Appendix 1-2: Comments on the 2003 Everglades Consolidated Report from Outside Persons and Organizations

These comments were provided to the public
on the District's WebBoard

With the Exception of reformatting some of the information for better readability, the appendices were not edited or spellchecked by the ECR production staff and appear as posted on the District's WebBoard.

Posted on the SFWMD WebBoard September 30, 2002 11:06 AM

Peer Review Panel:

The Community Watershed Fund is a science-based nonprofit committed to helping others to manage watersheds (www.cwfund.org). We would like to address Chapter 5 of the 2003 Everglades Consolidated Report, Development of a Numeric Phosphorus Criterion for the Everglades Protection Area. The following comments are the express opinion of the Community Watershed Fund.

We are in agreement with the Florida Department of Environmental Protection (Department) that an imbalance in Everglades' flora and fauna can reasonably be related to phosphorus enrichment. We are also in agreement with the Department that the imbalance in Everglades' flora and fauna reasonably occurs between stations E4/F4 and E5/F5 in WCA-2A, and X2/Z2 and X3/Z3 in WCA-We are not convinced that the surface water phosphorus criterion should be 10-ppb total phosphorus (TP). Nor are we convinced that the measurement methodology fully encompasses normal variability in surface water phosphorus concentration.

The WCA-2A mesocosm studies indicate a relationship between surface water phosphorus and periphyton, with a decline in calcareous periphyton first occurring at an orthophosphate loading rate of 0.8 g P/m2/yr. McCormick and O'Dell (1996) estimated that the shift in periphyton species occurred at surface water concentrations of 10 to 28 ppb TP, and McCormick et al. (2001) estimated the shift as occurring at pulses of 14 to 46 ppb SRP. The Department has not attempted to derive the phosphorus criterion from the mesocosm studies, correctly recognizing that loading rate does not easily translate to surface water concentration. Furthermore, periphyton response to orthophosphate may not mimic periphyton response to Stormwater Treatment Area (STA) outflow, which tends to be predominantly comprised of the less bioavailable dissolved organic phosphorus and particulate phosphorus.

Gradient study conclusions regarding the effect of surface water TP on flora and fauna are confounded by the presence of a second gradient – soil TP. This second gradient makes it impossible to precisely define the relationship between surface water TP concentration and flora and fauna.

Reference stations were used to determine the average surface water phosphorus concentration in unenriched parts of the Everglades Protection Area. Unfortunately, this approach tells us only that flora and fauna are not imbalanced at these surface water concentrations, and not the concentration that causes an imbalance.

In addition, the calculations of average surface water phosphorus concentrations at the reference stations may be biased low by the decision to only use data collected in > 10 cm of water. Surface water phosphorus concentration varies inversely with water depth (reference stations 1994 through 1999 $r = -0.21$, $p < 0.0001$; also Smith and McCormick 1996). Therefore, higher phosphorus concentrations associated with normal low water conditions were eliminated from calculations. A 10-cm depth seems excessive, and we would suggest that accurate samples could be collected from 5-cm depths and less. In addition, 52 percent of samples used in the phosphorus calculations were collected in > 50 cm of water, and 83 percent were collected in > 30 cm of water, so the phosphorus mean is further biased.

Ideally, we would have estimated the phosphorus concentration at which imbalance occurs by exposing reference stations to post-STA-like water. Alternatively, we could have exposed reference station community components to post-STA water under controlled conditions. No such experiments were conducted.

We also have some concerns that the proposed measurement methodology – 5 year annual geometric mean of < 10 ppb, up to 15 ppb in any individual year – inadequately represents actual variability within the system. Smith and McCormick (1999) suggested that at least a 10 year period of record is necessary to identify mechanistic relationships between environmental variables (e.g., inflow and marsh TP). Yet the period of record for most reference stations is only 7 years in WCA-2A and 5 years in WCA-1.

Application of the measurement methodology to the reference stations indicates that normal variability is not encompassed. Station U3 in WCA-2A exceeds 15 ppb TP in 1985, and exceeds the five year average of 10 ppb in 6 of 11 periods. The Department eliminated the 1985 data (as well as 1984 and 1992) because of too few samples. However, we cannot know if the 1984, 1985 and 1992 values are representative or not, and they should not be dismissed so easily. Reference stations in WCA-1 would also be out of compliance according to the proposed methodology. One of the annual geometric means for the combined stations, and 6 of the 20 individual station means, exceed 10 ppb. The Department attributes these excursions to the lack of sampling platforms, differences in sampling methodology, and abnormal events like drought conditions, fires and hurricanes. The QA/QC plan should have negated sampling bias, and drought conditions, fires and hurricanes are normal events that should be accommodated by the measurement methodology.

Our concerns about variability are furthered by initial examinations of WCA-2A sample size. To define reference station surface water phosphorus concentration on any given day with a precision of 1 ppb, 96 to 963 samples would have been necessary (1994 data). At most, 3 samples per day were collected. To characterize reference station annual surface water phosphorus concentration with a precision of 1 ppb would have required 187 to 2,020 samples. Only 49 to 94 samples were collected in a year (1994 to 1999). About 800 samples would have been required to precisely characterize reference station surface water phosphorus concentration over the entire study period (1994 to 1999), but only 436 samples were collected. The problem of sample size becomes increasingly problematic when considering individual stations, especially impacted stations. From 160 to 5,490 samples would have been required to characterize U3 annual surface water phosphorus concentration to within 1 ppb, yet only 9 to 19 samples were collected (1994 to 1999). At F4, an marginally impacted site, 654 to 115,882 samples were required to characterize annual phosphorus concentration to 1 ppb, but only 9 to 18 samples were collected (1994 to 1999).

Precisely defining the surface water phosphorus concentration that causes an imbalance would be less critical if we had the ability to treat water to the proposed criterion level. Unfortunately, the best operational scale treatment performance achieved to date was 13 ppb TP (geomean) by STA-1W Cell 4 during a particularly favorable two year period (1998 to1999). Mesocosm and Test Cell experiments have sporadically achieved effluent values around 10 ppb TP under low hydraulic loading conditions and with artificial substrates. Replicating these conditions, or results, at an operational scale is unlikely. Opportunities for optimizing the STAs exist, including establishment of submerged aquatic vegetation, elimination of hydraulic short circuiting, internal berms, and flow equalization basins. However, current information suggests that a lower limit for phosphorus removal by green technologies is defined by the ability to remove recalcitrant DOP, and internally generated PP. Pilot-scale Chemical Treatment with Solids Separation (CTSS) has achieved 10 ppb TP; fiscal and technical feasibility, and impacts on downstream biota, is currently unknown.

Even if we could treat phosphorus enriched runoff to 10 ppb TP, soil phosphorus flux in impacted areas would preclude compliance for an extended period. According to District models (1999) 5 to 20 years or more may be required to eliminate bioavailable soil phosphorus, reverse eutrophication and reduce cattails in imbalanced areas. The Department's proposed criterion and measurement methodology do not address this situation.

In summary, we agree with the Department that flora and fauna imbalance is related to phosphorus enrichment, and where the imbalance occurs along gradients in WCA-2A and WCA-1. We are unconvinced that background surface water phosphorus concentrations, the imbalance point, or phosphorus variability have been accurately characterized. We are concerned that existing and near-term technology cannot achieve the proposed criterion of 10 ppb TP, and that the criterion does not accommodate recovery.

Sincerely,

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Executive Director
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Literature Cited

McCormick, P. V. and M. B. O'Dell. 1996. Quantifying periphyton responses to phosphorus in the Florida Everglades: a synoptic-experimental approach. J. N. Am. Benthol. Soc. 15:450-468.

