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MEMORANDUM

31 January 2005

TO: Daryl Weatherhead and Ron Evans, EPA

FROM: James Neumann, Industrial Economics, Incorporated

SUBJECT: Summary of Mercury Exposure Methodology Peer Review

Industrial Economics, Inc (IEc) was contracted to coordinate a critical review of the November 2004 document prepared by RTI International entitled *Assessment of Mercury Exposures to Women of Childbearing Age from Consumption of Noncommercial Freshwater Fish in the U.S.: Revised Methodology Report*. The report was prepared for E.H. Pechan and Associates, for submission to EPA's Lisa Conner, by four researchers at RTI International. This memorandum summarizes the results of the peer review. The first section provides a brief description of the process for conducting the review. The second section provides my summary of the results of the review. The attachment to this memorandum (contained in a separate file) provides the full text of the peer reviewers' comments.

The overall assessment of the reviewers is that the approach reflects a sophisticated and reasonable application of exposure analysis methods to existing data that fits the intended purpose. The major strengths of the approach are its appropriate use of large national datasets and the effort to adjust and standardize fish tissue concentrations using the NDMMFT model. The major weaknesses cited by the reviewers tend to be focused on specific components of the analysis, and include the following: the possibly important underestimation of the NDMMFT model for higher mercury concentrations in tihs application; the reliance on average data with relatively less attention to variation in exposure; and the lack of consideration of specific recent literature on fishing behavior, fish consumption, and exposure assessment through fish consumption routes.

One reviewer believes the method should be considered illustrative rather than conclusive evidence of exposure to women of child-bearing age; he is particularly concerned that the method does not consider the impact of fishing advisory risk communication on fish consumption in the target demographic group. He also expresses a concern that the underlying fish tissue data may not reliably support a disaggregated analysis. A second reviewer notes "perhaps substantial problems" in the specific application of the NDMMFT model that may be the source of underprediction of mercury levels in fish tissue with high observed levels of mercury. In addition, the reviewers noted many instances where the documentation is unclear, lacking in sufficient detail to determine what was done, or where the report does not provide sufficient justification for key assumptions and analytic choices.

PROCESS FOR CONDUCTING THE REVIEW

The peer review process for the above-referenced document began in September 2004 and is now complete. In late September IEc submitted a list of potential reviewers with appropriate expertise based on an early draft of the methodology. In mid-November, EPA provided input on our proposed reviewers, clarification of some elements of the methodology related to the specific mercury-in-fish-tissue model applied in the analysis, and a revised set of charge questions. EPA completed a revised document in late-November, at which time the review package was finalized. Peer reviewers began their reviews in early December and completed them in late December. All reviewers were compensated for their time at their standard government rates.

Exhibit 1 lists the four reviewers and provides a brief description of their relevant expertise. IEc selected an initial pool of potential reviewers through a process that included discussion with our professional contacts from similar prior work, our own review of similar exposure analysis work conducted to support the National Academy of Science's report *Toxicological Effects of Methylmercury*, and review of approximately 30 curriculum vitae for potential reviewers.[1] We sought highly qualified individuals with at least 10 years of experience in their fields, and questioned candidates on whether they might have a professional or financial conflict of interest. As a secondary consideration we sought expertise related to analysis of mercury. While we consulted with EPA staff to clarify the type of expertise necessary to conduct a thorough review, and considered a small set of potential reviewers suggested by EPA, IEc independently selected all of the reviewers, consistent with guidelines set forth in the *EPA Peer Review Handbook*.[2] In addition to discussing conflict of interest and independence concerns with potential reviewers, all four reviewers signed a contract with IEc that included a requirement to immediately report any potential personal or organizational conflict of interest.

[1] Committee on Toxicological Effects of Methylmercury of the National Research Council, *Toxicological Effects of Methylmercury* (National Academy Press: Washington, DC), 2000.

[2] US Environmental Protection Agency Science Policy Council, *Peer Review Handbook*, 2nd Edition, Document Number EPA 100-B-00-001, December 2000, available through www.epa.gov.

Exhibit 1
Summary of Qualifications of Peer Review Panel

Reviewer	Brief Description of Relevant Expertise
Dr. Kevin Boyle, University of Maine	Economist with extensive experience assessing demand for recreational fishing, mostly at specific sites but also in support of national assessments. Has also conducted surveys of angler preferences and behavior (e.g., consumptive versus catch-and-release angling, choice of angling site) for the State of Maine.
Dr. Joanna Burger, Rutgers University	Ecologist who specializes in the response of aquatic bird and fish populations to contamination. Has conducted several analyses that integrate ecological and exposure analyses to assess exposure of individuals to methylmercury through fish ingestion.
Dr. James Gilliam, North Carolina State University	Zoologist who specializes in analysis of fish biology and behavior. Expertise includes applications of mathematical biology.
Dr. Donna Vorhees, Menzie-Cura & Associates	Public health expert who specializes in exposure analysis. While most of her work has focused on inhalation exposures, recent work includes analysis of mercury exposures in New Jersey and Louisiana. Also has familiarity with EPA benefits analyses.

Exhibit 2 provides a copy of the cover letter and Exhibit 3 a copy of the charge questions sent to the reviewers. In addition to the methodology report itself, referenced above, reviewers were provided with a link to the Internet site that provides additional information and documentation for the fish tissue model that was applied in this analysis. That model was developed by Dr. Stephen Wente of USGS. At least two of the reviewers appear to have made use of the link to better understand the Wente model, referred to in the charge questions as the NDMMFT.

Exhibit 2
Cover Letter for Reviewers

IEc
INDUSTRIAL ECONOMICS, INCORPORATED

December 2, 2004

Kevin Boyle
Department of Resource Economy & Policy
University of Maine
5782 Winslow Hall
Orono, ME 04469
[transmitted via electronic mail]

Dear Dr. Boyle:

Thank you for agreeing to serve as a peer reviewer of the enclosed report, the USEPA-funded *Assessment of Mercury Exposures to Women of Childbearing Age From Consumption of Noncommercial Freshwater Fish in the U.S: Revised Methodology Report*, dated November 2004.

To provide estimates of the economic benefits of reducing mercury air emissions, EPA has developed methods to map changes in mercury emissions to changes in human health outcomes and to ecological markers such as uncontaminated fish populations. The purpose of the enclosed report is to propose methods that can be used to estimate human exposures to mercury through freshwater fish consumption. The charge for this peer review is to provide technical feedback on the methods employed for this portion of the benefits assessment approach.

Below you will find a list of both general and specific questions that we would like you to consider in conducting your review. We do not expect you to answer each question individually, but we would like you to use them as a guide in preparing your review. Please address as many of these issues as possible but feel free to focus on areas that correspond best with your technical expertise and interests. As you read the report, you will see that the authors of the report point out several potential areas for improvement. Feel free to comment on their suggestions as well.

We request that you submit a written review no later than December 21. You can e-mail the review to me at jneumann@indecon.com. Please organize the review in the form of a memorandum or a short report (preferably in WordPerfect but otherwise in MSWord), beginning with your general impressions of the benefits analysis and then moving to your more specific comments.

Thanks again for your participation. If you have any questions, please feel free to contact me via e-mail or at (617) 354-0074.

Sincerely,

James Neumann
Principal
Industrial Economics, Inc.

Enclosures

Exhibit 3
Key Questions to be Addressed by Peer Reviewers for USEPA's
Assessment of Mercury Exposures to Women of Childbearing Age From Consumption of Noncommercial Freshwater Fish in the U.S: Revised Methodology Report
November 2004

General Topics

- Are the methods, models and data presented and used in this methodology reflective of the current state-of-the-art you are familiar with in this field?

- Given the scope and intended use of the methodology, does the analysis reflect all relevant studies and methods to assess population-level exposure to mercury from freshwater fish?

- Is the methodology of sufficient quality and comprehensiveness to serve the intended purpose (that is, to support benefits analysis for regulatory alternatives to reduce airborne emissions of mercury)?

- Given the scope and intended purpose of the methodology, are the analytical framework, simplifying assumptions, degree of modeling precision, and application of data appropriate? Should the methodology be refined to reflect factors or conditions that are not considered but are likely to affect key target variables?

- What are the overall major strengths and weaknesses of the methodology?

- Are all of the essential elements included in the final report? Is the report clear and well-written? What additional documentation, if any, do you feel is needed?

Specific Topics

Mercury Levels in Fish Tissue

- Is the application of the NLFWA data in this methodology appropriate given the intended scope and purpose of the analysis?

- In future enhancements to the methodology, would it be appropriate to combine the NFTS data with the NLFWA data for use in this methodology?

- Is the method for standardizing the data using the NDMMFT model appropriate?

- Is the degree of uncertainty introduced by the use of the NDMMFT model acceptable?

Exhibit 3 (continued)
Key Questions to be Addressed by Peer Reviewers for USEPA's
Assessment of Mercury Exposures to Women of Childbearing Age From Consumption of Noncommercial Freshwater Fish in the U.S: Revised Methodology Report
November 2004

- Are there other methods that you are aware of to calculate the mercury concentration in fish tissue in freshwater species? EPA has used a simple average of the samples in the NLFWA for other analyses. This approach does not take into account the difference in species and size of species in determining the degree of mercury contamination in an area. Would this approach be preferable for this analysis?

- Is the use of GIS analysis to assign samples to waterbody type an appropriate and reasonably accurate method?

- Are the choice of representative species and size of fish chosen for the benefits calculations appropriate?

Exposed Population and Spatial Distribution of Fishing Behaviors

- Are the two survey databases (NSFSWR and NSRE) good sources of information on anglers and their behavior (i.e., number of anglers, number of fishing days, location of fishing trips, demographic information for anglers)? Are the survey respondents representative of the entire population in the study area?

- Are the analytical framework (i.e., theory and mathematical application) used in the Population Centroid Approach and the Angler Destination Approach appropriate? Are the key assumptions in the two approaches described adequately, justified, and acceptable?

- What are the strengths and limitations for each of the two approaches? How do they compare to each other? Would you recommend one approach over the other for presentation of results, or should EPA maintain both approaches and report a range of benefits?

Fish Consumption and Mercury Ingestion Rates

- Is the formula used for the calculation of mercury ingestion for a population consistent with current methods for estimating mercury ingestion?

- Is the assumption that pregnant women in angler households ingest fish at rates similar to the anglers themselves reasonable? Do you have other recommendations (provide citations)?.

- Is the conversion of uncooked filet to cooked fish reasonable?

- Is the conversion of average daily ingestion rate to maternal hair concentration reasonable?

SUMMARY OF REVIEWER COMMENTS

As noted above, the overall response of the reviewers is that the methodology makes use of the best available methods and national data to support an analysis of this type. In this section, I summarize the reviewers' responses to each of questions posed in the review charge. The full text of each reviewer's response is included as an attachment to this memo in bookmarked PDF format.

Note that Dr. Gilliam's comments focus on Section 2 and Appendix C of the report, related to the application of the NDMMFT for this analysis. The study authors and other interested parties will also find more specific comments referenced to report page numbers in the reviews of Drs. Boyle, Burger, and Gilliam (the last covering only Sections 2, 3 and 6 and Appendix C).

General Topics

- *Are the methods, models and data presented and used in this methodology reflective of the current state-of-the-art you are familiar with in this field?*

All four reviewers answered "yes" to this question. Dr. Boyle also noted in responding to this question that the way information is used in this analysis is new and untested, and noted that exposure analysis "is typically done at a more micro level,", citing examples from EPA's Exposure Factor Handbook.

Drs. Burger and Vorhees suggested that, in future work, probabilistic methods might be applied. Dr. Gilliam suggested that future refinements might include placing fish species into trophic guilds for tissue analysis, smoothing across sites, or using process models for bioaccumulation.

Dr. Vorhees also provided a detailed set of suggestions for future improvements related to the data used to estimate fish tissue concentrations, the data used to estimate the size of the target population, exposure models, and exposure analysis methods that might be applied in future work. With respect to documentation of the approach, she suggests some additional review of literature to bolster the assumptions about the fraction of freshwater anglers who consume their catch or share it among the target population (several reviewers make this point in other parts of their reviews). She suggests clarifying the development of inputs for the exposure model, and suggests that an implicit assumption that fish are consumed in proportion to their prevalence in the database might be improved by data on species that are more highly favored by anglers for consumption, if such data are available. Dr. Vorhees also suggested that the authors provide some indication of the magnitude of other sources of exposure to mercury.

- *Given the scope and intended use of the methodology, does the analysis reflect all relevant studies and methods to assess population-level exposure to mercury from freshwater fish?*

Drs. Boyle, Burger, and Vorhees noted that the national scope of this methodology is broader than existing studies. Each suggested that there are additional sub-national scope studies that are relevant. Dr. Boyle referred to ongoing work in Wisconsin and Maine, Dr. Burger to several of her own assessments with a particular focus on "high-end" consumers of fish, and Dr. Vorhees mentioned the New Jersey studies on which Dr. Burger is a co-author with Dr. Alan Stern.

