not been updated to perform probabilistic risks to assess uncertainties." I would strongly recommend that this feature be added prior to the general use of PRAM for Naval ship reef creation activities. Many important qualities were quite variable (e.g., PCB concentrations in the various solid materials) so a probabilistic approach seems essential. Related to this, some of the sampling of materials for exposure or flux estimates should be designed with the understanding that there is high variability, i.e., sampling heterogeneous materials.

The estimation via PRAM is fine but I am uncertain about the reason data from other artificial reefs were not analyzed using the conventional tools of scientists quantifying food web transfer of contaminants (i.e., nitrogen isotopes to define trophic status and subsequent regression against body concentrations) were not used to also produce straightforward statistical models. The uncertainties of the many constants in the PRAM model would make such independent reassurances valuable. These statistical methods are not as data demanding as the "biouptake" models discussed on page 1-8. A clear statement of the rationale for not using such statistical models, and instead, relying heavily on the "deterministic" PRAM is needed because this exercise is stated to be precedent setting. Also, in reality, the PRAM model has major "empiric" parts. Statistical aspects already are present, including the important examples of PCB release (page 2-4), statistically (QSAR)-derived partition coefficients (page 2-13), and assimilation coefficients (Equations 114 -117 on pages 2-90 to 2-92). There are significant issues associated with assimilation efficiency predictions (see page 2-91, fourth paragraph regarding highly lipophilic PCBs.) There are other examples within the model components. It is obvious that the modelers who developed and applied PRAM here understood the value of statistical models so some statement about why less mechanistic and complicated approaches were not used also would be useful.

Related to the above comment, the PRAM model assigns biota to discrete "trophic chain" categories/levels yet biota usually interact trophically in a "web." Such a web of trophic interactions would be extremely, and perhaps unnecessarily, difficult to model, i.e., inclusion of all actual sources in Equation 95 (page 2-78) with PRAM. The breakdown in Table 5 does not adequately describe all important differences among species within a "Trophic level." Perhaps a relatively simple model based on the relationship between * ^{15}N vs $[PCB_i]$ might be a helpful alternative for verifying or augmenting this complex model that must make dubious simplifications. Note that, like fugacity models, the statistical models do not require knowledge of biomass. At the very least, post-decision monitoring using * ^{15}N information and $[PCB_i]$ might provide further insight with which to assess the appropriateness of assigning different species to "Trophic Levels" and also the risk associated with future reef building. (Some related references include Cabana, G., Tremblay, A., Kalff, J., Rasmussen, J.B., 1994, Pelagic food chain structure in Ontario Lakes: A determinant of mercury levels in lake trout (*Salvelinus namaycush*), Can. J. Fish. Aquat. Sci., 51, 381-389; Cabana, G., and Rasmussen, J.B., 1994, Modelling food chain structure and contaminant bioaccumulation using stable nitrogen isotopes, Nature, 372, 255-257, Kidd, K.A., Schindler, D.W., Hesslein, R.H., and Muir, D.C.G., 1995, Correlation between stable nitrogen isotope ratios and concentrations of organochlorines in biota from a freshwater food web, Sci. Total Environ., 160/161, 381-390; Broman, D., Naf, C., Rolff, C., Zebuhr, Y., Fry, B., Hobbie,J., 1992, Using ratios of stable nitrogen isotopes to estimate bioaccumulation and flux of polychlorinated dibenzo-*p*-dioxins (PCDDs) and dibenzofurans (PCDFs) in two food chains from the northern Baltic, Environ. Toxicol. Chem., 11, 331-345.) Post-reefing monitoring will be very helpful for risk decisions for future artificial reef production.

As a related issue, the same species can vary in its trophic position. Here is an example of lake trout from eight Canadian Shield lakes (Figure from Newman & Unger (2003), Fundamentals of Ecotoxicology, CRC/Lewis Publishers; Modification of Fig. 2 & 3 of Cabana & Rasmussen. 1994. Nature 372: 255-257.)