McCormick, P. V., M. B. O'Dell, R. B. E. Shuford III, J. G. Backus and W. C. Kennedy. 2001. Periphyton responses to experimental phosphorus enrichment in a subtropical wetland. Aquatic Botany 71:119-139.

Smith, E. and P. V. McCormick. 2001. Long-term relationship between phosphorus inputs and wetland phosphorus concentrations in a northern Everglades marsh. Environmental Monitoring and Assessment 68:153-176. South Florida Water Management District. 1999. Everglades Interim Report. January.

Posted on the SFWMD WebBoard Friday, October 04, 2002 11:58 AM

In addition to the comments made by the Tribe at the Peer and Review and Public Workshopheld on September 24 and 25, 2002, the Tribe offers these general comments.

The phosphorus data that is presented in the Consolidated Report needs to more accurately depict the true levels of phosphorus that are entering the Everglades Protection Area. Each structure should be listed with the levels of phosphorus actually entering the EPA. More detail is needed than that provided in past Reports in order to discern the true levels of phosphorus and their source. Although the schematic provided in past reports is helpful, it does not give the level of detail needed for a true assessment.

Kelly Brooks

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Posted on the SFWMD WebBoard October 04, 2002 10:51 AM

October 3, 2002

Dr. Garth Redfield, PhD.
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SUBJ: USEPA comments on the August 26, 2002 draft 2003 Everglades Consolidated Report

Dear Dr. Redfield:

The purpose of this letter is to provide the Environmental Protection Agency's (USEPA) comments on portions of the draft 2003 Everglades Consolidated Report (Report). USEPA commends the state in this annual effort. The Consolidated Report provides an excellent summary of where we are in the Everglades restoration process. We offer the following comments for your consideration.

Chapter 2A: Status of Water Quality in the Everglades Protection Area.

Page 2A-8. The existing WCA2A water quality monitoring stations shown in this figure are different than those shown in Figure 5-4 on page 5-25. Please clarify.

Page 2A-28, Paragraph 1. For the first time total phosphorus data reported as less than the MDL are now changed to ½ MDL for data analyses. In addition, our understanding is that recently the SFWMD lab has changed their reported total phosphorus MDL from 4 ppb to 2 ppb. Many previous calculations of baseline conditions in the Everglades (such as those deriving TP requirements for the Refuge and Park in the Everglades Consent Decree) used the MDL, not ½ MDL. It is important that comparisons of new data to historic data be done consistently so as to not introduce artificial bias.

Page 2A-34. The section on sulfate conditions is a welcome addition.

Chapter 2B: Mercury Monitoring, Research and Environmental Assessment

From 1993 to 1999 USEPA Region 4 conducted the South Florida Ecosystem Assessment Project, commonly referred to as the Everglades R-EMAP (Regional Environmental Monitoring and Assessment Program) effort. During 1995-1996 and 1999 about 750 marsh locations were sampled throughout the Everglades Protection Area (EPA), unprecedented in terms of simultaneous spatial coverage and intensity. A major focus of this effort was mercury. The project's three final reports (EPA 904-R-01-001, a four-page flyer, 2001a; EPA 904-R-01-002, a 63-page color summary document, 2001b; and EPA 904-R-01-003, a 400-page technical report with 1200 pages of supporting appendices, 2001c, contained on a CD enclosed within 2001b) were widely distributed in January 2002, after completion of the 2002 Everglades Consolidated Report.

All of these reports are available at http://www.epa.gov/region4/sesd/sesdpub_completed.html. Printed copies are available upon request. One of the unique strengths of the R-EMAP effort is that its broad spatial approach and its data intensity provide an independent source of information as a context for more localized, site-intensive scientific efforts. There are instances in Chapter 2 where additional consideration of R-EMAP results would provide an alternative interpretation to the information presented, or independently buttress what is already stated. Some examples follow.

Page 2B-2 Bullet 3: USEPA R-EMAP data show high methylmercury in surface water in WCA2A, but not high mosquitofish mercury. These data and interpretations are presented in USEPA 2001b. Potential explanations include bottom-up versus top-down controls in eutrophic systems versus oligotrophic systems. This interpretation was framed on the basis of scientific literature and extensive spatial data supported by a statistical analysis technique called structural equation modeling or path analysis. Path analysis estimates the strength of the associations or linkages among different constituents simultaneously, by evaluating the patterns in variability among constituents. Estimating path coefficients provides an indication of the strength of the relationship among variables and can indicate which pathways are statistically significant and whether positive or negative. Examples are presented in the USEPA summary document (USEPA 2001b pages 35-38 and chapter 7 in USEPA 2001c). In addition, a different food web in eutrophic areas may be a factor. Bioaccumulation factors are much higher in the southern Everglades than the north (see USEPA 2001b page 49), resulting in a "hotspot". The R-EMAP reports strengthen the understanding of stressor interactions in this system on an ecosystem scale and compliment the results of other studies performed at specific locations.

Page 2B-9 Paragraph 3: Citation of USEPA 2000 is incorrect. The 1994-1995 mercury loads to the EPA in EAA water discharges is reported in USEPA 1996, USEPA 1998 and USEPA 2001b.

Page 2B-9 footnote 4: change to 1994-1995 and cite USEPA 1996.

Page 2B-11 Next to last paragraph: The statements regarding mercury management are somewhat speculative. The control of local mercury emissions with corresponding reductions since the mid-1990s in Everglades gamefish and wading birds is a success story. [Independent R-EMAP data also indicate lower mosquitofish mercury in 1999 than in 1995-1996 at Southwest WCA3A and Shark Slough (USEPA 2001b)]. However, it is possible that achieving even lower mercury concentrations so that the fish consumption advisories throughout the EPA may be discontinued may require more aggressive local controls or international reductions in the global mercury background.

Page 2B-19 Paragraph 3: "high sulfate levels tend to inhibit production." R-EMAP data indicate that the highest methylmercury in water and soil is found in the northern Everglades (WCA2A) where the highest sulfate, sulfide, phosphorus, and total organic carbon also tend to occur. How high is too high? Sulfide is probably more inhibiting. There are several dynamic interactions occurring. The porewater sulfide spatial footprint is probably an important delineator of where changes occur in this system (see figure 38 in USEPA 2001b).

Page 2B-20 Paragraph 3: Biodilution has not been shown to be a process that controls mercury bioaccumulation in the Everglades.

Page 2B-20 Paragraph 6: Statements that only attribute methylation as occurring in sediment ignore that the process can also occur in the water column and in periphyton mats where anoxia can also occur. Has their been scientific demonstration that methylation occurs and is important only at the sediment-surface water interface? Anoxia can occur throughout the EPA, especially in the early morning hours.

Page 2B-20 Last Sentence: Wouldn't it be more accurate to say "while sulfate is required for microbial methylmercury production, high sulfide levels tend to inhibit production or increase binding"? This was demonstrated in the 1999 R-EMAP data by a large increase in sulfate with marsh drydown, and the oxidation of sulfide to sulfate which can stimulate microbial methylation upon rewetting (figure 6.44 in USEPA 2001c).

Page 2B-21 paragraph 2: "…SRB methylmercury production rate is closely linked to concentration of methylmercury in fish." Again, R-EMAP data indicate that the areas with the highest surface water methylmercury concentrations are not necessarily the areas with the highest mosquitofish mercury. Methylmercury binding by TOC and sulfide may leave less methylmercury available for bioaccumulation.