- *Is the methodology of sufficient quality and comprehensiveness to serve the intended purpose (that is, to support benefits analysis for regulatory alternatives to reduce airborne emissions of mercury)?*

Drs. Burger and Vorhees answered "yes", Dr. Boyle answered "no", and Dr. Gilliam deferred to his colleagues with more exposure analysis experience. Dr. Burger stated that future analyses could be strengthened by additional effort to characterize high-end exposures and by using data on fish species consumed.

Dr. Boyle stated that the analysis is "likely to substantially overestimate the mean (or median) consumption rate by women of child bearing age, and is silent on women who are maximally exposed." This statement is supported by his comments on specific charge questions, summarized below. The critical element affecting Dr. Boyle's conclusion appears to be the fish consumption rate for women of child-bearing age; Dr. Boyle notes that the analysis did not take into account the effect of fish consumption advisories and other risk communication efforts on fish consumption rates among this sensitive group.

- *Given the scope and intended purpose of the methodology, are the analytical framework, simplifying assumptions, degree of modeling precision, and application of data appropriate? Should the methodology be refined to reflect factors or conditions that are not considered but are likely to affect key target variables?*

Drs. Boyle and Berger agree that the analytical framework is adequate for the scope and purpose. Dr. Gilliam felt the overall "three-step strategy" is reasonable, but largely deferred to his colleagues with more exposure analysis experience in responding to this question. Dr. Vorhees appeared to answer the question in the context of the "overall strengths and weaknesses" question response (see below).

Dr. Boyle noted, however, that the "simplifying decisions and degree of modeling precision are suspect" because of two factors: the assumption that women of child-bearing age consume fish at the same rate as the general population; and the small number of fish tissue concentration estimates per HUC in the national dataset. Dr. Gilliam made a similar point about the thin data at the HUC level in his detailed comments. Dr. Burger suggested that, in future analyses, the work could be strengthened by a stronger effort to characterize exposures to high fish consumption communities.

- *What are the overall major strengths and weaknesses of the methodology?*

Drs. Boyle and Burger noted that a major strength of the analysis is its national scope and generally appropriate use of national datasets. Dr. Vorhees cited the use of the NDMMFT model as a major strength in developing fish tissue concentration data that are likely to be more representative of actual exposures than the raw data.

Dr. Boyle noted the major weakness is the lack of attention to existing studies of fish consumption by women of child-bearing age. Dr. Burger stated the major weakness is the reliance on average data, rather than more detailed site-specific data, for key exposure factors.

She also noted four other weaknesses in responding to this question, largely related to her main point about characterizing high-end exposures.

Dr. Gilliam expressed a concern about EPA's application of the NDMMFT model related to under-prediction of high-end and over-prediction of low-end fish tissue concentrations - he was unable to determine if this was a weakness of the model or EPA's application of the model, but urged EPA to engage the model's developer, Dr. Stephen Wente, in direct review of the application. Dr. Vorhees noted the discussion and selection of fish consumption rates for both the general population and important subpopulations is a major weakness of the study. She noted that existing data are sufficient to support a more sophisticated analysis than reliance on defaults provided in EPA's Exposure Factors Handbook.

- *Are all of the essential elements included in the final report? Is the report clear and well-written? What additional documentation, if any, do you feel is needed?*

All four reviewers found the report well-written. All four reviewers also requested additional discussion, and three of the four found the description of the NDMMFT model and its application to be insufficient. Drs. Boyle and Burger also made specific recommendations to expand the literature base used to justify analytic choices and other statements made in the report. As noted above, both of these reviewers, and Dr. Gilliam as well, provided specific detailed comments on the documentation with page and line references, and all four provide suggestions for additional literature to consider.

Specific Topics

Mercury Levels in Fish Tissue

- *Is the application of the NLFWA data in this methodology appropriate given the intended scope and purpose of the analysis?*

Dr. Boyle expressed the strongest concerns about this dataset, noting that the impact of fishing advisories on fishing behavior is not addressed in the analysis (that is, an advisory is likely to dampen demand for a site and/or for consumption of fish from the site). This appears to be more a concern with the application of the data rather than the data itself, as he stated that the NLFA (or NLFWA) is the best available data to characterize bioaccumulation of mercury in fish tissue. In responding to this question, he also noted a need for "ground truthing" assumptions in the angler destination approach. Dr. Gilliam also expressed a concern that the dataset may be too thin at the HUC level to support disaggregated analysis.

Dr. Vorhees and Dr. Burger supported the study's application of this dataset for the scope and purpose of this analysis. Dr. Vorhees made a similar point to Dr. Boyle, noting a concern that the dataset, which results from advisory development, would be more likely to overestimate risk. Dr. Vorhees stated that a bioaccumulation model is not a feasible option, but suggested that the report include a more extensive rationale for the choice of using measured rather than modeled fish tissue concentrations. Dr. Burger also suggested further comparison of the data with other published data sources (specifically noted in her review, under her comments on Section 2 of the report and more detailed comments on the Wente model).

- *In future enhancements to the methodology, would it be appropriate to combine the NFTS data with the NLFWA data for use in this methodology?*

Dr. Burger appeared to support combination of the two databases in future work. Dr. Vorhees suggested conducting separate analyses, one using NLFWA and one using NFTS, and comparing the results, rather than combining the two datasets. Dr. Gilliam appeared to support use of both datasets, although he did not comment directly on the appropriateness of any specific combination strategy.

- *Is the method for standardizing the data using the NDMMFT model appropriate?*

The main thrust of Dr. Gilliam's comments relate to his concerns about the NDMMFT model or its application (as stated above, based on data provided in the report he cannot pinpoint the source of concern). His comments under points 1 and 2 of his review address both this question and the next one concerning the degree of uncertainty in the use of the NDMMFT model.

Dr. Burger did not answer this question directly in this part of her review. She did comment on the NDMMFT model in her comments on Appendix C. In that section of her review, she expressed some concerns about the application of the model, which she notes "was not designed or intended to estimate mercury levels in populations." She also noted some initial skepticism about the goal of generating a single average mercury concentration value for each water body, and examined the three basic assumptions of the Wente model in light of her own ecological background. Neither her comments nor Dr. Gilliam's should be interpreted, however, as a peer review of the Wente model - both recognize that is outside the scope of their charge.

Dr. Vorhees thought the general method was appropriate, but did not elaborate.

- *Is the degree of uncertainty introduced by the use of the NDMMFT model acceptable?*

Both Dr. Gilliam and Dr. Vorhees thought the reported information on predicted versus actual values indicated a relatively large error, as noted above. Dr. Gilliam provided a detailed elaboration of his concerns in his report; for example, he disagreed with a statement made in the report that the degree of underprediction was "slight." His conclusion is that the model may therefore "de-emphasize potentially real 'hot spots.'" This is one source of his concerns about the model. Dr. Vorhees concluded that the goal of standardization is important, and that absent better data or mechanisms for improving the model or its application "this degree of uncertainty is acceptable but must be considered in interpreting and using results for risk management."

- *Are there other methods that you are aware of to calculate the mercury concentration in fish tissue in freshwater species? EPA has used a simple average of the samples in the NLFWA for other analyses. This approach does not take into account the difference in species and size of species in determining the degree of mercury contamination in an area. Would this approach be preferable for this analysis?*

Dr. Boyle answered "yes" to preferring the alternative of using a simple average of samples in the NLFWA dataset. He also urged that the effect of risk communication related to advisories on the size and species of fish consumed be taken into account.

Dr. Vorhees stated that the NDMMFT, even with its uncertainties, is an improvement over simple averages drawn from the monitoring data.

In response to the first part of the question, Dr. Burger outlined an alternative approach that would involve a "detailed interview survey of fish consumption practices and fish caught by people fishing in that body of water", similar that used in one of her own analyses, but repeated for 50-100 representative sites around the country. She also states her awareness that this would be impractical to accomplish for the current study's purpose - she may be suggesting a long-term goal for future EPA efforts.

Dr. Burger believes the method for estimating fish tissue concentration in fish that are ultimately consumed is one of the critical choices in the methodology. She stated three specific suggestions for improving the approach: 1) expand the discussion of the effect of fish size and age, trophic status, and foraging location on mercury levels; 2) build into the model the possibility of different consumption scenarios that include varying the species and size of fish (she cites some of her own work for examples); and 3) include a model for the general population that uses the 75th or 90th percentile of mercury levels in fish, rather than just the mean.

Dr. Gilliam expressed confusion on how the "simple average" was taken, in part because the report used common rather than latin names for fish species. He thought the ideal method would be to "use an estimate based on species likely to be present at the site (but not necessarily in the sample, per Wente) and sizes likely to be consumed." His detailed comments (under point 3 of his report) provide additional detail.

- *Is the use of GIS analysis to assign samples to waterbody type an appropriate and reasonably accurate method?*

The panel did not have a GIS expert. Drs. Boyle, Burger, and Vorhees commented on this question. Dr. Boyle stated that he is "not sure" whether this is an appropriate or accurate method. Dr. Vorhees stated that the use of GIS as a tool for this step is appropriate. Dr. Burger urged clear expression of the uncertainties, and consideration of a model that includes both ends of the continuum.

- *Are the choice of representative species and size of fish chosen for the benefits calculations appropriate?*

Drs. Boyle, Burger, and Vorhees thought the choice was appropriate.

Commenting on the report documentation, Drs. Boyle and Vorhees urged further research, including investigation of state-specific creel survey data, to support the choices made.

Exposed Population and Spatial Distribution of Fishing Behaviors

- *Are the two survey databases (NSFSWR and NSRE) good sources of information on anglers and their behavior (i.e., number of anglers, number of fishing days, location of fishing trips, demographic information for anglers)? Are the survey respondents representative of the entire population in the study area?*

Drs. Boyle, Burger, and Vorhees agreed they are good sources for the general population.

Dr. Boyle noted that the NSFHWR database is designed to be representative at the state-level, and the NSRE at the national level, not at the more disaggregated level they are applied here. Dr. Burger stated these sources do not adequately target the high-risk population, and may over-sample higher income samples.

- *Are the analytical framework (i.e., theory and mathematical application) used in the Population Centroid Approach and the Angler Destination Approach appropriate? Are the key assumptions in the two approaches described adequately, justified, and acceptable?*

Drs. Burger and Vorhees thought the approaches were appropriate and described adequately.

Dr. Boyle found no theory in the report, and suggested that there are assumptions about fishing behavior that he would not support, as described elsewhere in his more detailed comments. Both Dr. Boyle and Dr. Burger noted that the assumptions may be too general to capture important variation as well as fishing behaviors that differ across groups or in response to risk information.

- *What are the strengths and limitations for each of the two approaches? How do they compare to each other? Would you recommend one approach over the other for presentation of results, or should EPA maintain both approaches and report a range of benefits?*

Drs. Boyle and Vorhees stated they would recommend keeping both approaches. Dr. Burger agreed that both approaches should be kept, but "leaned toward" the Angler Destination Approach.

Dr. Boyle was disappointed that the report did not do a better job of drawing out potential strengths and weaknesses and the impact of critical elements of each of the two approaches.

Fish Consumption and Mercury Ingestion Rates

- *Is the formula used for the calculation of mercury ingestion for a population consistent with current methods for estimating mercury ingestion?*

Drs. Burger and Vorhees answered "yes." As an enhancement to the current approach, Dr. Vorhees recommended more justification for the selected consumption rate, and provided citations for two more recent reviews of fish consumption data (Moya, 2004 and California OEHHA, 2001 - full citations and Web addresses provided in her detailed comments).

Dr. Boyle stated that this is the first analysis he is aware of that uses aggregate secondary data rather than site-specific data at the state or water-body level.

- *Is the assumption that pregnant women in angler households ingest fish at rates similar to the anglers themselves reasonable? Do you have other recommendations (provide citations)?*

Dr. Boyle thought the assumption was not reasonable and cited two EPA-funded studies in Wisconsin and Maine that he believes provide better alternatives. He stated the Maine report should be available soon. Dr. Burger cited her own work in South Carolina that indicates the ingestion rate (meals/week) is reasonable. She noted that amount of fish consumed, however, differs by gender. Dr. Vorhees was not aware of any studies that directly address this question, but noted an ongoing freshwater fish study where she is a participant that may at some future point provide relevant information. All three provided citations in their detailed comments.

- *Is the conversion of uncooked filet to cooked fish reasonable?*

Drs. Burger and Vorhees both thought the value of 1.5 used in the study was reasonable.

Both Dr. Burger and Dr. Vorhees cited specific studies that may be informative in defining the variation across cooking methods, in the event EPA might choose to pursue a probabilistic approach in the future.