As a minor point that may reflect the nature of the bias here toward "Trophic Levels", the term "Guild Representatives" is used when referring to tropic level representatives. Guilds are groups of organisms in an ecological community that act similarly (i.e., have similar or very similar niches). Scrappers in a cold, high gradient stream can constitute an ecological guild but all benthic invertebrates around an artificial reef are not from the same guild. Such a group could easily include a predatory annelid and a suspension feeding bivalve that are not functioning similarly, but simply occupying a general type of habitat. The use of the term, guild, here is not correct and likely reflects the group's preoccupation with trophic levels at the expense of other potentially important ecological qualities/interactions.

Does the recent Hurricane Dennis flipping of the USS Spiegel Grove (July 11, 2005) change any assumptions about the stability of the carrier once it is sunk? The 510 foot long Spiegel was sunk in 130 feet of water about 6 miles off Key Largo in 2002. It settled in the wrong position but, despite predictions, was flipped into the right position recently by Hurricane Dennis. Perhaps the larger size (888 feet long) of the carrier Oriskany and the planned deeper final depth (210 feet) makes this extremely unlikely but the ex-Oriskany will have an "extreme vertical profile"(page 3-4 of Document 3. Storm stability analyses provided estimates if the bow was into the storm generated waves. Given recent events, it might be reassuring to have more explicit details relative to the Spiegel Grove event.

4. **The choice of ZOI is paramount to accuracy of PRAM's modeled predictions. Please provide recommendations regarding its definition, basis, dimensions, and overall scientific soundness.**

The ex-Oriskany is documented on various web releases to be the largest Navy ship to be sunk for reef creation, Therefore, there is uncertainty associated with this activity. Regardless, the reasoning given for the ZOI dimensions are sound, and a 2 to 5 multiplier (of the ex-Oriskany's volume) is adequate. A factor of 2-2.5 for near-field foragers and 4-5 for less reef-related fish are reasonable.

Because there are plans for additional artificial reefs in the area, any future assessments will need to include the influence of nearby reefs, e.g., fish within an immediate zone may have received a significant previous dose while residing/feeding at another nearby reef. Treatment of a mosaic of zones-of-influence similar to those commonly applied in metapopulation models will be warranted. This is particularly relevant as the TDM model predicts that much of the PCB mass leaves the domain of the model around each ship (reference: Dr. Ken Richter's presentation).

5. Please provide specific opinion regarding PRAM's transferability to other naval reefing applications including scenarios that include multiple ships reefed in close proximity.

Please see comments above relative to multiple reef scenario.

6. Lastly, provide comments on the sufficiency of the ex-Vermillion fish tissue study in calibrating and validating the PRAM.

According to Johnston et al.'s "Assessing the ecological risk of creating artificial reefs from ex-warships", there was a difference in liver size and lipid content for ex-Vermillion reef fish and those from natural reefs, resulting in higher PCB concentrations. (Lipid content is important in predictions, e.g., Equation 93 on page 2-77.) Some of the variability was attributed to differences in feeding and behavior. This issue needs to be addressed more before tissue data from this reef can be generally applied.

Please comment on the need for acute ecological health concerns given the results of the TDM and reef species colonization timing and uncertainty.

I believe that there is not much chance of acute ecological health concerns.