Page 2B-21 Paragraph 4: The USEPA R-EMAP three sub-area conceptual model of the Everglades ecosystem is not the most recent version. The most recent version (see Figure 45 in USEPA 2001b and its caption) is attached as a separate file. It depicts why a single conceptual model cannot be used to accurately describe mercury bioaccumulation in all areas of the EPA.

Pages 2B-22 Paragraph 2 and 2B-23 Paragraph 3: There are statements regarding a methylmercury maxima in central WCA3A. Clarify that this is referring to fish only. R-EMAP data indicate the highest methylmercury concentrations in mosquitofish are not found in the same areas as the highest methylmercury in surface water.

Page 2B-25 Paragraph 1: The end of this paragraph would be a good place to mention all six R-EMAP reports (USEPA 1996, 1998, 2000, 2001a, 2001b, 2001c) and provide a link to the internet site where all reports, appendices and data can be accessed (see second paragraph above).

Page 2B-29 Biodilution: This is a process that has not been demonstrated to be of great importance in explaining the observations in the Everglades.

Page A-2B-2-1 Paragraph 2: The sulfate gradient throughout the Everglades is documented in several R-EMAP reports: USEPA 1996, 1998, 2000, 2001a, 2001b and 2001c.

CHAPTER 4C ADVANCED TREATMENT TECHNOLOGIES

Pages 4C-45 and 56-57. The results of toxicity testing for various potential advanced treatment technologies are reported here. USEPA Region 4 toxicity testing experts thoroughly reviewed the lab results for these tests in great detail during 2001. Their overall conclusions were that both the toxicity and algal growth potential tests produced inconclusive results, and it is impossible to rule in or rule out any of the technologies based on these results. Reasons included overall study design, lack of repetition at sites, algal biomass inadequate to determine effect of treatment, and limiting nutrients not determined. Several suggestions for improving future evaluations were offered. Therefore, the results as described in the draft chapter are misleading. Specifically, the chronic test on minnows described on page 45 requires 80% survival (both the inflow and the outflow failed). Weight gain in the remaining fish does not necessarily indicate that these surviving fish were healthy. We disagree with the conclusions on pages 56-57 that for CTSS bioassay and algal growth potential studies demonstrated no significant impact on receiving waters. We suggest: mention that the tests were done, do not report the data, state that the tests were inconclusive and why, and state that based on these tests no technologies can be ruled in or out.

Chapter 5: Development of a Numeric Phosphorus Criterion for the Everglades Protection Area

Page 5-11, Paragraph. "During the 1996 through 1999 period of record, the group of five reference sites in the Refuge (WCA-1) exhibited annual geometric mean TP concentrations ranging from 7.2 to 11.8 ug/L, with a median geometric mean concentration of 9.2 ug/L." This statement does not appear to agree with the data in Table 5-2.

Page 5-14. The USEPA 2001 citation for using the 75th percentile method for deriving nutrient criteria is incorrect. The correct citation for the quote is: "USEPA, 2001. November 14, 2001 letter on development and adoption of nutrient criteria into water quality standards from the Director of the Office of Science and Technology to the Directors of all State and Tribal Water Programs, pages 14-15." See http://www.epa.gov/waterscience/criteria/nutrientswqsmemo.pdf. The same statement is found in "USEPA, 2002. Water Quality Criteria, Nutrients, Frequently Asked Questions, Question 5." See http://www.epa.gov/waterscience/criteria/nutrient/faqs.htm. The 75th percentile concept, but not this exact quote, is also found in various national nutrient criterion guidance documents.

Page 5-26 to 5-28. These figures show the locations of existing water quality monitoring network stations, as a starting point for where the numeric total phosphorus criterion would be measured within the water body to assure that the designated use is being met. First, why are the stations shown in these figures different than the stations shown in the existing monitoring network described in Chapter 2? Second, presumably this historic network was not established for the present objective. Third, the number of and location of stations will have to be re-evaluated and modified in order to meet the present objective, particularly for WCA2A. Fourth, will efforts be made to coordinate with the water quality monitoring network that CERP has independently proposed for different objectives?

Once again, we compliment the state on this timely report and we hope that these comments are helpful. Should you have any questions please contact me at (561) 615-5292.

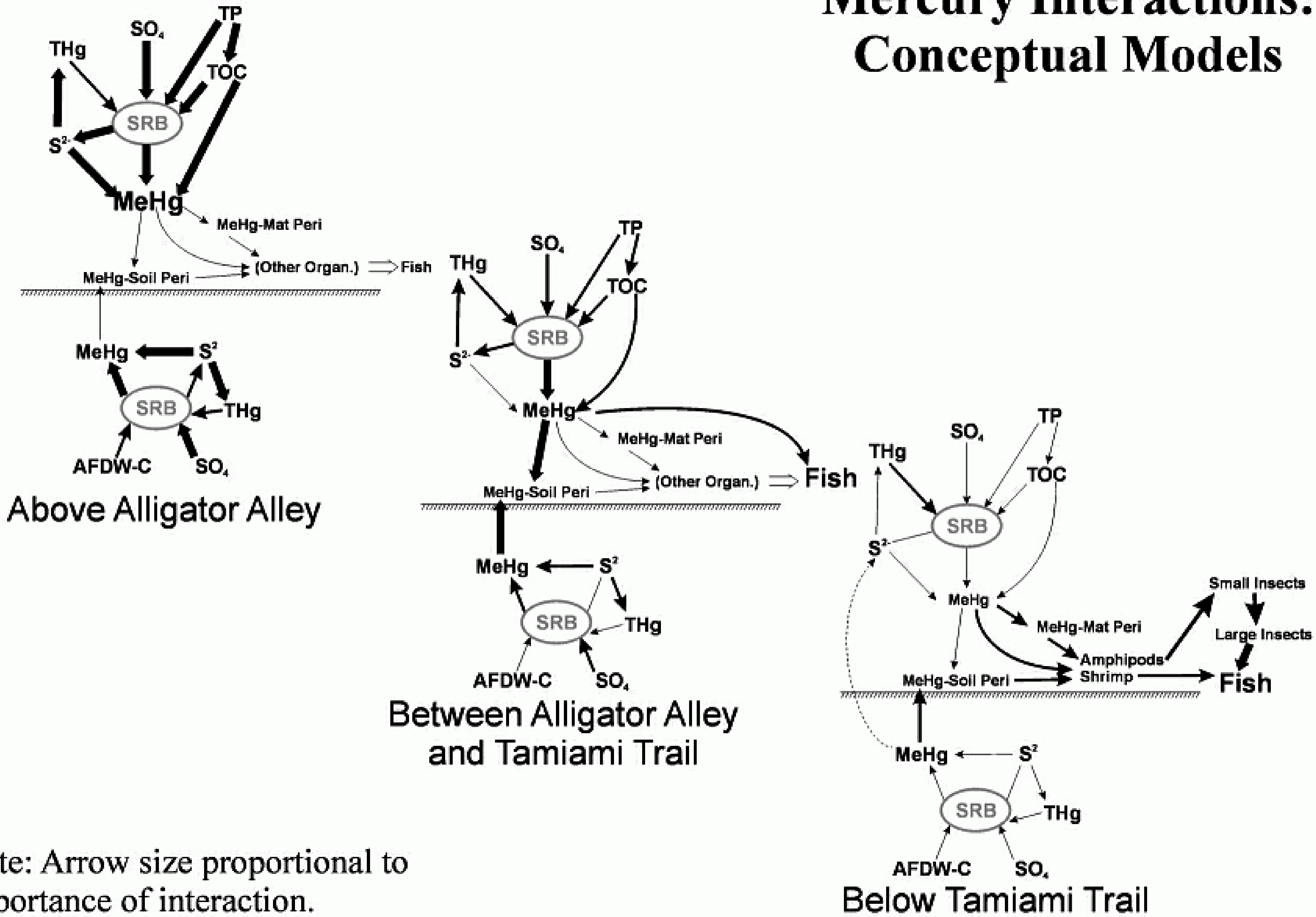
Sincerely,

Original signed by

Richard Harvey, Director
South Florida Office

Figure 45, USEPA, 2001b

Mercury Interactions: Conceptual Models



Note: Arrow size proportional to importance of interaction.