Dr. Boyle declined to comment.

- *Is the conversion of average daily ingestion rate to maternal hair concentration reasonable?*

None of the reviewers responded to this question.

Attachment: Full Text of Reviewers' Responses to Charge Questions

Comments on:

"Assessment of mercury Exposures to Women of Childbearing Age From Consumption of Noncommercial Freshwater Fish in the U.S.: Revised Methodology Report"

By:

Kevin J. Boyle
Distinguished Maine Professor,
University of Maine

December 21, 2004

In providing this peer review I will specifically address each of the questions directed to me in the letter from James Neumann dated December 2, 2004. I will restate each question and then follow with my response to the questions.

After responding to the questions I will provide page specific comments on the report that will help to clarify my responses to the questions.

My overall impression is that this is a very sophisticated analysis of existing secondary data. However, I am concerned that the underlying data are too thin to reliably support the disaggregated computations undertaken. It seems to me that this analysis is at best illustrative, but not conclusive information. The assumption in the calculations, particularly with respect to consumption rates by women of child bearing age leads me to suspect that both approaches have overestimated consumption. In addition, the analysis really does not address what share of women of child bearing age, and consequently fetuses, are in the high risk categories due to elevated consumption rates.

General Topics

- Are the methods, models and data presented and used in this methodology reflective of the current state-of-the-art you are familiar with in this field?

  The models and data used are reflective of the current state of the art, but the way this information is used in the analyses is new and untested. In fact, risk assessments related to human exposure to toxins in fish tissue is typically done at a more micro level as demonstrated from the Maine, Michigan and Lake Ontario studies cited in EPA's *Exposure Factor Handbook* (1997).

- Given the scope and intended use of the methodology, does the analysis reflect all relevant studies and methods to assess population-level exposure to mercury from freshwater fish?

The analysis does reflect the major contributions to the literature on fishing behavior and consumption at an aggregate level. However, the are ongoing studies in Wisconsin and Maine, which are funded by EPA, that specifically investigate fish consumption by women of child bearing age. There are other studies that have looked at fish consumption by women that are not considered. Thus, studies specific to fish consumption by women of child bearing age have been totally neglected in that analyses and citations.

- Is the methodology of sufficient quality and comprehensiveness to serve the intended purpose (that is, to support benefits analysis for regulatory alternatives to reduce airborne emissions of mercury)?

  No. My assessment is that analysis procedures are likely to substantially overestimate the mean (or median) consumption rate by women of child bearing age, and is silent on women who are maximally exposed. My specific comments on the report that will be presented below clarify these concerns.

- Given the scope and intended purpose of the methodology, are the analytical framework, simplifying assumptions, degree of modeling precision and application of data appropriate? Should the methodology be refined to reflect factors or conditions that are not considered but are likely to affect key target variables?

  The analytical framework is generally good, but the simplifying decisions and degree of modeling precision are suspect. For example, the assumption that women of child bearing age consume fish at the same average rate as the general consuming population seems fallacious given the extensive consumption advisory and risk communication efforts that are in place. While there are many estimates of mercury concentration in fish tissue, the number of observations for each part of the disaggrgatesd analysis (e.g., by HUC) are quite small). Thus, the methodology should be refined to better account for these and other uncertainties, explicitly considering data from studies that have examined fish consumption by women of child bearing age.

- What are the overall major strengths and weaknesses of the methodology.

  The major strength of the methodology is that it attempts to use existing information to develop a risk assessment over a large geographic region. Unfortunately, the major weakness is that no attention appears to be given to studies of fish consumption by women of child bearing age, which leads to questionable assumptions in the analyses.

- Are all of the essential elements included in the final report? Is the report clear and well written? What additional documentation, if any, do you feel is needed?

  The report is well written, but lacks clarity in the technical explanations. For example, the Wente (2004) NDMMFT model can not be fully understood without going to the original report. The reader should not need to do this. Likewise, the description of some of the

technical calculations of consumption rates could have been more clearly explained. While grammatically well written, these sections of the report are unnecessarily vague and lacking in detail at times. I would have liked to see a more direct comparison of the estimates from the alternatives methods and a clear, succinct explanations of why the estimates differ and which estimates are potentially more accurate.

Specific Topics

Mercury Levels in Fish Tissue

- Is the application of the NLFWA data in this methodology appropriate given the intended scope and purpose of the analysis?

  I have concerned about the weighted average concentration concentrations. A basic premise of recreational fishing is that people will travel further for a higher quality fishing experience, which has been statistically supported in the estimation of recreation demand models of fishing. If information is available about fish consumption advisories and advisories by species, people may not be likely to consume fish from all rings around their census block. This consideration is overlooked in the analysis.

  For the angler destination approach, the assignment of estimates to rivers and lakes may be flawed given the large number of standing and flowing waters in the northern states. No ground truthing of this assignment method was presented.

  A minor point, I did not find an explicit definition of NLFWA in the text. I am referring to the use of NLFWA in Figures 3-1 and 3-2 in my comments above.

  However, if your question was intended to refer to NLFA, I do have some added comments. NLFA provides the best available data of the bioaccumulation of mercury in fish tissue. However, the sample sizes for the substate computations used in the analyses result in very small samples that I can not assess their accuracy.

- In future enhancements to the methodology, would it be appropriate to combine the NFTS data with the NLWA data for use in this methodology?

  I am not sure what you are asking because it seems that these data were used to compute the average tissue concentration rates.

- Is the method for standardizing the data using NDMMT model appropriate?

  I do not know as this is not my specific area of expertise in the peer review, However, this model was not explained sufficiently in the report. Nor was the need for standardization sufficiently justified.

- Is the degree of uncertainty introduced by the use of the NDMMFT model acceptable?

  See my response to the bullet above. However, the apparent degree of uncertainty introduced by the use of this model seems small relative to some of the other simplifying assumptions used in the analyses.

- Are there other methods that you are aware of to calculate the mercury concentration in fish tissue from freshwater species? EPA has used a simple average of the samples in the NLFWA for other analyses. This approach does not take into account the difference in species and size of species in determining the degree of mercury contamination in an area. Would this approach be preferable in this analysis?

  Yes, it would be preferable. Risk communication efforts are telling the public, women of child bearing age in particular, that concentrations of toxins in fish tissue vary by species and species size. Such an adjustment in the concentrations needs to be accompanied information on how women of child bearing age adjust their consumption of freshwater fish in response to these fish consumption advisories.

- Is the GIS analysis to assign samples to water body type an appropriate and reasonably accurate method?

  I do not know if this approach is appropriate or accurate. In northern states like Wisconsin and Maine where many standing and flowing weather bodies are closely interspersed I suspect that there is substantial error. I also do not know that closest water body is the appropriate correlation if mercury concentrations could vary with the bio-physical attributes of water bodies.

- Are the choice of representative species and size of fish chosen for the benefit calculations appropriate?

  The selected species seem reasonable. However, state-specific Creel data could be used to refine the popular species and typical sizes by state.

Exposed Population and Spatial Distribution of Fishing Behaviors

- Are the two survey databases (NSFSWR and NSRE) good sources of information on anglers and their behavior (i.e., number of anglers, number of fishing days, location of fishing trips, demographic information for anglers)? Are the survey respondents representative of the entire population?

  I assume you mean NSFHWR, not NSFSWR. These are the best national data sets on recreational, fresh water fishing. They are recent and use state-of-of-the-art survey design. In addition, NSFHWR provides, consistent, systematic data on a state-by-state basis. However, there are some caveats. The NSFHWR is designed to be representative at the state

level, not the below this geographic stratification. The NSRE is representative at a national level. Thus, methodology uses these data at a level below what they are designed for, which affects the accuracy of the resultant estimates and the credibility for expansion to the affected population.

- Are the analytical framework (i.e. theory and mathematical application) used in the Population Centroid Approach and the Angler Destination Approach appropriate? Are the key assumptions in the two approaches described adequately, justified, and acceptable?

  I have to admit that I did not find any theory in this report so I can not comment on the underlying theory that supports the analysis. I did not find any mistakes in the mathematical equations as presented. There are assumptions that should change, which would change the arguments in the equations and ultimately change the estimates reported. I have noted some of these concerns in my responses to questions above and will provide more detail in my specific comments below

- What are the strengths and limitations for each of the two approaches? How do they compare to each other? Would you recommend on approach over the other for presentation of results, or should EPA maintain both approaches and report a range of benefits.

  I have to admit that this is a key element that I found to be missing from the report; I was expecting the authors, who are most familiar with the analysis to present this information.

  Neither approach is without so I would recommend keeping both. I would add itemized listing of strengths and weaknesses of each approach and factors that lead to over and underestimation for each approach. I would present his information in table form, accompanied by a verbal discussion of the net over/under estimation by approach and which approach the authors feel is most defensible.

  This question allows the authors to dodge a crucial portion of the report and place the responsibility on the "shoulder" of the reviewers.

Fish Consumption and Mercury Ingestion Rates

- Is the formula used for the calculation of mercury ingestion for a population consistent with current methods for estimating mercury ingestion?

  No, this is the first attempt that I know of to use aggregate, secondary data to estimate mercury ingestion over a large geographical region. All analyses to date that I am aware of have used site-specific data for a much smaller region, e.g., a state or specific water body.

- Is the assumption that pregnant women in angler households ingest fish at rates similar to the anglers themselves reasonable? Do you have other recommendations (provide citations)?

No, for at least two reasons. There are extensive risk communication efforts in place to encourage women of child bearing age to reduce their consumption of fish. While fish consumption advisories are not completely successful, there is convincing evidence that this information has changed consumption behavior. As the most restrictive advisories are targeted at women, it is logical to think that these individuals will consume less fish. Second, bag limits control how much fish an angler brings home. If fish is consumed on overnight trips, then consumption by family members who remain at home would be expected to be less than that of the angler. There is new data on fish consumption by women of child bearing age coming from the EPA studies funded in Maine and Wisconsin. The Maine report should be available shortly.

- Is the conversion of uncooked filet to cooked fish reasonable?

  I do not feel qualified to comment on this specific conversion factor. However, I did find the discussion of this item confusing in the text.

- Is the conversion of average daily ingestion rate to maternal hair concentration reasonable?

  I do not feel qualified to comment on this specific conversion factor.

My Detailed Comments

p. 2-1 – While 83,000 samples appears to be a very large sample, with 10,000 sites this is approximately only about 8 observations per site. This is not large for the disaggregated computation undertaken.

p. 2-2 – The proximity analysis is untested. In states like Wisconsin and Maine where there are large numbers of flowing and standing waters in close proximity it is likely that errors in assignment will arise. I do not know what the error rate is, but this is a fundamental part of the analysis that should have some ground-truthing undertaken to understand what degree of error this approach introduces into the analysis.

p. 2-2 – The NFLA data selection described in the bottom paragraph is good and appropriate.

p. 2-5 – focusing on fish that are typically consumed is appropriate.

p 2-6 – Discussion of the NDMMFT needs more clarification here and on the following pages. I want to know exactly what the variables are and the functional form is. I really don't care that SAS was used and I just assume Newton-Raphson. There is too much information on irrelevant details and too little explanation of the model itself.

There is no justification given for using this model. Did you do this to remove error that might arise from small sample cells by letting the entire data set be used to develop prediction parameters?

p. 2-7 – It seems that no predictions were made outside of locations where observations existed, which is good, but this means that you had a lot of missing cells for consumption rates. This seems to be a serious bias in your aggregate estimate that you do not directly address. Am I missing something?

Figure 3-1 – Approach 1 seems to overestimate exposure because there is no upper age limit (e.g., your note of 45) imposed on NA.

p. 3-10 – As I stated in my responses to the questions above. The weighted average concentrations within the regions seems highly suspect to me as anglers will travel farther to fish a higher quality fishing site. The weighting employed includes no information that would reflect angler preference/choices based on fish consumption advisory information. In addition, geographical differences would make this distributions differ from region to region and I am not sure you have captured this in your calculations.

p. 3-12 – The assumption that everyone in the same block has the same exposure is iffy. Income is one stratification to get at subsistence anglers, but $50,000 is too high. Another factor to consider is education as this may affect understanding of fish consumption advisories and the health consequences.

p. 3-17 – I tend to think that conceptually that the "angler destination approach" is more accurate for the reasons you allude to in the text, but I am afraid the data are not sufficient to support this analysis. This is why I recommended above that booth approaches be reported and that you clearly identify the pros/cons and over/under estimation.

p. 3-20 – The assumption of unbiasedness in the last paragraph would only seem to hold if bioaccumulation in fish tissue is not affected by the bio-physical attributes of water bodies.

p. 3-21 – I would like to see the direct comparison of the results from the two methods here.

p. 3-24 – You need more explanation and justification for the asserted overestimation and underestimation.

p. 4-2 – It seems that a more accurate analysis would allow adjustments in catch rates by region that would affect consumption rates. Factors such as catchability, fishing restriction, size of fish, etc. vary from region to region and directly affect annual consumption rates.

p. 4-13 – Here again I would like to see a direct comparison of the estimates. It seems to me that the action is mostly in the affected population and not the consumption rate per individual. This, I fear is an artifact of your simplifying assumptions and perhaps not reality.

p. 6-1 – I am very disappointed in this section of the report. Here is where I think tables that document strengths and weaknesses of the approaches in a bulleted format is needed. Likewise, know sources of overestimation and underestimation, and unknown directional affects need to be identified. The text should be an integration to clearly explain conclusions of net over estimation or underestimation, and the preferred approach.