Dr.Gregory Biddinger

- Our goal is to give you a "cold eyes" review. So feel that both technical and more general impressions are fair game.
- The Ecological Risk Assessment FEELS like an after thought; The exposure tools are a race car and the ERA is a push cart.
  - Language in the PRAM documentation indicates Exposure regime was designed for Human Health Risk Assessment.
  - Focus on Ecological Service of Sport fishing and tropic transfer to anglers supports this as well
- The Ecological Risk Assessment is a screening risk assessment against Benchmarks (details on pages 5-6 and 5-7).
  - Ecological benchmarks such as Water quality criteria and Sediment ERM's and not toxicological endpoints, not linked to a dose-response curve.
  - Benchmanrks should be used as pass/fail to screen in or out of a risk assessment. (listed on table 6)
  - Exception - Food chain Benchmarks - TRV based on dose-response curve and Critical Body Residues. (NOED's and LOED) -See page 5-9
- HQ's should be based on dose-response data not regulatory
- The Categorization of HQ's less than 1.0 as having some risk significance is wrong and misleading.
  - Tables (Page 5-18 and 5-19) are inappropriate. They overwork the concept of the hazard quotient.
- Assessment endpoints (sec. 1.2 para. 2 and table 2) - Tissue concentrations are not assessment endpoints for survival, growth and reproduction (see definitions)
  - They are also not Measurement Endpoints for these impact categories.
  - TRV's could be linked to some effects (for adults) but there is no discussion of which effect endpoints are considered.
    - See discussion on herring gull page 5-11

- Bioaccumulation in food chain is not an effect. Its a detoxification mechanism and an indication of exposure not effect.
- The food chain is incomplete without consideration of Bacteria and other lower order species (Gary Saylor/Taylor Eighmy)
  - These organism may make of much of the mass of the biological portions of the ecosystem and are the engines for much of the respiration, decommission and transport of materials into the environment.
  - Need to address or say why they won't effect you estimate of risk.

- Risk Communications / Benefits discussion- Would be advised to more fully discuss relative risk in light of environmental benefits (sect 3.2 page 3-3).

Timothy Thompson
Secondary Reviewer

- Please comment on whether the selected ecological communities and trophic relationships are inclusive and representative of reefing sites. Please also comment on whether the assessment endpoints and conceptual model are acceptable.

  - The conceptual model, ecological communities, and trophic relationships are generally acceptable for the intended purpose.
  - The risk assessment is a good, functional screening-level risk assessment (SLRA). The document (title and text) should be edited to reflect that.
  - The focus on trophic levels and specific species is appropriate for an SLRA.
  - Additional documentation is needed to understand the species and size classes that are being used to model exposure to mammals and birds.

- Please comment on whether sufficient toxicological benchmarks are used to be confident that risks to Threatened and Endangered species and other species of critical concern are adequately considered.
  - Recommend that more recent risk assessments for tPCBs be evaluated to supplement the toxicological benchmarks in the SLRA. The Housatonic River ERA at http://www.epa.gov/ne/ge/thesite/restofriver-reports.html#Eco would be a good place to obtain the most recent values for minks, birds, and in particular amphibians – which may be a better surrogate for turtle-PCB toxicity reference values.
  - The sources for reference values in Table 6 should be better documented

- Please comment on the soundness and inclusiveness of the selected assumptions, the inherent uncertainties, and the overall limitations of the ecological risk assessment. Are additional data analyses or risk characterizations warranted to support the conclusions about ecological risks?
    - As noted, the document needs to be revised to reflect it is a screening-level risk assessment.
    - Within the context of the Advisory Panel's previous comments to PRAM, the SLRA should be revised to acknowledge the limitations and uncertainties associated with PRAM, and the subsequent effects on the SLRA conclusions.
    - A probabilistic forecast should be done with a revised PRAM box model, and that those probability estimates should be incorporated into the SLRA.
    - Recommend that specific congener modeling be incorporated into PRAM, and subsequently incorporated into the SLRA.

- Please specifically identify fundamental analytical flaws and/or key data gaps that might negate or restrict the use of this Ecological Assessment in supporting risk-based reefing (ex-Oriskany as well as other vessel reefing).

    - See previous comments that identified limitations to PRAM that restrict the information derived solely to the decision on the ex-Oriskany, including the SLRA.
    - The Panel finds that while ultimately the methodology applied in the SLRA could be applied to other vessel reefing sites in the Gulf region, the limitations to PRAM must be overcome before a broader application could be approved.

- Please comment on the need for acute ecological health concerns given the results of the TDM and reef species colonization timing and uncertainty.
    - While a discussion of the potential for acute ecological health hazards is warranted in the SLRA, it is unlikely that the levels of PCBs on the ex-Oriskany will cause any acute toxicity.