Appendix 1-3: Comments on the 2003 Everglades Consolidated Report from Outside Persons and Organizations

Please note:

The following pages do not reflect the header and footer format of the 2003 Everglades Consolidated Report.

These pages are comments that were received as individual documents and have page numbering and header and footer formats specific to each individual document.

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September 6, 2002

Garth Redfield
Report Editor
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Dear Garth,

I just received an electronic copy of the 2003 *Everglades Consolidated Report*. As you know, I have been working on a number of Everglades issues for the past 13 years; thus I am always interested in the summary of Everglades research. In particular, I am extremely interested in the numeric criteria for establishing a phosphorus threshold for the Everglades. Thus, for the present time I will focus my comments on Chapter 5 of the 2003 report. My main concern with this chapter is the District's continued misinterpretation of the Duke University Wetland Center's dosing phosphorus research. For example, on page 5-2, it is stated, "The adoption of a 10 μg/L P criterion is further supported by both the comprehensive literature review conducted by the USEPA during their evaluation of the Miccosukee Tribe's proposed 10 μg/L criterion and the results of Duke University Wetland Center (DUWC) reanalysis of their studies conducted in WCA-2A." Actually, the Duke University Wetland Center results support a phosphorus criterion of 15.6 μg/L, not 10 as stated in your summary. Moreover, a careful analysis of the USEPA document would find that few, if any, specific scientific publications directly show a phosphorus criterion of 10 μg/L. Thus, it is most alarming that the South Florida Water Management District reports continue to misrepresent information regarding the numeric phosphorus criterion for the Everglades.

On pages 5-16 and 5-17, under the subheading "Other Supporting Information," the district again assesses the DUWC research from the six-year phosphorus dosing experiment. In this assessment, they refer to Figure 5-3, which was reproduced from our September 2001 Phosphorus Criteria Workshop Report. This figure clearly shows that the average changepoint is 15.6 μg/L for all trophic levels; however, the accompanying FDEP text provides a complete misinterpretation of the DUWC weighted average changepoint and Bayesian methodology. The report states, "In other words, by the time the P concentration is increased to 15.6 μg/L, half the biological changes observed had already occurred. Therefore to establish a P criterion that is protective of all the natural biological communities characteristic of the Everglades, the criterion would need to be derived using the lower end of the range or (i.e., 10 ppb or less) [*sic*]." Again, this is a complete misunderstanding and misinterpretation of our analyses and approach, since the Bayesian analysis provides for a probability estimate of the most appropriate changepoint. In addition, our analysis of the 5% (lower credible interval), when compared to the selected changepoint, reveals that the lower interval has an extremely low probability (very near zero). This analysis does not mean that 50% of the population is above or below the selected changepoint. FDEP's misinterpretation is surprising, because we have sent the database and the very files we used for our analysis to the state. Moreover, in early August we provided the state with a paper describing our methodology (S.S. Qian, R.A. King, and C.J. Richardson. *Ecological Modeling*, in press). A draft of this paper was sent to FDEP more than a year ago. Thus, the FDEP's statement that DUWC has not provided the documentation concerning the details of reanalysis is incorrect.

In summary, I can only say that I am extremely disappointed that the South Florida Water Management District and the Florida DEP have continued to misinterpret the Duke University Wetland Center's research findings. We have been conducting P research for over 10 years, and millions of dollars have been spent developing a stringent, scientifically based phosphorus criterion for Everglades protection. Dozens of researchers and graduate students have been working on our analysis. We have continued through the years to cooperate with FDEP and SFWMD whenever asked, and we have presented our materials, methods, and publications. Moreover, we are always available to answer questions about our work and have participated in a large number of peer review workshops and meetings in the past. Thus it is inappropriate for the state to continue to reinterpret and misrepresent our analysis. On several occasions during past meetings, we have had to point out to the FDEP staff that they have totally misused our results or presented incorrect information. For example, in an earlier FDEP report, photographs of the control channel in our dosing study were mistakenly identified as phosphorus treatment channels, while photos of the phosphorus treatment channels were wrongly labeled as the control channels. As published in the report, these photographs would lead the reader to conclude that a certain concentration of phosphorus was having significant effects on the Everglades periphyton mat. In reality, the photographs only showed seasonal effects. Unfortunately, the FDEP staff interpreted our experiment for us and made no attempt to verify the experimental setup or design before using these photographs in their final report. This continuous pattern of misrepresentation of our findings and experimental results–especially in the 2003 consolidated report–is most disconcerting, since I sent a letter to Frank Nearhoof in early August 2002 clearly outlining our position on the 15.6 μg/L TP threshold. Thus I hope that in the near future I will have the opportunity to clearly present, in full, our research findings and the scientific basis for what we believe is the most appropriate phosphorus criterion to save the Everglades.

Sincerely,

Curtis J Richardson

Curtis J. Richardson
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September 21, 2002

Comments on the 2003 Everglades Consolidated Report

Executive Summary

The indisputable fact in the Everglades is that at high phosphorus concentrations you do not have high mercury concentrations in fish. At low aqueous concentrations of phosphorus, concentrations of mercury in fish can be and often are very high. This is not in any way unique to the Everglades but is seen in other waters of the United States, Europe, and Canada. The Consolidated Report states that phosphorus does not influence mercury methylation rates and in some situations this may be the case. However, in phosphorus-limited systems, such as the Everglades, phosphorus does dramatically influence both biological productivity and food chain components (hence the tremendous money and effort being applied to reduce phosphorus concentrations). What the report authors have not made clear is that the amount of mercury in fish is not solely a function of the methylation rate but is strongly influenced by the biological productivity of the water body. As phosphorus concentrations increase, shifts in the food chain and productivity increase the amount of biomass and increase the rate at which settling biomass removes mercury from the water column and sequesters it in the sediment. And, at moderate phosphorus concentrations, similar to that which historically overflowed from Lake Okeechobee (30–60 μg/L), productivity is elevated. It follows, therefore, that the current drive to have a low 10 μg/L phosphorus standard everywhere in the Everglades may have the unintended consequence of raising mercury risks to wading birds in areas where the mercury risks are now low.

The authors have misunderstood the concept of biodilution, which the Cooperative believes explains the observed patterns in fish mercury in the Everglades. Biodilution of mercury is caused by the production of higher biomass levels, and greater particle and detrital settling fluxes in areas with higher nutrient levels, resulting in lower concentrations of mercury in the biota. Nutrients, such as phosphorus, may not affect the rate of production of biologically available mercury (methylmercury), but do affect its uptake in the food web. Experiments conducted by the District and cited in the 2003 Draft Consolidated Report focus on the effect of phosphorus on methylmercury production. These experiments do not show any strong relationships. What is missing from the District research effort are studies that consider the effects of nutrients on methylmercury concentrations in biota. Experiments by other research groups along these lines (e.g., Pickhardt et al. 2002) that clearly demonstrate biodilution in the food web are incorrectly explained.