PEER REVIEW REPORT of DR. JOANNA BURGER:

Assessment of Mercury Exposures to Women of Childbearing Age from Consumption of Noncommercial Freshwater Fish in the U.S. (Nov.2004)

Prepared by RTI International for E.H.Pechan and Associates inc for submittal to USEPA

Date of Report December 28, 2004.

My report is organized into the following components.
1. Introductory remarks
2. Responses to specific questions followed by other comments on each of the Sections.
3. Additional references to be considered in discussing validity of assumptions
4. Specific editorial comments, questions, clarifications on a page-by-page basis.

Some of my remarks in the different sections are redundant since they were made at different times.

1. Introductory remarks

This is a sophisticated and elegant approach to estimating the public health magnitude of exposure to the sensitive receptor (the fetus) from mercury obtained from non-commercial freshwater fish.

I strongly support EPA's effort to arrive at a benefit calculation for reducing mercury emissions. Obviously deposition reaching freshwater bodies and bioamplification leading to relatively high mercury levels in self-caught fish (recreational, subsistence) is a major outcome of atmospheric transport and deposition of mercury, now originating mainly from coal-fired powerplants. However, EPA should not forget that the same atmospheric transport leads to deposition on estuarine bodies, with resultant contamination of estuarine-dependent marine fish, which is another major source of mercury exposure for the general public as well as anglers. Also important is that many (perhaps most) recreational fishers get most of their fish from commercial sources in the course of a year----particularly in northern- climes where fishing is seasonal. This is alluded to in passing on page 4-7.

The report is interesting, elegant, ambitious and relevant. It makes good use of several large scale data bases. And, as is evident from Section 6, the authors are well aware of the limitations of the data bases and the uncertainties introduced in the modeling process. Section 6 is a sophisticated discussion of sampling error and validation. Unfortunately it was not apparent at the beginning, so that my reading was colored by a sense that the implications of the simplifying assumptions were NOT understood.

Therefore I recommend a disclaimer, clear and upfront, in section 1, that the authors are well aware of the limitations of the data bases and the inherent uncertainties and that these are discussed in detail in Section 6.

Although the title is "non-commercial" fish, throughout sections 2-3-4 the emphasis is on recreational rather than subsistence fishing and then switches subtly to subsistence fishing in section 5.

The weakness of the report is the paucity of references supporting (or arguing against) the assumptions. I have provided references to papers that I and my associates have authored, but the literature on fishing behavior, fish consumption, and advisories is richer than the report authors acknowledge. In at least two places they state erroneously that information is unavailable.

Both high income and low income people may fish primarily for recreation and likewise both may consume a large portion of their catch. Hence the distinction between recreation and subsistence is blurred in real life. We found this blurring to be true in several of our studies of anglers in New York-New Jersey (Burger et al. 1993b), South Carolina and Puerto Rico (Burger et al. 1993a), and examined it along the Savannah River (Burger et al. 1999). High-level consumers are the important group for risk communication which can be reached through formal channels such as advisories linked to fishing licenses. But there is ample evidence that people who fish a lot consume more fish than those who fish occasionally. Many of the latter recreational fishers, engage in catch-and-release.

At the end of my comments in reviewing appendix C, I discuss the Wente (2004) report which forms an important base for the present study.

2. Responses to specific questions followed by other comments on each of the Sections.

General Topics

· *Are the methods, models and data presented and used in this methodology reflective of the current state-of-the-art you are familiar with in this field?*

Overall, the methods, models and data are reflective of the current methodology in this field. It may have helped to use probabilistic methods rather than the usual assumptions, and this is done in Section 5.

· *Given the scope and intended use of the methodology, does the analysis reflect all relevant studies and methods to assess population-level exposure to mercury from freshwater fish?*

Within the scope of the project, it does. However, there are several issues which should be discussed, including the several studies that have examined fish consumption in at risk populations, not just the two discussed. The EPA data base on fish consumption does not adequately estimate consumption by high end consumers---the small but real group who eats 10 fish meals per week or more, for example. Several of our studies (Burger et al. 1993ab, Burger et al. 1999, Burger 2002ab) indicate that there are really high end consumers who may consume 50-100 g/day, and even a few who consume more than that (two fish meals per day). The population impact may be small, but the risk-communication impact is not.

· *Is the methodology of sufficient quality and comprehensiveness to serve the intended purpose (that is, to support benefits analysis for regulatory alternatives to reduce airborne emissions of mercury)?*

Yes. Even with the limitations indicated the estimate of excessive exposure (Section 5) is clear. However, I feel the case could be made stronger by:

a. Including some of the other studies of fish consumption of low-income, ethnic, and Native American communities (see references below for some of these: Harris and Harper 1998, Toth and Brown 1997).

b. Using real data from studies that show high fish consumption, rather than only the EPA guidance.

c. Using data on fish species consumed.

d. Using the high-end of exposure, rather than the means (this was done in Section 5).

· *Given the scope and intended purpose of the methodology, are the analytical framework, simplifying assumptions, degree of modeling precision, and application of data appropriate? Should the methodology be refined to reflect factors or conditions that are not considered but are likely to affect key target variables?*

Given the scope, the framework is sufficient. However, the case could be made stronger by including other ethnic groups, and including data from high fish consumption communities, rather than relying only on the EPA guidance. One could show cumulative frequency graphs, taking into account studies that document higher exposures.

· *What are the overall major strengths and weaknesses of the methodology?*

The major strengths are that the project relies on the largest data sets, employs appropriate statistical methods and modeling, and uses two different approaches to approximate exposure, mercury levels, and risk to the pregnant population. It is extremely valuable that the study was done for pregnant women.

Another strength is the full discussion of uncertainties in the analytical methods, although this doesn't appear until Section 6.

The major weaknesses are that it relies on average data, and not site-specific data for individual exposure (in terms of consumption, distance traveled, fish consumed, amounts consumed).

Other weaknesses include:

a) The assumption of an 8 once meal may be an underestimate.

b) Examining only pregnant women, and not including those with small children (for whom mercury exposure is a problem, although post-natal mercury seems to have less of an impact than pre-natal exposure).

c) Not including other Native American scenarios.

d) Not including some of the consumption studies that have much higher-end exposures than the consumption rates used in this report.

· *Are all of the essential elements included in the final report? Is the report clear and well-written? What additional documentation, if any, do you feel is needed?*

Overall the report is clear and well-written. My final section has a page-by-page comment which identifies a relatively few points that require clarification or expansion. Overall there is a clear description of the methodology.

One major area that is notably lacking is the literature. The report has many assertions which are not documented with references, many clear and obvious papers that are not included, and in many cases, only one reference when there are others that should be cited. In addition, some more recent references should be included. The addition of many more references would make this document much stronger. I have provided some suggestions below.

EVERY SECTION requires additional references to back up the data and arguments.

The report is well- written. The element lacking is the discussion of exposure and risk for young children.

Specific Topics

Section 1. Introduction: Reference should be made to the caveats discussed more extensively in section 6.

Section 2. Mercury Levels in Fish Tissue Reference should be made to Appendix C.

· *Is the application of the NLFWA data in this methodology appropriate given the intended scope and purpose of the analysis?*

Yes, it is appropriate, but it would help to have it backed-up by some of the other published data sources on mercury levels in freshwater fish, as well as the National contaminant biomonitoring program (see Schmitt, C. J., and Brumbaugh, W. G. 1990). (see also my comments on the Wente paper below).

· *In future enhancements to the methodology, would it be appropriate to combine the NFTS data with the NLFWA data for use in this methodology?*

Probably, as long as the method of doing so was clearly stated. There are other data sources on mercury which are apparently lacking. For example, New Jersey has extensive data which apparently aren't in the data base (unless they were filtered out for some reason).

· *Is the method for standardizing the data using the NDMMFT model appropriate?*

I'm not sure which part of the document this refers to.

Is the degree of uncertainty introduced by the use of the NDMMFT model acceptable?

In risk assessment we have frequent discussions about uncertainty and uncertainty analysis and the propagation of uncertainties. It's hard to tell whether the degree of uncertainty is acceptable, until some analyses are run to see specifically the impact of varying input parameters.

It may be acceptable because it is the best that is currently available. However, I am uncomfortable with the use of so many assumptions, and the use of mean values, which underestimate the risks to the high-risk population.

· *Are there other methods that you are aware of to calculate the mercury concentration in fish tissue in freshwater species? EPA has used a simple average of the samples in the NLFWA for other analyses. This approach does not take into account the difference in species and size of species in determining the degree of mercury contamination in an area. Would this approach be preferable for this analysis?*

I agree that "this approach does not take into account the difference in species and size…" both as they occur in water bodies and as they are caught and kept and eaten by anglers.

This is one of the biggest difficulties with this methodology. Firstly, there needs to be a discussion of the effect of fish size (and age) on mercury levels, of trophic status and mercury levels, and of foraging location (some bottom feeders have high mercury levels).

Secondly, it would be preferable to build into the model the possibility of different consumption scenarios that include varying the species of fish (and thus the levels of mercury), and the sizes (and trophic status) of the fish in the model. We did this in our study of mercury exposure along the Savannah River (Risk Analysis 2001).

Thirdly, it might be appropriate to include a model for the general population that used the 75th or 90th percentile of mercury levels in fish (not just the mean) to account for people who are fishing for these specific fish and keep the large ones (fish regulations preclude keeping small fish).

As written, the model may underestimate the mercury levels in fish that are consumed because it is some of the high mercury species that are preferred, and it is certainly the larger fish the fishermen like to bring home (and that in many states, are the legal ones to catch).

So with respect to the question of · *"Are there other methods that you are aware of to calculate the mercury concentration in fish tissue in freshwater species?"*

There is an alternative. The approach we used was a detailed study of mercury levels in eight species of fish representing different trophic levels, coupled with a detailed interview survey of fish consumption practices and fish caught by people fishing in that body of water (Burger et al. 2001). If this were done for about 50-100 representative sites around the country, one would get a more detailed picture with far fewer assumptions, to accomplish the same purpose (impact of mercury reduction). Thus the modeling approach is used here (as in many parts of environmental health) where the detailed data with adequate spatial and temporal resolution) would be impractical.

· *Is the use of GIS analysis to assign samples to waterbody type an appropriate and reasonably accurate method?*

Yes, as long as the uncertainties associated with people fishing in both waterbody types is clearly expressed. It might be that models should be built that include both ends of the continuum (all lakes, all rivers/streams).

· *Are the choice of representative species and size of fish chosen for the benefits calculations appropriate?*

Yes, although not all species occur everywhere. The larger problem is the few sampling sites (and the few fish per site) used in the analyses. This is a limitation imposed by the underlying data set and the Wente modeling of that data set.

OTHER COMMENTS ON SECTION 2:

1. Many statements require references to be credible.

2. What is the temporal distribution of the data included in the sample. Are all pre-1990 data excluded? How much of the sample was collected after 2000. Point source mercury pollution was higher decades ago and atmospheric transport is higher today than 20 years ago.

3. The statement that most sources of airborne mercury are in eastern states is unclear. What constitutes the “eastern states”. Contamination comes mainly from what we easterners call “the Midwest” (Ohio, Indiana and so on)(Maybe the authors consider these eastern). They are in the eastern third of the United States (NESCAUM, 1998 see below). The problem may be that "eastern" and "western" mean different things to different people, and this needs to be carefully defined.

4. The Wente model, which can use only one mercury value per location and (if I read Wente correctly, a single fish from a single location would suffice). This seems to oversimplify the problem - since mercury levels can range from nearly zero to over 1 ppm depending upon the species and size of the fish (even in the same water body).

5. Within a species, mercury levels tend to be log-normally distributed, in my experience (and this should be used in the model).

6. The statement that the concentration of mercury in any fish will be a good approximation of the time the fish is alive is true only WITHIN a species and probably within a water body. It is not true across species.

7. Not all places have the fish species used - what happens when some are not present?

8. I am uncomfortable with states with fewer than 10 sites, especially when there are few fish from each site (and little indication of how many fish are from each site).