The mercury problem associated with STA2 is not limited to Cell 1, as stated in the Consolidated Report. In fact, the other two cells in the STA, Cells 2 and 3, are also large producers of methylmercury, with concentrations 5 to 10 times higher in the outflow than the inflow. STA 6 is also a significant producer of methylmercury, whereas the other STAs currently do not seem to have this problem. It is interesting to note, though, that STAs 2 and 6 receive the lowest phosphorus waters from EAA and produce the lowest outflow phosphorus concentrations. With all of the existing data, it is not correct to say that phosphorus concentrations do not influence fish mercury concentrations.

There are several instances in both chapter 2B-5 and Appendix 2B-5 where the descriptions of findings from published literature did not clearly reflect the results that were reported. Specifically:

1. The study of Pickhardt et al. (2002) did show a strong and significant inverse relation between biological productivity and methylmercury concentrations
2. The study by Miles et al. (2001) reported higher methylmercury accumulation in green algae as compared to blue-green algae
3. The lack of biodilution effect seen in the English-Wabagoon River study was the result of a 50-fold increase in mercury methylation rates associated with high phosphorus treatments
4. Babairz et al. (2001) observed higher methylmercury concentrations associated with particulate in the phosphorus-rich regions of the Everglades

as compared to the phosphorus-limited regions, where the majority of the methylmercury is found associated with the dissolved fraction

5. Moye et al. (2002) found higher variability in the uptake rates of methylmercury between species of green algae than were seen between green algae and blue-green algae.

Exponent has reviewed the South Florida Water Management District's (the District) 2003 Everglades Consolidated Report with specific emphasis on Chapter 2B and Appendix 2B-5. General and specific comments are provided on the following sections.

Comments on Chapter 2B: Mercury Monitoring, Research and Environmental Assessment

General Comments

As in past years, the mercury assessment in the 2003 Everglades Consolidated Report has its highs and lows. One of the best features in the 2003 edition is the objective treatment of the perennial debate over the source of mercury (local, regional, or global) that supplies atmospheric deposition (pp. 2B-9 to 2B-19). The authors have gone out of their way to show how the somewhat competing hypotheses agree rather than disagree.

The discussion of aquatic cycling of mercury (pp. 2B-19 to 2B-35), unfortunately, is still highly speculative and contains several factual errors and misinterpretations that we feel have resulted in some erroneous conclusions on the part of the authors. Specifically, it is of concern that the chapter lacks a balanced consideration of the effect of phosphorus on biological productivity and the role of increased productivity on mercury cycling. We feel that this may have resulted in an inadvertent disservice to the public interest and a more balanced approach would be more appropriate. The specific points of our concern are as follows.

The authors misunderstood the concept of biodilution, and its associated processes—higher biomass, and greater particle and detrital settling fluxes—resulting in a serious

oversight in their initial premise. This section starts the discussion of factors influencing mercury concentrations in Everglades fish (p. 2B-20) by presenting the hypothesis that atmospheric deposition is the key variable in mercury cycling within the Everglades. In our study, we find that current data support the assumption that the primary source of inorganic mercury in the Florida Everglades originates from the atmosphere. Biodilution does not conflict with this, but rather is concurrent, and is primarily manifest in environmental processes involving post-methylation distribution and availability. The general process of biodilution is simply that, when comparing eutrophic and oligotrophic situations, methylmercury tends to bioaccumulate to a greater extent under oligotrophic conditions. The mechanisms responsible for this are numerous and interrelated. The primary controlling factors as related to the manifestation of biodilution in the Florida Everglades include the following:

- Reduction in methylmercury concentrations in individuals as the result of increased individual growth rates (also referred to as growth dilution)
- Reduction in methylmercury concentrations in overall biomass as the result of increased population growth rates (also referred to as bloom dilution)
- Reduction in methylmercury bioavailability through sequestration in detritus and other inanimate compartments when such compartments are positively influenced by increased biological productivity.

We feel that the authors of the chapter have lost some perspective in their fervor to examine mechanistic-based hypotheses associated with methylmercury cycling. Clearly, the issue that should be of paramount concern is whether the effort of the Florida Department of Environmental Protection (FDEP) and the District to achieve levels of 10 μg/L of phosphorus in water, within currently nutrient-enriched areas (approximately 6 percent of the northern Everglades), will further threaten native bird populations by increasing methylmercury bioconcentration levels in fish.

The authors of the Chapter appear to have misunderstood the position of the Sugar Cane Growers Cooperative of Florida. The last paragraph on p. 2B-21 states, "As to the contention by the Sugar Cane Growers Cooperative that phosphorus should be used to control mercury methylation rates… .” This is misleading. The Cooperative’s concern is that areas with higher phosphorus loading in the Everglades experience an increase in biomass, and increased settling fluxes of particles and detritus. Methylmercury produced in the ecosystem in higher phosphorus areas can then be taken up by this larger biomass, and also removed from the water column more rapidly by the greater settling fluxes. The net result is a reduction in the methylmercury concentrations observed in fish, as a result of biodilution in the food web and not as a result of any changes to the rate of methyl mercury production. As a result of biodilution that naturally occurs in the phosphorus-enriched areas of the Everglades, wading birds that feed in these areas experience lower risks due to mercury toxicity than birds that feed in oligotrophic areas. It has never been the position of the Sugar Growers Cooperative of Florida that phosphorus should be used to control methylmercury bioconcentration. Our principal concern is that the District and FDEP have failed to recognize that in its projected program to reduce phosphorus inputs to the Everglades, its actions will result in increased mercury bioaccumulation and thus increased potential threat to indigenous wildlife.

Our studies into the mechanisms of methylmercury cycling have consistently shown, and hence it has always been our position, that biodilution (and its associated processes—higher biomass, and greater particle and detrital settling fluxes) caused by phosphorus loading predominantly affect uptake of methylmercury in the food web, and not the *rate* of mercury methylation. Thus, we agree in principle that the primary variables controlling mercury methylation in sediment are:

- Bioavailable inorganic mercury
- Labile organic carbon
- Temperature
- Sulfur species.

The authors’ finding that phosphorus concentration in the water column had no significant control on mercury methylation rates in sediment therefore comes as no surprise. Unfortunately, the studies described on pp. 2B-22 to 2B-24 did not measure or consider methylmercury partition in the water column and therefore cannot be considered to have even addressed "the Cooperative's hypothesis," much less refute it.

The District appears to have misinterpreted the nature of the relationship between phosphorus in water and mercury in fish. The U.S. Environmental Protection Agency's (EPA) data collection program in the Everglades and our interpretation of the phosphorus in water and mercury in fish results are described on pp. 2B-24 to 2B-26, and the data are shown in Figure 2B-10. Exponent presented a hypothesis in 1998 that there existed an inverse relationship between total phosphorus concentrations in surface water and methylmercury concentrations in fish. Since that time, as more and more data has been collected, we have seen this relationship become less significant. But most interestingly, this loss in statistical power has been the result not of increased variability at higher phosphorus concentrations, but *almost exclusively as the result of increased variability in the observation at low phosphorus concentrations* . The reason for this is demonstrated in the experimental results of Sundborn and Meili (1996) that are presented in Figure 1. Biodilution is only manifest when phosphorus concentrations increase to a point where they can sustain increased biological productivity. Sundborn and Meili (1996) did not see exponential algal growth nor increases in the fraction of methylmercury associated with the algae until phosphorus concentrations exceeded 10 μg/L. Below this concentration, there is no reason to believe that phosphorus will have any affect on methylmercury bioconcentration.

The relationship between phosphorus and fish mercury and uneven variance distribution is perhaps more clearly demonstrated in the chapter Figure 2B-10a, as well as in Figure 2 presented here that is specific to WCA-2A. At higher phosphorus concentrations, high mercury concentrations are rarely observed in fish because the phosphorus has produced enough biomass through primary production to result in a lower concentration of mercury in biota which are part of the base of the aquatic food web in the Everglades. At lower phosphorus concentrations (also shown in Figure 2B-10b), small changes in total phosphorus concentrations have no effect on biological production rates and hence there is no relationship between phosphorus and mercury. The problem is further complicated by seasonal variations within the Everglades. During the winter months, reduced flow rates increase phosphorus concentrations at a time when lower temperatures are slowing the biological productivity of the region. These seasonal differences, which were not accounted for in the authors' analysis, also add to the variability that is independent of biodilution. Hence, the authors derived weak regression relationships not because of a lack of manifest biodilution, but because the analysis that was presented was based on the mistaken premise that biodilution is a continuous function regardless of actual phosphorus concentrations or seasonal implications.

The authors of the chapter have misunderstood the findings of Pickhardt et al. (2002) in their discussion of biodilution. Earlier this year, Pickhardt et al. (2002) published the findings of an investigation into the relationship between aquatic productivity and methylmercury bioconcentration (see Attachment A). This paper was reviewed beginning on p. 2B-29, where the chapter authors described the primary hypothesis tested by Pickhardt et al. (2002) as follows:

> We experimentally tested the hypothesis that at equal initial concentrations of aqueous Hg, an increase in algae will result in a decrease in Hg uptake by zooplankton grazers. Our rationale for this hypothesis was that the concentration of metal per cell would be lower in dense algal blooms (hereafter, bloom dilution[1]) because the same amount of metal would be distributed among a greater number of algal cells. (Pickhardt et al. 2002)

Unfortunately, the appendix author appears to have missed an important concept purported by Pickhardt et al. (2002) that is very important with regard to the design and interpretation of the study's results and that is the definition of growth dilution. Pickhardt et al. defines growth dilution as follows:

> A related but different phenomenon, *growth biodilution* of trace metals, is observed in rapidly growing phytoplankton, whereby biomass-specific concentrations of metal diminish as cells divide.

We feel that this is a critical point of comment because the authors of the chapter have presented the study in such a way that the distinction between the two manifestations of biodilution could

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[1] *Bloom dilution* is one of the effective components of *biodilution as seen in the Florida Everglades*.

be easily confused by the reader. Hence, we recommend that the authors clarify the description of the included quote to indicate that it pertains only to the growth dilution in zooplankton[2]. The overall manifestation of biodilution was significant in both phytoplankton and zooplankton (see Figure 3). Furthermore, the range of phosphorus tested in this study (7−44 μg/L) is directly comparable to the ranges found in the northern portions of WCA-2A (Figure 2). Therefore, to balance the presentation of this study and to clearly represent to the reader the results relative to bloom dilution in phytoplankton and zooplankton found in Pickhardt et al. (2002), the appendix author should quote his findings relative to bloom dilution as follows:

> Our second major finding was that as hypothesized, bloom dilution of Hg in algae initiated different mercury uptake dynamics in the zooplankton under high- vs. low-nutrient enrichment. Specifically, methylmercury concentrations were consistently and significantly lower in Daphnia from the high nutrient, high initial algal biomass tanks compared with Daphnia from the low nutrient, and low initial algal biomass tanks at 2 and 3 weeks after zooplankton addition (Fig. 3A and C). Correspondingly, low algal abundances resulted in a 2−3 fold increase in the accumulation of CH_3Hg^+ in Daphnia from low-nutrient tanks (Fig. 3A and C). From these results, we infer that the concentration of CH_3Hg^+ in Daphnia across treatments was related to the concentration of CH_3Hg^+ (Fig. 2C) in the algal cells they ingested, which was in turn affected by algal biomass; e.g., that bloom dilution drives a diminution of metal in the zooplankton. We also observed similar results for effects of bloom dilution on calanoid and cyclopoid copepods (P.C.C., unpublished data). This result has important implications for trophic transfer of toxic CH_3Hg^+ to fish in oligotrophic lakes.

The inclusion of this quote should greatly clarify the results of the study. The author may wish to further clarify his description by including the final conclusion of Pickhardt et al. (2002), which was as follows:

> We conclude that CH_3Hg^+ transferred to grazing zooplankton, and eventually to fish and other vertebrates, will be influenced by nutrient pulses and algal blooms. More specifically, algae effectively and rapidly concentrate both inorganic and organic Hg, but the metal burden per cell decreases in algal blooms. Bloom

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[2] Growth dilution is a phenomenon where new tissue is produced at a rate faster than methylmercury can adsorb to the organism. Because the experiments occurred over a time span of 2 to 3 weeks (methylmercury adsorption can reach equilibrium within 24 hours), the time scale of the experiment was too long to effectively test the occurrence of *growth biodilution*. So it is not surprising that the Pickhardt et al. (2002) study concluded that the evidence produced a "marginally significant trend" with reference to growth biodilution.

> dilution of CH_3Hg^+ in algae results in a substantial reduction of CH_3Hg^+ uptake by cladocerans in high nutrient, high algae conditions. Conversely, cladocerans feeding within low nutrient, low algae treatments accumulate more CH_3Hg^+.

The analyses of methylmercury and phosphorus relationships in STA-2 are seriously flawed (pp. 2B-27 through 2B-29). The statement at the end of the first paragraph of this section provides an illustration of these flaws. The sentence states that "...Figure 2B-11 indicates that STAs can be managed to minimize methylmercury production." However, it is clear from the figure that two of the four STAs cited have higher concentrations of methylmercury in their outflow than in their inflow. In other words, rather than demonstrating that STAs minimize methylmercury production, the figure shows that half of the STAs in operation are net sources of methylmercury. The author has simply ignored what the data indicate and instead made a declaration that the data do not support. It should also be noted that in choosing to display the data using an expanded scale and in relation to the Florida water quality standard for total mercury, the author has obscured the issue, which is that the average outflow concentrations of methylmercury, the toxic form of mercury that is bioaccumulated, are at least double the average inflow concentrations in STA-6 and greater than five times the average inflow concentrations for all cells in STA-2.

One of the characteristics shared by STA-6 and STA-2 is that the inlet and outlet phosphorus concentrations are lower than for the other STAs in operation. The inlet and outlet phosphorus concentrations for STA-6 are 77 μg/L and 30 μg/L, respectively. Phosphorus concentrations entering STA-2 are about 70 μg/L and in the range of 15–25 μg/L in the outflow. It is interesting to note that the two STAs that are clearly producing methylmercury are the STAs with the lower inlet and outlet phosphorus concentrations.

With regard to statements made specifically relating to STA-2, the most significant flaw in the analysis is the repeated contention that Cells 2 and 3 "...have no mercury problem." Figure 2B-12 clearly shows that, on average, the methylmercury concentration in the outlet of Cell 3 is five times higher than the inlet concentration. For Cell 2, the average outlet methylmercury concentration is nearly 10 times higher than the inlet concentration. It is very misleading to state that engineered systems that generate a neurotoxin at concentrations significantly above those found in the inlet have no mercury problem. Differences in the mercury behavior between Cell 1 and Cells 2 and 3 may have a variety of causes, but all three cells clearly have a mercury problem.