9. Weighting approaches (for species of fish) should be considered for different locations.

10. The scientific names of fish used should be given. Exactly which bass and so on?

Section 3: Exposed Population and Spatial Distribution of Fishing Behaviors

· *Are the two survey databases (NSFSWR and NSRE) good sources of information on anglers and their behavior (i.e., number of anglers, number of fishing days, location of fishing trips, demographic information for anglers)? Are the survey respondents representative of the entire population in the study area?*

Yes, the two sources are good sources for the general population, but they probably do NOT adequately represent or target the high-risk population (who may not answer phones, or even have phones, or return survey forms). They may over-sample the higher-income populations

The results should be compared with interview surveys (bank surveys, creel surveys) of anglers.

· *Are the analytical framework (i.e., theory and mathematical application) used in the Population Centroid Approach and the Angler Destination Approach appropriate? Are the key assumptions in the two approaches described adequately, justified, and acceptable?*

Yes, they are appropriate. They are very interesting attempts to make use of large data sets. The limitations are set out clearly in Section 6, but it would have helped to mention this earlier. But again, it is a matter of the assumptions that are applied, which have to do with many different kinds of averaging, such that the variation is masked.

· *What are the strengths and limitations for each of the two approaches? How do they compare to each other? Would you recommend one approach over the other for presentation of results, or should EPA maintain both approaches and report a range of benefits?*

The document itself adequately examines the advantages and disadvantages of each method. Both approaches should be included, although I lean toward the Angler Destination Approach because it relies on more site-specific information. The lack of data on mercury levels for each distance has required either extrapolating values or eliminating blocks.

In addition, EPA should use some of the site-specific data for high risk populations (from a number of studies in the peer-reviewed literature) to examine the risk to these individuals from freshwater fish. This would give some indication of the truly high-end exposed group.

OTHER COMMENTS ON SECTION 3:

In section 3 the flow charts are very clear and helpful and referenced, to provide a roadmap through the complicated calculations. It is gratifying that the maps are intended to be read in black and white, but I found the maps difficult to resolve, and suggest some experiments with other means of depicting the five category scale, perhaps by using stiples, cross-hatching and solids.

Section 3.2 I am impressed with the elegance of what this section tries to accomplish. However, as an ecologist I find it hard to accept that the species of fish analyzed and the species of fish caught and preferred have no place in such a model. It may be that with a very large data base (thousands of samples) the inter-species differences and the species * length interactions "average out". But it would be reassuring to have this assumption stated explicitly as well as some support for the assumption.

Section 3.2.1 Should specify that fs is the annual or general fertility rate as opposed to total fertility rate. These range from 53/1000 in New Hampshire to 89/1000 in Utah. Texas (GFR=76.2/1000) had the highest number of fetuses at risk (section 3.2.2.1).

Table 3-4 (page 3-17) shows a surprising gradient of increasing mercury concentrations in lakes with increasing distance from block group centroids, whereas rivers show no difference. If the sampling procedure is random enough and the two data bases are independent, I can't see why there should be a gradient of such a magnitude (0.194 to 0.281). Is it because fish can grow bigger far from population centers?

1. Many of the statements and assumptions used in this section require references.

2. What about young children, who are also vulnerable to mercury problems from fish (children start eating fish at age 3, see Burger et al, 1999).

3. Panfish needs to be defined as to species.

4. The models should be site-specific with respect to which species of fish are present, and are eaten in each state or region.

5. They need a good paragraph on Why the 1994 NSRE data were used.

6. The biases inherent in using only the "most recent trip" need to be explored.

7. How were the households selected for the NSRE.

8. The approach for the general population may underestimate the risk to the maximally-exposed individuals within that general population.

9. More explanation of the differences in the number of individuals at risk should be included, since in some cases it is 50 % higher by one method than the other.

10. The at-risk population per state needs to be related to the population of that state for anyone to figure out whether there is a disproportionate risk for any given state or block.

11. Again, the numbers show a big discrepancy, depending upon the methodology.

12. It would help to include some site-specific studies that show higher consumption rates.

13. Further, using a mean ignores the fact that a proportion of the population is much higher, and the risk for these people is not included.

14. I am more concerned about the section on high-end exposure for a number of reasons.

-Using averages for high risk groups is a problem because again, it does not include the very high end consumers, but rather the average of high end consumers.

-The average ingestion rate was found to be similar (On page 4-11) because the same data were used to construct the model. This higher standard deviation is actually the point of interest, because it goes to the point of truly high-end exposure.

Fish Consumption and Mercury Ingestion Rates

· *Is the formula used for the calculation of mercury ingestion for a population consistent with current methods for estimating mercury ingestion?*

Yes.

· *Is the assumption that pregnant women in angler households ingest fish at rates similar to the anglers themselves reasonable? Do you have other recommendations (provide citations)?.*

The ingestion rate (number of meals/week) is similar, but the amount is different. Men eat larger meals than females (see Burger 2000, 2002b). For South Carolina, meal size for men averaged 373 g, while females averaged 232 g; they ate the same number of meals per day or week, since women cooked the fish for the whole family).

· *Is the conversion of uncooked filet to cooked fish reasonable?*

The conversion of 1.5 is probably OK, but not conservative. And (as acknowledged in Section 6) conversion should vary by cooking method (Burger et al. 2003). Burger et al (2003) suggested a conservative conversion factor of 2 X for uncooked to cooked (due to loss of moisture). We found that there is substantial variation in the amount of weight loss (hence mercury enrichment) in deep-fried fish, and that the concentration increase was as high as 78% when fish were fried and breaded (a favorite consumption method in many areas of the southeastern U.S.) But I it may be that 1.5 is realistic across many cooking methods.

· *Is the conversion of average daily ingestion rate to maternal hair concentration reasonable?*

Somehow, I didn't find this item, so no comment.

OTHER COMMENTS ON THIS SECTION:

Page 4.2. The statement (first full paragraph) that "consumption rates are certain to vary across the study area and across angle subpopulations" is certainly TRUE. The statement that "systematic recreational fish consumption data that account for these variations are not available" is not TRUE (see references below).

I do not think that the use of the 8 gm/day consumption can be considered conservative. It is equivalent to about 1/3 ounce per day (28.4 grams/ounce). This amounts to about 2 ounces/week or one 8 ounce fish meal per month. This may be appropriate for catch-and-release fishers, who only occasionally take home fish to eat, but it is not representative for subsistence fishers, nor for many recreational anglers that we interviewed. EPA should have questioned these data.

For example, we have published papers on fish consumption in New York, New Jersey, South Carolina, and Puerto Rico (see references below). One could question whether these are recreational or subsistence, but the papers uniformly identify subpopulations of anglers with consumption rates much higher than the studies cited by EPA.

Table 4.1 and 4.2 estimate the mean mercury ingestion rate (ug/day). The data are aggregated at the state level. The maximum intake is 4.7 ug/day (Maine by method 2). Which amount to 0.08 ug/kg/day for a 60 kg female which is below the EPA's Reference Dose of 0.1 ug/kg/day (based on fetal protection). But averages are misleading; it is always the high-end person who needs to be protected. This phenomenon is well-established, for example in the case of lead effects on infant development. It is not the average child for whom we worry about 3 IQ points, but the extreme child who is already at the border of retardation (and whose behavior may increase exposure as well).

For Figures 4.1 and 4.2 the labels for the X-axis should read Intake Rate (or Ingestion Rate) not "ADI". The ADI could be marked on the X axis at 6 ug/day with an arrow.

The authors recognize these points on page 4-7, but underemphasize them. Their latter estimate (2% of 354,000 exposed) amounts to over 7000 with exposure exceeding the RfD.

OTHER COMMENTS

1. Again, more references should be included.
2. The conversion from cooked to uncooked should be bounded by other published studies.
3. Annual ingestion rates of self-caught fish vary markedly in northern and southern states, since people can fish year around in these regions. Some inclusion of this factor needs to be considered.
4. Many other ethnic groups could have been included, even for the east (Toth and Brown, Fleming et al).
5. People may go on longer trips to obtain a large quantity of fish to freeze or smoke for later use, or to give to relatives (page 5-14).

SECTION 6 COMMENTS:

1. It is not clear whether the statements refer to the study area, or the whole US.
2. Again, more references are needed.
3. I am disturbed by again putting 'average mercury exposures in this section, because it is those above this level that are of concern.
4. In many cultures, non-fishers regularly receive fish from friends or neighbors, and they would not be included in this methodology. In some cultures, this number of exposed women can be large (see Toth and Brown)
5. Some attempt should be made to determine what proportion of women fall into the anglers, and married to anglers).

APPENDIX C.

The Wente model itself is a remarkable and ambitious undertaking which clearly recognizes the complexities in interpreting mercury concentrations in diverse tissues, sizes, sexes, species, locations and time, obtained by different capture, processing, and analytic methods, with different detection levels and proportions of "non-detects".

Since so much of the present report hinges on data modeled in the Wente report it was necessary to find, download and review the Wente paper.

Wente's model was designed to clarify how sampling methodology and its associated sampling errors could be dealt with in attempts to uncover temporal and spatial trends in a mercury data base. It was not designed or intended to estimate mercury levels in populations.

Moreover, the data base itself was NOT designed for assessing either trends or exposure, but in most cases for determining the need for advisories for particular water bodies.

The un-evenness of the underlying data base reflects the different investments that state agencies made in documenting mercury levels in fish.

Wente reports "Three major assumptions were made in designing this model. First, the fish-mercury concentration is linearly related to the size of a fish for any sample and cut combination and any sampling event in log-log space. Log transformation of the response variable was deemed necessary because the variability of the observed fish-mercury concentrations appeared to be directly related to the fish-mercury concentration." [Note that this is a common characteristic of biologic and ecologic data, a positive correlation between mean and standard deviation. Various scaling procedures including log-transformation are taken to minimize this. However, when dealing with real fish in a real fisherman's creel as well as size limits imposed by regulatory agencies, it is essential to realize that fish eaten by recreational anglers is likely to oversample large fish (of any species) within any given waterbody.

Wente continues: "Second, the variation in this relation's slope parameters (k) among sampling events is assumed to be small enough that each slope can be treated as a constant for all sampling events." [This simplifying assumption may be true, but Wente provides no

reference for it. It is a sweeping generalization that ignores much of ecologic variability between seasons and waterbodies as well as localized historical contamination. To the extent that most of today's and future mercury pollution will be due to atmospheric transport and regional deposition, a certain among of homogenization across water bodies may occur. I think it is essential to acknowledge that this simplifying assumption is untested.]

Finally, "Lastly, the variation in this relation's intercept parameters also is assumed to be small enough that each intercept can be treated as a constant for all species and cut combinations sampled at each sampling event." [This may well be true].

"These assumptions help to minimize the number of parameter estimates necessary to describe variation in fish-mercury concentrations for sample characteristics....which reduces the uncertainty associated with each parameter estimate (assuming that these assumptions accurately represent reality)."

The Wente paper is very new (2004) and has not been published in the peer-reviewed literature. And, I am (at least initially) skeptical about the generalization to provide a single average mercury value to each water body. It is gratifying to know that there is some internal validation for that data set.

In section 6 and again in Appendix C, the report authors acknowledge this source of uncertainty, but it still plays a central role in their work.

3. Additional references to be considered in discussing validity of assumptions

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4. Specific editorial comments, questions, clarifications on a page-by-page basis. (Some of these have been mentioned above under general comments).

PAGE 1.1 end of paragraph 2. (i.e. recreationally or subsistence caught fish). Native American fishermen do not consider themselves recreational.

PAGE 1-2 Last paragraph "top fifth percentile" is unclear. Express it as "top 5%-ile" otherwise it could be confused with "top quintile"=top 20%.

PAGE 1-3. Last paragraph. This would be a good point to expand on the assumptions and limitations and uncertainties with a few sentences. A good paragraph here would have made me a lot less skeptical as I read through section 2,3,4.

Mention here model uncertainties, input parameter uncertainties, extrapolation uncertainties, etc. It then becomes fair to say that some biases cancel out or are likely to have negligible systematic bias, and that internal validations have been conducted (e.g. Wente) and that different methods seem to provide consistent results.

Page 2-1. towards end of page, direct readers to Appendix C for more details on the data base.

Page 2-2 middle of page "eastern portion of the country" is undefined. Does it correspond to the Atlantic coast (where most of the population is), to the 38 state study area, or to what we call the "mid-west" whence cometh most of our atmospheric mercury.

Page 2-3 et seq. The maps give an impression of the main foci of contamination but not the details. They could be increased in size by judicious re-arrangement of the page and the legend. Also a different set of graphics (rather than just gray scale) might help.