In summary, the review of the STA mercury performance data shows that the high concentrations of methylmercury in the outflows from STA-2 and STA-6 are a matter of concern. That the highest concentrations of methylmercury appear in STAs with the lowest inflow and outflow phosphorus concentrations strongly suggest a link between phosphorus and mercury that should be a priority item of research.

Specific Comments

1. p. 2B-5, paragraph 2: This comment applies to the overall focus of the chapter—methylation is important, but it is not the whole story. Transport processes are important, as well as uptake, distribution, and accumulation. By consistently focusing solely on methylmercury production, a skewed picture of mercury dynamics in the system is produced.

2. p. 2B-6, first bullet: The removal of methylmercury in the STAs has not been demonstrated. Two of the four operating STAs are actively producing methylmercury.

3. p. 2B-6, second bullet: There has been no demonstration of methylation of mercury associated with runoff. Considering the solids content of EAA runoff, most of the mercury leaving the EAA is likely associated with particles and is not available for rapid methylation.

4. p. 2B-6, fourth bullet: Where is the documentation of the enhanced transport? This appears to be a hypothesis stated as fact.

5. p. 2B-6, fifth bullet: The statement that sorption to settling organic particles can serve as a significant removal pathway is likely the key to the argument that high phosphorus concentrations can indirectly lead to lower mercury concentrations in biota. Higher

phosphorus leads to greater productivity and thus enhanced removal of mercury from the water column by settling detritus.

6. p. 2B-9, paragraph 2: What about the National Acid Precipitation Assessment Program? This was a $100-million program looking at the impact of air pollutants on surface water quality.

7. p. 2B-19, last paragraph: Gawlik and Crozier (2002) say that water depth is the most important factor impacting wading bird populations, not cattails. Furthermore, a recent study by Crozier and Gawlik (2002), who are associated with the District, reported significantly higher wading bird populations in the phosphorus-enriched areas compared to those that are phosphorus-limited.

8. p. 2B-20, last section: See comment 6 above.

9. p. 2B-21, first full sentence: Hypothesis stated as fact.

10. p. 2B-21, last paragraph and all of p. 2B-22: The contention has never been that methylation or the MeHg production rate was influenced by phosphorus concentrations, only the subsequent distribution of MeHg.

11. p. 2B-23, all: The results of these experiments don't argue against biodilution. The experiments were not correctly designed. They should have measured methylmercury in surface waters and in biota, and not just methylmercury in surface sediments.

12. p. 2B-24, first paragraph: All of the things mentioned may impact MeHg production, but that misses the point of the biodilution/enhanced productivity argument. The key is distribution, not production.

13. p. 2B-30, bottom: "…in the Everglades, phosphorus additions could affect sulfur dynamics and thus might either increase or decrease the production rate of methylmercury by sulfate reducing bacteria through indirect effects."

    Best current evidence suggests that phosphorus has no effect on the rates of mercury methylation in Everglades sediments (Gilmour et al. 2002). We recommend that the authors of the chapter refer to Gilmour et al. (1998, 2002). An illustration of the findings of Gilmour et al. (1998) is provided in Figure 4.

14. p. 2B-31: "A second factor affecting methylmercury bioaccumulation is water chemistry…"

    This hypothetical relationship has been demonstrated not to occur in the Everglades. We feel that it would be appropriate for the authors of this chapter to cite the findings of Babiarz et al. (2001), who reported that at WCA-2A U3, a phosphorus-poor region of WCA-2A, the corrected proportional partition of methylmercury in the water column was 15 percent absorbed to particulate, and 85 percent bound to TOC, whereas in the phosphorus-rich region associated with WCA-2A, F3 was observed to have a corrected proportional partition of methylmercury in the water column of 77 percent absorbed to particulate, and only 23 percent bound to TOC (Table 1). This information could go a long way in explaining fish mercury levels in different locations in the Everglades.

15. p. 2B-31 and 32: The District regards the relationship between phosphorus in water and mercury in fish as a continuous function that can be evaluated by regression analysis to yield a predictive equation. This is not possible because the relationship is not continuous in the Everglades. Phosphorus, through maintenance of greater primary productivity and biodilution, serves to limit the amount mercury bioaccumulation in fishes. However, this effect exists only where there is sufficient biomass for biodilution to have a controlling influence. At lower phosphorus concentrations, the available mass is small compared to the available dissolved methylmercury, and other factors control biodilution.

Preliminary Comments on Appendix 2B-5: Evaluation of the Effect of Surface Water, Pore Water, and Sediment Quality on the Everglades Mercury Cycle

General Comments

Conceptual Model of Mercury Cycling in the Everglades

The author of the appendix provides a lengthy discussion on his understanding and hypotheses related to mercury cycling in the Florida Everglades. While we understand the need to develop such a conceptual model, we are cautious because reasoned argument can often be mistaken for factual certainty without the necessary scientific testing, including analysis of existing data. Therefore, we feel it would better serve the public if such discussions were presented not as a monologue, but rather as experimental designs where the author would propose a hypothetical premise in the context of currently available information and then include a discussion as to how such a hypothesis could be properly tested. Furthermore, we feel that the current conceptual model as presented in the appendix contains some flaws that need to be addressed. Alternative hypotheses for scientific testing by the District must also be presented. In the following subsections, Exponent outlines its concerns specific to the methylmercury bioaccumulation discussion.

Factors Influencing MeHg Bioaccumulation: DOC—The main supporting premise for this discussion was an observation from Miles et al. (2001) that was presented in the appendix as follows: “Miles et al. (2001) observed an inverse relationship between the concentration of DOC and Freundlich isotherm coefficients for methylmercury sorbed to algae in the linear concentration region.” This observation was not presented in Miles et al. (2001). To the contrary, Miles et al. reported the following: “Assuming that these exudates bind MeHg like humics and using MeHg constants determined by Hintemann [1997] in MINEQL, these levels of DOC will not result in a significant fraction of the MeHg-DOC species.” If this was a mistaken reference, then it should be corrected. Otherwise, we feel the description should be changed to reflect the conclusions of the referenced study.

The author of the appendix hypothesizes that DOC may compete with particulates for available methylmercury. While there are some logical reasons for this position, actual measurements from WCA-2A do not support this hypothesis. For example, Babiarz et al. (2001), cited earlier in the appendix, measured the partition of methylmercury between the particulate, colloidal, and dissolved fractions at station F1 (high phosphorus) and U3 (low phosphorus). Their published findings indicate that at F1, 68 percent of the methylmercury in the water column is associated with particulate, 6.6 percent is associated with colloids, and 13 percent is found in the dissolved fraction. However, at U3, Babiarz et al. (2001) found only 17.2 percent of the methylmercury associated with the particulate, 36.8 percent associated with the colloid, and 60.3 percent associated with the dissolved fraction. Hence, the hypothesis proposed in the appendix that increased DOC would reduce methylmercury partition into the particulate fraction is refuted and should be so amended in the final report.

Factors Influencing MeHg Bioaccumulation: TP—In the establishment of the conceptual model, the authors stated that "Miles et al. (2001) observed an inverse relationship between the concentration of phosphorus and Freundlich isotherm coefficients for MeHg sorbed to algae undergoing exponential growth rates in the linear concentration region of methylmercury sorption." This is not an accurate representation of the findings. Reference to the table on p. A-2B-5-66 of the appendix will demonstrate that Miles et al. (2001) actually reported a direct relation between phosphorus concentrations and the Freundlich constant for green algae in exponential growth.