Page 2-6 at the very end of section 2-2. This is where I felt myself becoming very skeptical about the utility of the assignment of average mercury levels to water bodies. Again reference to Appendix C for additional methodologic details would be helpful (see also my comments on the Wente report above). I wasn't asked to critique Wente, but it is essential to understand this report. The impact of censored analytic data, is very important and varies from state to state depending on the detection levels achieved.

Page 2-7 As an ecologist who analyzes fish, birds and other creatures for mercury, this is the page I had the most trouble with.

Need to clarify that the assumptions are those of Wente. It is not reassuring that "most of the observations were predicted to within 0.24 ppm fish tissue concentration". That seems very noisy.

Page 2-8 Six "species" are mentioned. It would be appropriate here to indicate the scientific names and the full name of the species since there are several species of bass, trout, catfish, and perch. If results are averaged over several species of catfish, for example, this can be stated.

Heading for Table 2-1. I assume that all of these values are on a wet-weight or fresh-weight basis, and this should be indicated in the table heading as (ppm wet weight).

Section 6 recognizes that anglers probably do not consume species and sizes randomly (indeed they may favor large individuals), but it would be helpful if this disclaimer were mentioned in Section 2.3.

In other words, the authors obviously know more about the strengths and limitations of their data set, than they let on in the main body of the report.

Page 2-9. I was surprised at the paucity of NJ data (Wente doesn't even include NJ data in his table). NJ has done very extensive testing of freshwater fish, and those data apparently have not reached the federal data base, even though they have been published.

Page 3-1. Here again the authors recognize in Section 6 that commercial fish may play a major role in exposure, but this should be mentioned as a caveat on page 3-1 as well.

Page 3-2. It would be helpful if the authors indicated how these data bases can be accessed (web-sites if available).

Page 3-8. It is not apparent to me whether there is an assumption that fishing distance traveled is independent of geography. Is there an impact on the data of the fact that many blocks don't have a water body within 10 mi (or a mercury-sampled water body).
Or doesn't it matter.

Page, 3-8 At this point I began to wonder about the linearity of demographics. In our interview study of 258 anglers along the Savannah River (SC) we found that people with low and high income and low and high education did more fishing and ate more fish than the middle group and this was true of both blacks and whites (Burger et al. 1999).

Likewise is fertility independent of income? or does it matter?

Page 3-9. This and subsequent flow charts are very useful.

Page 3-10 Many recreational anglers engage in catch and release. Some groups claim that 85+% of their members catch-and-release ("and wouldn't think of eating their fish").

Page 3-13. At this point I became surprised that Monte Carlo methods were not employed. This was apparently done in Section 5.

Page 4-2 Elsewhere in this report I have commented that the EPA value of 8 g/day seriously underestimates the small percentage of anglers who consume large amounts of the fish they catch. Since that is the most serious weakness of the approach, it would be appropriate to mention here that this caveat is addressed in sections 5 and discussed in section 6.

For example, "The EPA values may be characteristics of a broad population, but there is also a small percentage of anglers who consume very large amounts of fish". NOTE that there is also a small percentage of the general population (probably much less than 0.1%) who for health reasons consume large amounts of fish. They end up in doctor's offices (see Gochfeld 2003).

Page 5-2. It is obvious from my previous remarks that I think section 5 is the most important part of the report and that the earlier sections mainly establish the methodology.

On page 5-2 I would like to see a paragraph discussing the rationale for assigning ingestion rates to block groups and randomizing on the block-group level.
It seems deterministic that if the same mean value for ingestion (8 g/day) is used, the same outcome (2.9 ugHg/day) must be the outcome.

Page 5-4. The heading for Table 5-2 and subsequent tables suddenly jumps to "subsistence populations". This needs to be discussed in the text. And, it is important to emphasize that people who would never consider themselves subsistence fishers, may still be high end fish consumers. I would suggest using that term in the title, rather than subsistence.

ALSO in this and other tables, mercury ingestion is given to 5 significant figures. This creates an overly-precise impression. There probably aren't more than 3 significant figures anywhere.

Page 5-6, END OF first paragraph in Section 5.2. I do not agree with this statement. There are an increasing number of papers which provide data on fish consumption. I include references to several of our own papers on this very topic (see references above).

page 5-6 3rd paragraph. $10,000 household income is probably too low of a cut-off. $20,000 is more realistic.
http://aspe.hhs.gov/poverty/04computations.shtml
Gives a level of $18,850 as the February 2004 "poverty guideline" for a family of four.

page 5-7. Log-normal distributions are commonly encountered, but our data indicates that fish consumption data may have more of a right skew than log-normal, hence log-normal underestimates truly high end consumers.

page 5-17. The implication of the last sentence could be that "fish tissue mercury concentrations" do not contribute to the variation in exposure. The model estimate for the Chippewa seems to be at odds with studies of Indian groups in Canada, where proximity to high mercury contamination does result in high exposure.

I think the authors mean to say that "higher consumption rate contributes more to our exposure estimate than the local fish-mercury levels, although both are significant"?

page 6-1. The handling of "corresponding RfD" could be clarified as follows "which is below the RfD for mercury of 0.1 ug/kg/day", based on a 60 (or 64) kg body weight. There really isn't a corresponding RfD. There is only one RfD, and the corresponding daily intake would be about 6.0-6.4 ug/day.

page 6-3. The first paragraph seems to lead to a recommendation that there be a nationally standardized data base for contaminants in fish. EPA issues guidelines and states can follow or not follow them, hence there is currently not a uniform data base. I applaud efforts to rectify this.

page 6-3 END OF 3rd paragraph. This is the important caveat that I was looking for earlier: "If anglers systematically keep fish of different species or sizes than those included in the normalization and averaging process, then the modeling approach may lead to over or underestimates of exposure."

Anglers tend to keep larger fish and to target "game fish" which are often higher trophic level fish (faster, stronger species which fight). Advisories encourage keeping smaller fish, but fishing regulations set size limits which preclude keeping smaller fish. So the bias is more likely to underestimate than overestimate consumption.

page 6-6 footnote. "As long as the dose-response...is linear". Dose response relationships are seldom linear. None of the MeHg dose response curves identified from the Iraq epidemic are linear, although portions of the curve may be linear.

page 6-7 2nd paragraph. There is a much more extensive literature on fish advisories. Fish consumption advisories did not impact the number of fishing licenses issued in New Jersey, for example (see figure 2.9 in chapter 11 of volume 2 of NJ Mercury Task Force Report) http://www.nj.gov/dep/dsr/mercury_task_force.htm

page 6.7 4th line under Section 6.4 I do not agree with this statement. there are more data on fish consumption.

page 6-8. Last 2 sentences. We agree and provide some references in above.

Pages R1-R2. Perhaps the greatest limitation of the report is the sparseness of references, particularly in support of assumptions. There are only 13 non-EPA references of which only 5 are 2001 or later.

Appendix C. See my general comments above.
Page C-1 2nd paragraph gives a nice summary of mercury cycling. However I would make two slight changes as follows:

"In both cases, mercury may be returned to the atmosphere through volatilization. In sediment, mercury is transformed to methylmercury...."

Page C-5. It isn't clear which assumptions come directly from Wente and which from the report authors. The assumption that Hg accumulation is time dependent comes from Wente. However, it is an oversimplification. Although the accumulation of mercury with age is probably monotonic, it is not likely to be linear and it differs among water bodies depending on nutrition, hence it is not likely to be "a good approximation of the time the fish is alive prior to being caught". It is a relative indicator at best.

It is not clear from Wente what difference that assumption makes, because Wente doesn't seem to have tested or questioned that assumption.

Page C-6. A further source of hidden variation, not mentioned by Wente, is that length is sometimes reported as total length (snout to tip of tail fin) and sometimes as "standard

length" (snout to base of tail fin). It is likely that both measures are used interchangeably in the data base, and although they are highly correlated across fish, they could impact the Wente data by a certain percentage which differs across species.

Page C-16 The last paragraph is unclear. "...concentrations for consumable fish too small for consumption....":

To: James Neumann, Principal, Industrial Economics
From: James F. Gilliam, Professor, N. C. State University
Re: Review of "Assessment of Mercury Exposure…," E. H. Pechan and Associates
Date: December 23, 2004

I have studied the above referenced draft report (hereafter called the Pechan report) thoroughly, both to understand the overall strategy of the approach, and to assess the soundness of the details of the methodology. As requested, I focus especially on Section 2 and Appendix C. In doing so, I found it important to also read the original Wente (2004) report, and to consult the SAS online manual regarding the LIFEREG software module.

Subject to resolution of perhaps substantial problems I address below, I feel the methodology in Section 2 and Appendix C are reasonable approaches to the problem addressed. While I can envision improvements to the Wente NDMMFT statistical model or development of process models to predict mercury concentrations in fish, I know of no presently developed model that would be better to use. The NLFA (aka NLFWA) is by far the largest database available, and despite problems with the data that are mentioned in the report, there is no alternative except possibly the NFTS database. Since the NLFWA database is so much larger than the NFTS database, I would favor use of both datasets, rather abandonment of the NLFWA database in favor of the NFTS database, if the NFTS database is used in future work.

However, I did find some potential problems of concern in Section 2 and Appendix C:

1. The application of the Wente model, or possibly the model itself, appears to have a serious deficiency which is manifested in Figs. C-4 and C-5 on page C-10. Those figures show a systematic and substantial underprediction of mercury levels in fish with high observed levels of mercury. In particular, with concentrations ranging from zero to about 5 ppm, fish with observed concentrations of about 2 ppm or higher are substantially underestimated. No information is given on what these fish are, but I would anticipate that many of them are large individuals, and probably also piscivorous, and large piscivores are historically among the most favored fish for consumption. The report acknowledges (p. C-16) "a slight under-prediction of higher values... [that would] be acceptable…", but I disagree with the characterization of the underprediction as slight. For example, the mean residual for fish observed to have 3ppm appears to be about -1.5 (Fig. C-5), and hence the mean predicted value about 1.5 ppm (Fig. C-4), an underprediction of about 50%. Possibly these high-mercury fish are mainly large fish likely to be consumed, although, again, no information is given to let me assess that. As presently applied, the model would de-emphasize potentially real "hot spots."

Why does that underprediction occur? First, possibly mercury concentration does not empirically follow the power law posited by Wente, or there is some other fundamental shortcoming in that model. I think that is unlikely, but a relationship between length and mercury that does not follow a power law could produce that result. Second, another

possibility that I raise reluctantly is that I could not be certain from the presentation in the draft document that the model was implemented exactly as Wente intended. Hopefully only a better explanation is needed, and I return to that matter below. Third, possibly there is no real problem; the model predicts mean values (actually, maximum likelihood estimates) for a given set of values for the independent variables, and possibly the high observed values are just unusually high values for a given fish species, length, cut, sampling site, and sampling time, "balanced" by individuals with low values for that same set of values. That is, possibly the large observed values are outliers for which a model should indeed underpredict ("temper") the observed values; when statistical models explain little of the variance, a negative slope on that plot in Fig. C-5 can be expected, as can a slope <1 on Fig. C-4. However, we cannot assess that possibility from the information presented. A more useful analysis would examine, e.g., the residuals as a function of fish length, stratified by species. Examination of residuals plotted against values of the independent variables are important tools to understand the source of the seeming prediction bias suggested by the those figures.

2. From Pechan report alone, I could not picture what exactly the Wente NDMMFT model was, as the description on p. C-5 and the identical description on p. 2-7 were not especially helpful, and are potentially misleading. In particular, on p. 2-7, the equation does not seem to be the Wente model if the explanations of the matrix X and the vector Beta are accurate. In the Wente (2004) model, two sets of parameters are estimated: a set of slopes as a function of the log of fish length, with one slope for each species and "cut" of tissue, and a set of intercepts, with one intercept for each sampling site and time. However, the description in the report has the model estimating only the slopes; the intercepts seem to be described as independent data given in the X matrix. Further, the parameter sigma is not explained; it appears from reading Wente that sigma is set equal to 1. Also, in the Wente model the distribution of errors in mercury concentration are assumed to be log-normal, not normal as stated in the present report; it's the errors in the log of mercury concentration (actually, concentration +1) that is assumed to be normal in the Wente model. The most serious problem is the matter of stating that the intercepts that estimate site/time effects are given as part of the X matrix, rather than parameters to be estimated, if I understand all of that correctly. Reading the explanation of the LIFEREG procedure in the online SAS manual, I see that the text in Pechan is a paraphrasing of the wording in the SAS manual, but it should instead be a characterization based on Wente's publication. As written in Pechan, taken literally, it appears that Pechan implemented LIFEREG differently than what was done by Wente. I have a queasy feeling that possibly the poor predictive performance in Figs. C-4 and C-5 might be attributable to a problem with implementing the Wente model for the Pechan study, e.g., that parameters were not estimated as Wente intended, e.g., missing a log transform and hence the power law assumption, or some problem with estimating the site-specific parameters. I hope that is not the case, but if Wente is not part of the present study, I feel he should be contacted to be certain all was done as intended.

Given these problems, it might be useful for me to outline Wente's approach, for use by another reviewer or whomever, as it is much simpler than might be surmised from the Pechan report. Wente's idea was to model mercury concentration as a power function of

fish length, i.e., $y = ax^b$, where y = mercury concentration, x = length, and a,b are parameters. Potentially, that could be done for each species at each site, but the data are far too sparse for that, and, further, he wanted to devise some way to predict mercury concentrations for a species that was not even collected at some given site. So, he took the leaps of (i) assuming a universal, national value of "b" for each tissue type from each fish species, and (ii) assuming that the value of "a" at a given site at a given time is the same for all species and tissues there. Thus a prediction of mercury concentration for a given tissue from a fish of an arbitrary species of length x could then be predicted by "looking up" the right value of "b" for that species and tissue and the value of "a" for that site and time. All the rest is just implementation of the idea. He log-transformed the data to model the relationship as log y = b * log x + log a, added 1 to x and to y to escape the problem of taking the log of zero, and used the SAS program LIFEREG to estimate the parameters. I am not certain that the survival program LIFEREG was the best way to do that, and the authors of the current report might have doubts as they make a statement aimed at justifying the use of a survival model. Also, I don't immediately see the reasoning for making "a" site-specific but "b" species-specific in the power law, rather than, e.g., vice versa, but I understand that assessment of the Wente model is outside the charge of this review.

At a minimum, the account of the Wente model in the present report must be improved. At worst, if the account on p. 2.7 is literally correct, the model seems to have been applied differently from what was done by Wente. I suspect, and hope, it was applied correctly, and only the explanation need be improved.

3. The Pechan report says on p. 2-8 that a "simple average" across species and dates was taken for each of the 1,388 lake sites and the 2,739 sites. However, the report is not clear how that was done:

(a) the report says that estimates of mercury in "bass, trout, catfish, crappie, perch, and walleye" were made. This statement must be clarified, providing latin and common names for what species were actually used. For example, I might have guessed that "perch" was meant to mean yellow perch, *Perca flavescens*, but could not be certain. However, on p. C-7, there is a reference to "perch – white." The "white perch" is an entirely different taxonomic family than the yellow perch, illustrating a problem in using common names. Similarly, "bass" can refer to at least six species likely to be in the database, representing more than one taxonomic family, and with quite different ecologies. Trout, catfish, and crappie also come in different species. Only "walleye" is unambiguous.
(b) the report does not say whether, for the above "species," estimates were made for (i) only the species collected at the site (defeating one of the purposes of the Wente model), (ii) those species likely to be there based on known ranges, (iii) all six "species" regardless of whether they are likely to exist at the site, or (iv) something else. It is very important to clarify here.
(c) the report does not say for what length fish the estimate was made, vs whether the lengths in the sample was used, or what.

(d) the report does not say whether the six "species" were equally weighted in the "simple average," or whether individuals were averaged, or what.
(e) the report does not say whether consideration was given to what sizes of fish are likely to be eaten.
(f) the report does not say what "cut" of meat was used to compile Table 2-1.

All the above needs to be exactly clarified. Ideally, to me, the method would use an estimate based on species likely to be present at the site (but not necessarily in the sample, per Wente), and sizes likely to be consumed. If the text had been clear on exactly what had been done, possibly I could have suggested improvements.

The above items are the main issues I identified. I add the following remarks:
1. The assignment to "lake" or "river" categories is likely to be reasonable, albeit inexact. Species compositions at the site would be another clue, but would require biological expertise that may not be worth the trouble/cost.
2. On p. C-4, there is a reference to 27 different sampling methods. It might be helpful to readers to say here, as done later, that those will be reduced to three categories via deletion and consolidation.
3. p. C-5: again, the description of the model is not good. In addition to my remarks above, I suggest that it be made clear that there is one just one "covariate" (continuous variable), i.e., length, and apparently two categorical variables (specifying fish species/cut, and location/time). It would be best to write out the exact Wente model rather than the canned description of LIVEREG from the SAS manual.
4. p. C-6: Item 9 refers to time-consuming efforts to obtain world record lengths, and hence discarding of some data. It would have seemed easier to get length ranges from any of many "Fishes of …" books available. E.g., Fishes of Ohio, etc., or a compilation of such data in a series of volumes by Carlander. These species accounts typically include information on size ranges.
5. p. C-6. The use of length-weight regressions to estimate lengths of Mississippi fish from their weights is reasonable.
6. p. 3-4: Use of the term "panfish" needs to be clarified. "Black bass" presumably means smallmouth bass plus largemouth bass, but might include other species. Again, clarify.
7. p. 3-12: Here, I had notations about being puzzled about what would be done when no samples occurred on a given distance interval. It would be helpful to give some indication here, or earlier in the report, although it is covered later.
8. p. 2-9: "The results reported in the remainder of this report are based on the mercury concentration estimates reported in Table 2-1." From that wording, I thought that perhaps the actual numbers in the table, i.e., statewide averages, would be used. Only in Section 3 did it become clear that was not the case, to my relief. I suggest rewording to clarify, e.g., "…based on the data from the HUCS, as summarized by state in Table 2-1." That's not the best wording, but something to make it clear that the data are not reduced to a state level in the coming applications in Sections 3 and 4.
9. p. 3-24: the number "8" for DC seems stunningly low, but if that's the estimate (i.e., almost nobody fishes there), so be it.
10. p. 6-2: in the penultimate line, the text refers to "normalized conditions (size, species, and cut of fish)." However, I could not find that information clearly stated in Section 2!

11. p. 6-3: the NLFA is said to be variable, but not likely to be biased. However, I might surmise that "hot spots" would be oversampled in those state data?
12. p. 6-3: in Section 2 and on this page, no biological or other specific motivation seems to be explicitly given for separation to "lake" or "river" categories. You might as well say why that was done.

Summary and final remarks:

The task accepted in the report is an important one but one with much uncertainty given the data available. I do feel the overall three-step strategy is reasonable. The methodology does appear to represent the readily available "state of the art" approaches as far as I know, although the other experts addressing the other sections will know better about those areas. If the problems mentioned here can be addressed, it is a reasonable prototype, at least, for addressing consequences of regulatory alternatives. In further developments, approaches such as placing species into trophic guilds, smoothing across sites, or using process models might improve precision and accuracy of estimates. The report is written reasonably well in terms of grammar and flow, but my comments on Section 2 indicates that there are matters of clarification and documentation of exactly what was done that need to be much improved.

Specifically regarding mercury levels in fish tissue (Sec 2 and Appendix C), the application of the NLFA data appears reasonable, but I am unclear whether the actual application was done as Wente intended, and/or whether the Wente model is itself somehow flawed, given the problems listed above. This matter needs serious attention and maybe renewed brainstorming, including pulling Wente or perhaps additional statistical expertise into the matter, if Wente has not "signed off" on what was done. Pulling NFTS data into the mix would also be a likely future improvement. The description of how the data were actually, exactly "normalized" to obtain the numbers in Table 2-1 and elsewhere must be improved in the text. The uncertainty in the estimates might be reduced by, e.g., placing species into functional feeding groups to pool their data for estimates of the slopes in the Wente model, but substantial variation will remain. The matter of bias in the predictions is of greater concern, specifically possible underestimation of values >2 ppm, and severe underestimation of observed values >3 ppm, unless those really are determined in further analyses to be high values that should truly be revised downward in estimating mean levels for that combination of independent variables.

I am not aware of better currently available approaches to predicting mercury in fish flesh on a national scale. I remain unclear how, or whether, the "simple averages" for sites include information on fish length and species composition. If that information is not used, accuracy could be improved by doing so. Finally, it needs to be made exactly clear what the six "groups" (bass, perch, etc.) were, and exactly how the calculations based on those six groups were used to compute a single number to be used for each site in subsequent analyses. Without knowing what was done, I cannot comment on the appropriateness of what was done.

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December 30, 2004

Jim Neumann, Principal
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Re: Peer Review of *Assessment of Mercury Exposures to Women of Childbearing Age From Consumption of Noncommercial Freshwater Fish in the U.S: Revised Methodology Report*, November 2004.

Dear Dr. Neumann:

Thank you for the opportunity to review the *Assessment of Mercury Exposures to Women of Childbearing Age From Consumption of Noncommercial Freshwater Fish in the U.S: Revised Methodology Report*, dated November 2004. The report describes proposed methods for estimating human exposures to mercury through freshwater fish consumption. These methods will be used to provide estimates of the economic benefits of reducing mercury air emissions.

This letter includes the results of my review, including comments on the topic areas listed in your letter of December 7, 2004. This letter begins with comments on general topics followed by comments on specific topics that are related to my areas of expertise.

General Topics

1. Are the methods, models and data presented and used in this methodology reflective of the current state-of-the-art you are familiar with in this field?

In general, yes, although they could be improved in accordance with the following suggestions.

Data used to estimate average fish tissue concentrations. The authors used measured mercury concentration data instead of attempting to model these concentrations given the limited understanding and availability of models to predict bioaccumulation of mercury in fish. They appropriately note important limitations of the measured data. For example, the data were collected in association with the development of fish advisories and might provide average mercury concentrations that are biased high. The report could be improved by providing some additional discussion of why modeling is not a reasonable option due to the fact that defensible model(s) are not available and, even if they were, it might not be possible to use them in a way that is representative of such a large, heterogeneous study area.

The data are combined to estimate an average mercury concentration in fish. This is a common approach, but it would be helpful to provide a more complete treatment of these data by developing distributions of mercury concentrations. A single distribution could be developed that is representative of the relative proportion each species is expected to contribute to the target population's diet. On page 2-9, the authors also indicate the utility of such an alternative weighting to better reflect the target populations' diet.

The assessment incorporates data for bass, trout, catfish, crappie, perch, and walleye and normalizes these data using the National Descriptive Model of Mercury and Fish Tissue (NDMMFT) to convert concentration data for one species/size/sample method to an estimated concentration for a defined species/size/sample method that is relevant to target population exposure. In general, such an extrapolation is important and reasonable. Appendix C describes how "skin-on" and "skin-off" data were handled, but it is not clear how this information was used in defining exposures, including the cooking conversion factor (See related comment under *Specific Topics*). More discussion on this topic would be helpful in interpreting results of this assessment.

The report clearly acknowledges the limitations of the NLFA dataset, including EPA's lack of control over how samples were collected and analyzed. The report also discusses implications associated with the fact that data from states did not meet pre-established criteria and were subjected to further analyses before being used in the assessment. Some additional discussion of efforts to ensure data quality would be useful, particularly in light of recent emphasis on this issue.

Data used to estimate the size of the target population. The analysis incorporates the number of anglers who participate in freshwater fishing in lakes, ponds, or rivers. I am not aware of more representative or higher quality datasets. It would be useful to quantify the extent to which this approach might overestimate the size of the target population if some anglers do not use the fish they catch for the purpose of consumption and, more importantly, do not share the fish with pregnant women for the purpose of consumption. Because I do not know of a better dataset, this issue is worthy of some discussion but additional data analysis might not be possible. Did the authors investigate whether there are any studies regarding the fraction of freshwater anglers who consume their catch or share their catch for the purpose of consumption?

Models. The exposure model used to predict average daily dose of mercury is generally consistent with current practice, although some clarification of inputs would strengthen the report. Also, average fish tissue concentrations are commonly used in risk assessment for this exposure pathway because fish consumers are likely to consume a range of fish species with varying concentrations of mercury, thus "averaging" their exposure. However, the prenatal exposure under consideration is a relatively short exposure period; therefore, the authors should provide some discussion regarding the representativeness of an average concentration for this period of exposure. Ultimately, this is a risk management decision that EPA must make, but it is important that the thought process is explicit.

The authors accounted for sampling method (e.g., skin-on, skin-off, whole body) in predictions of mercury concentration. In the exposure model, they refer to uncooked fillet

concentrations. Did the authors consider fish consumption practice variability across the 38 states with respect to cooking practices and the part of fish consumed and whether these factors might influence mercury intakes? How does mercury concentration vary among species? Were all data for all species combined without consideration of what species and fish parts anglers are most likely to consume? If so, then the approach assumes anglers consume different fish species in proportion to their prevalence in the database. Is it possible that there are species more highly favored by anglers for consumption, which also have comparatively high mercury concentrations?

Methods. The authors provide estimates of average daily mercury exposure, expressed as a mean and a standard deviation. They also consider the sensitivity of results to assumptions about fish consumption practices by performing separate analyses for the general population and several subpopulations (i.e. individuals at the high end of the consumption rate distribution; individuals in low-income, high consumption households; and two ethnic subpopulations). As a result, it is clear that the authors are sensitive to the regional, cultural, and socioeconomic factors influencing fish consumption patterns.

The analysis would be easier to review and results would be easier to interpret and use if a fully probabilistic analysis were performed in which the variability and uncertainty in each model input were described as well as possible. EPA and others are increasingly using probabilistic analysis to answer similar questions about human exposure to chemicals in the environment. Such analyses have been performed recently for human exposure to chemicals in CCA-treated wood (EPA's Draft Preliminary Probabilistic Exposure and Risk Assessment for Children who Contact CCA-Treated Playsets and Decks and CCA-containing soils around these structures (http://www.epa.gov/oscpmont/sap/2003/index.htm#December) and PCBs in fish caught and consumed from the Housatonic River in Massachusetts (http://www.epa.gov/boston/ge/reportindex.html). They allow the analyst to better understand how the variability and uncertainty in model inputs affect model output. The most important uncertainties appear to be those associated with the mercury concentration estimation model, the centroid and angler destination methods, and the fish consumption rates. The authors quantify some fraction of total uncertainty by reporting a standard deviation for HgI estimates. To the extent uncertainties can be quantified, probabilistic analysis might help in choosing between the centroid and angler destination approaches as a basis for decision-making. Even with a probabilistic approach, it might be important to conduct separate analyses given the large size of the study area (i.e. 38 eastern states of the United States), and it would be important to choose illustrative example analyses rather than exhaustive analyses for all geographic areas (e.g., census blocks or states).

The comparison of exposure estimates to EPA's reference dose (RfD) is appropriate, and the authors acknowledge that the target population is exposed to mercury from other sources. However, they provide no indication of the magnitude of other exposures. The reader is left wondering whether other exposures might contribute significantly to total mercury exposure experienced by the target population or whether the freshwater fish consumption pathway dominates the target populations' mercury exposure.

2. Given the scope and intended use of the methodology, does the analysis reflect all relevant studies and methods to assess population-level exposure to mercury from freshwater fish?

I noted no important omissions, but the report could be improved with some reference to any other attempts to estimate population exposures to mercury from fish consumption and how this analysis compares to and improves upon such efforts. For example, Dr. Alan Stern published such an analysis for consumption of recreationally caught marine fish species by New Jersey residents.

3. Is the methodology of sufficient quality and comprehensiveness to serve the intended purpose (that is, to support benefits analysis for regulatory alternatives to reduce airborne emissions of mercury)?

Generally, yes. The geographic area being studied includes mercury sources other than airborne emissions. Therefore, predicted exposures likely reflect total mercury exposure from fish consumption, not just airborne mercury exposure from fish consumption. Also see comments under *General Topic* #1 above.

4. Given the scope and intended purpose of the methodology, are the analytical framework, simplifying assumptions, degree of modeling precision, and application of data appropriate? Should the methodology be refined to reflect factors or conditions that are not considered but are likely to affect key target variables?

The analytical framework, simplifying assumptions, degree of modeling precision, and application of the data are reasonable except as otherwise noted in these comments. The authors note an important modification that is needed to improve mercury concentration estimates by better defining a diet representative of the target population.

5. What are the overall major strengths and weaknesses of the methodology?

A major strength is the use of the NDMMFT model to make mercury fish tissue concentration data more representative of actual exposures. To the best of my knowledge, the authors have identified and used appropriately mercury concentration data and angler information. Another major strength is the use of GIS to assess spatial distribution of mercury concentrations and exposures. A major weakness is the discussion and selection of fish consumption rates for both the general population. Sufficient data are available to permit a more sophisticated treatment of the data than, for example, simply assuming a lognormal distribution with a mean of 8 g/day and a 95^{th} percentile of 25 g/day based on EPA's 1997 Exposure Factors Handbook (See references provided under *Specific Topics*). Also, a probabilistic approach would improve the assessment by providing a clearer, comprehensive assessment of variability and uncertainty as they pertain to risk management decision-making.

6. Are all of the essential elements included in the final report? Is the report clear and well-written? What additional documentation, if any, do you feel is needed?

The document is well-written, clear, and contains essential elements for estimating population exposure to mercury from fish consumption. My comments largely involve requests for additional discussion.

Specific Topics

Mercury Levels in Fish Tissue

1. Is the application of the NLFWA data in this methodology appropriate given the intended scope and purpose of the analysis?

Yes, but some additional discussion is needed regarding the limitations of this application given that concentrations result from advisory development. Therefore, they are likely to overestimate exposure. Specifically, EPA explains that it cannot draw conclusions or identify trends from this national listing because states, not EPA, determine the scope and extent of monitoring and how to decide which waters should be placed under advisory (http://epa.gov/waterscience/fish/advisories/questions.htm). An alternative is to model fish tissue concentrations. Admittedly, this task would be enormously difficult given the scale of the effort and variability in bioaccumulation potential across 38 states, but this option deserves more than a couple of sentences, which is all that appears in the report now. In the end, I would agree that modeling is not a feasible option, but some additional discussion supporting this conclusion would help those using this report.

The report provides limited rationale for the species that were selected for use in the assessment. The selected species are popular among anglers, and the report identifies them as species commonly targeted by anglers. What sources were consulted to confirm that these are the most commonly targeted species among freshwater anglers for the purpose of consumption? How well do the selected species represent the mix of species an angler and others are likely to consume? Were data available for each species and used in proportion to each species' likely contribution to the target populations' diet? (The authors recognize this as a potential area of future work, and I agree that it would be useful to address this question, however difficult.) Were mercury concentrations considered in selecting species?

On page 2-2, the authors indicate that "[m]ercury samples were also selected from the NLFA data if they met the following criteria: … samples from primarily freshwater species…" What is the significance of the word "primarily" in this sentence? Elsewhere in the report, the authors explain that saltwater species were excluded from the analysis.

2. In future enhancements to the methodology, would it be appropriate to combine the NFTS data with the NLFWA data for use in this methodology?

It would be useful to conduct analyses with the two datasets and compare results as a form of uncertainty analysis. One would lose the major benefit of the systematically collected NFTS data by combining it with the non-systematically collected NLFA data (Note: EPA website indicates a name change from NLFWA to NLFA; therefore, the latter acronym is used in all comments). Also, the NFTS represents 500 randomly selected reservoirs and lakes from the 270,000 lakes and reservoirs in the contiguous United States (http://www.epa.gov/waterscience/fishstudy), while the NLFA incorporates data from lakes

and rivers. EPA indicates that "[l]akes are the focus of this [NFTS] study because they are environments where contamination accumulates and is more readily detectable."

In the comparison, it would be interesting to note if mercury concentrations are higher in the NLFA data than the NFTS data given that NLFA listings are related to advisories. This comparison should first be made separately for each waterbody type. The NLFA data might incorporate other biases that should be discussed in the report in more detail.

3. Is the method for standardizing the data using the NDMMFT model appropriate?

With the exception of related comments provided in this letter, the general method for standardizing the data appears to be appropriate.

4. Is the degree of uncertainty introduced by the use of the NDMMFT model acceptable?

On page 2-7, the authors report that a "residual standard deviation of 0.24 indicates that most of the observations were predicted to within 0.24 ppm fish tissue concentration." This amount is somewhat large in comparison to predicted average mercury concentrations shown in Table 3-4. [On page 2-7, the authors indicate that "approximately 8 percent of samples were used to validate the NDMMFT, but on page C-13, they say "approximately 10 percent).] Also, the residuals plot on page C-15 reveals a downward trend suggesting that predicted mercury concentrations are underestimated at higher measured mercury concentrations. The authors note this uncertainty but attribute little importance to it. Still, it would be helpful to explore why this trend exists. Nevertheless, without a more detailed assessment of the NDMMFT model, a method for improving accuracy of predictions is not apparent. Assuming no better data or mechanisms for improving the NDMMFT model are available, this degree of uncertainty is acceptable but must be considered in interpreting and using results for risk management.

5. Are there other methods that you are aware of to calculate the mercury concentration in fish tissue in freshwater species? EPA has used a simple average of the samples in the NLFWA for other analyses. This approach does not take into account the difference in species and size of species in determining the degree of mercury contamination in an area. Would this approach be preferable for this analysis?

I am not aware of other methods. Also, I think that the general approach involving use of NDMMFT, even with its inherent uncertainties, is an improvement over simply averaging NLFA data. Such simple averages have their own set of uncertainties that are not discussed or quantified in the report. I encourage consideration of comments provided elsewhere in this letter for improving and clarifying use of mercury concentration data.

6. Is the use of GIS analysis to assign samples to waterbody type an appropriate and reasonably accurate method?

I am not expert in the use of GIS, so I cannot comment on whether there are better GIS methodologies. However, use of GIS for this application is appropriate and appears to be reasonably accurate.

7. Are the choice of representative species and size of fish chosen for the benefits calculations appropriate?

They appear to be, but as noted above, more discussion about current fishing behaviors should be described to support the selection of species.

Exposed Population and Spatial Distribution of Fishing Behaviors

8. Are the two survey databases (NSFSWR and NSRE) good sources of information on anglers and their behavior (i.e., number of anglers, number of fishing days, location of fishing trips, demographic information for anglers)? Are the survey respondents representative of the entire population in the study area?

They appear to be reasonable, but I do not have expertise specific to these databases. On page 3-6, a note is provided that refers to data from the Pacific Northwest. I am unsure of the relevance of this note because the study area for the assessment does not include this region.

9. Are the analytical framework (i.e., theory and mathematical application) used in the key assumptions in the two approaches described adequately, justified, and acceptable?

Yes, they are described adequately. Fishing behavior is a critical part of the analysis, and the authors have focused much of their attention on these data and addressing uncertainties associated with its use.

10. What are the strengths and limitations for each of the two approaches? How do they compare to each other? Would you recommend one approach over the other for presentation of results, or should EPA maintain both approaches and report a range of benefits?

In general, I favor the approach used in this assessment of providing risk managers with the results of two plausible, reasonable approaches. I do not have any strong reasons to recommend one over the other. However, as noted earlier, use of probabilistic analyses to quantify uncertainty associated with each approach might help in making this decision, at least to the extent that one can quantify such uncertainty.

Fish Consumption and Mercury Ingestion Rates

1. Is the formula used for the calculation of mercury ingestion for a population consistent with current methods for estimating mercury ingestion?

Yes, however, more justification is required for the selected consumption rate. Two more recent reviews of fish consumption rate data for freshwater systems and marine water systems throughout the United States should be considered:

Moya (2004) in the December issue of the journal *Human and Ecological Risk Assessment* for a recent review of fish consumption rates in the United States.

California's Office of Environmental Health Hazard Assessment (2001) (http://www.oehha.ca.gov/fish/special_reports/fishconsum.html).

Also, note earlier related comments under *General Topics*.

2. Is the assumption that pregnant women in angler households ingest fish at rates similar to the anglers themselves reasonable? Do you have other recommendations (provide citations)?

I do not know of any studies that address this specific question. We recently conducted a creel survey of marine recreational anglers, but the data are not yet publicly available to the public. We asked anglers to report their fish consumption patterns as well as those of others with whom they shared their catch, including women of child-bearing age. When it becomes available, our study, and others like it, could be used to look at the relationship between anglers and others consuming their catch. For example, it is possible that the recent creel survey conducted for the San Francisco area has such data, but it would be more useful to obtain data relevant to freshwater fisheries. (See the San Francisco Bay Seafood Consumption report available at http://www.sfei.org/sfeireports.htm).

3. Is the conversion of uncooked filet to cooked fish reasonable?

Morgan et al. (1997) estimated a "preparation factor" defined as the ratio of mercury concentrations in fish as prepared for consumption to mercury concentrations as measured in typical environmental monitoring programs (skin-on fillets). They examined fish preparation and consumption practices of two communities of Chippewa living on the shores of Lake Superior in northern Wisconsin. The most commonly consumed species were walleye and lake trout, and commonly used cooking methods were panfrying, deep-frying, baking, boiling, and smoking. These methods were duplicated in the laboratory, and preparation factors across these methods for the two species ranged from 1.3 to 2.0. These estimates suggest a fairly narrow range of conversion factors, and the 1.5 used in this assessment is near the center of the range. Therefore, it appears to be reasonable. Performance of a probabilistic assessment would permit use of all of these data instead of being forced to choose a single value.

Morgan JN, Berry MR, Graves RL. 1997. Effects of commonly used cooking practices on total mercury concentrations in fish and their impact on exposure assessments. J. Expo. Anal. Environ. Epidemiol, 7(1):119-133.

The authors of this report have undertaken a complex exposure question. Inevitably, reviewers will find information gaps and areas requiring clarification with efforts of this scale. I hope that my comments are helpful in this regard, and I appreciate the opportunity to contribute to the review of this important report.

Sincerely,

Donna J. Vorhees, Sc.D.