In the last paragraph of p. A-2B-5-15, the author of the appendix hypothesizes that the indirect effects of increased phosphorus concentrations in the water column on DO and DOC may be responsible for reduced methylmercury bioaccumulation in the aquatic food web. This is the fourth annual report to purport this hypothesis (SFWMD 1999, 2000, 2001, 2002) with no evidence, either inferred from published studies or observed in the Florida Everglades, that such a mechanism is present. Exponent feels that after such a long period of consideration, the authors of the appendix should perhaps propose and include in the appendix methods for finally testing this hypothesis.

Exploratory Data Analysis of Water Quality vs. Fish THg: WCA-2A Nutrient Gradient

The analysis presented in this section, while academically interesting, is not indicative in the absence of a testable hypothesis. We fear that there are several places in this section where the application of statistical analysis has potentially led to the development and statement of conclusions that, as stated in the introduction of the section, do not address any specific hypothesis. Examples where such cases occur are as follows:

The Application of Pearson's Correlation Coefficient—Pearson's Correlation is, by definition, the square root of the regression correlation (r^2). A comparative table is provided in Table 7 as evidence of poor relations between mercury concentrations in fish and various water quality parameters. Inherent in the application of comparative Pearson's Correlation are three statistical assumptions:

1. **A consistent linear (or in this case, Log-linear) relationship:** Pearson's Correlation is indicative of the goodness of fit that a database has to a regression line. There is no hypothesis being tested so there is no basis for the assumption that any relationship should be either linear or Log-linear. Examination of the data provided in Figure 16 shows no reason to assume a linear or Ln-linear relation. Therefore, the lack of a fit should be of no surprise.

2. **Normal distribution of error about the regression line:** Although the residuals were not provided in the analysis, examination of Figure 16 strongly indicates that the distribution is not consistent over the entire range of the data set. This suggests that the regression is not consistent. We recommend that the final version include a constant variance test for all parameters compared in Table 7.

3. **Equal variance among the various factors under comparison:** The variance of the regression is sensitive to the number of observations used in the analyses. The use of a Pearson's Correlation matrix, as applied in this case, is only meaningful with equal numbers of observations. Data provided

by the District (Fink 2002, pers. comm.), which were purportedly used to develop these analyses, varied in the number of observations from n=50 for pH to n=486 for sodium and magnesium. Therefore, in order to make the table consistent, the author of the appendix should include tests of regression significance ($r \neq 0$) for the parameters compared in Table 7.

Field Mesocosm Studies of the Effects of P Addition on MeHg Bioaccumulation

In the review of the three papers presented in this section, it is felt that the appendix author could have provided additional explanations such that certain important details would not be overlooked by the reader. The suggestions are as follows:

The English-Wabigoon Mesocosm Study—The appendix author should point out that Rudd and Turner (1983) observed a “50-fold” increase in mercury methylation rates in their high phosphorus treatments and attributed the lack of an observed biodilution effect to this fact. This is a significant fact in the context of the second study reviewed in this section of the appendix that reported “…experiments in which phosphate was added to sediment cores suggested no direct effect of phosphate on net methylation (Gilmour et al. 2000[3])” and also the results of Gilmour et al. (1998), who observed no significant difference in methylmercury rates across the WCA-2A phosphorus gradient (Figure 4).

Mesocosm Dosing Study of P vs MeHg production and Bioaccumulation: ACME II—In the report, WCA-2A U3 is described as “moderately enriched”. We do not understand this qualification because, based on the transect data provided by the District (Fink 2002, pers. comm.), the average phosphate concentration at U3 is only 9.5 μg/L and the average total phosphorus concentrations as reported by the District (SFWMD 1999) is only 7.22 μg/L. Therefore, it would appear that U3 would be better characterized as background. We recommend that that the appendix author’s position would be better presented if the argument was predicated on comparisons of results from F1 verses U3 or F1 verses WCA-3A 15.

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3 Reference for Gilmour et al. (2000) was not present in the reference section.

Dartmouth Mesocosm Study—It is felt, that in order to properly represent the work of Pickhardt et al. (2002), the appendix author should include in his description the concluding paragraph of Pickhardt et al. (2002), which reads as follows:

> We conclude that CH_3Hg^+ transferred to grazing zooplankton, and eventually to fish and other vertebrates, will be influenced by nutrient pulses and algal blooms. More specifically, algae effectively and rapidly concentrate both inorganic and organic Hg, but the metal burden per cell decreases in algal blooms. Bloom dilution of CH_3Hg^+ in algae results in a substantial reduction of CH_3Hg^+ uptake by cladocerans in high nutrient, high algae conditions. Conversely, cladocerans feeding within low nutrient, low algae treatments accumulate more CH_3Hg^+.

Laboratory Studies of the Effect of P Addition on MeHg Bioaccumulation

The author of the appendix presents a brief description of two studies related to methylmercury uptake by algae. The first study by Miles et al. (2001) examined the equilibrium partition coefficients between methylmercury in water and algae. The second by Moye et al. (2002) examined differential rates of methylmercury uptake by different algal species.

Miles et al. (2001)—The study determined the Freundlich partition coefficient (Kp), which is the ratio between methylmercury concentrations in the algae over the methylmercury concentrations in the water. Hence, the higher the Kp, the greater the degree that the algae will bioaccumulate methylmercury. The appendix author makes two regrettable errors in his interpretation of the study results. First, he mistakenly concludes the findings of Miles et al. (2001):

> … the researchers evaluated the effects of phosphorus stimulation of MeHg uptake by *Selenastrum* and concluded that the Kp value is generally lower when measured in exponential (log) growth phase sustained by high TP concentrations than in P-limited, static growth phase…

This conclusion was not reported anywhere in the text of the study. On the contrary, Miles et al. (2001) concluded:

> In addition, partitioning coefficients determined with exponential and stationary phase cells at the same condition were not significantly different, while the

> partitioning constant for exponential phase, phosphorus-limited cells was significantly lower.

In effect, Miles et al. (2001) report that a) there was no difference in Kp between exponential or static cells under P-limited conditions or between exponential or static cells under phosphorus stimulated conditions, and b) that the Kp was *significantly higher* when measured in exponential growth phase sustained by high TP concentrations than in P-limited, exponential growth phase. We suggest that the appendix author include the above quotation and consider modifying the discussion to reflect the results.

The second error pertains to the statement in the appendix that "…high P also causes structural changes in the cell that reduce MeHg uptake." Again, we fear that the appendix author has misinterpreted the results and conclusions reported by Miles et al. (2001). In the published text of the study, the author reported: "Another observation to this issue is the decrease in the MeHg partition constant with phosphorus limitation in *Selenastrum*." The author hypothesizes that this difference may be the result of structural changes to the cells. To clarify this finding, we recommend that the author revise his discussion to reflect the fact that "high P causes structural changes in the cell that *increase* MeHg uptake," rather than the reverse as is currently reported.

Moye et al. (2002)—In reviewing the representation of the results of Moye et al. (2002), we find that Appendix 2B-5 may be misleading and that clarification is necessary in order to better inform the reader of the study's results. Specifically, the appendix states as follows:

> …the authors concluded that the uptake rate by the blue-green alga, Schizothrix calcicola, which predominates in the low P concentration ranges of the Everglades, takes up MeHg at a rate one-twentieth that of the green algae species tested.

The study reported an uptake rate of 21.3 nmol/g-h (nmols of methylmercury per gram algae per hour) for the blue-green species *Schizothrix calcicola*. While this uptake rate was lower compared to the green algae *Cosmarium botrytis* (45.8–49.2 nmol/g-h during exponential growth; 242–911 nmol/g-h in stationary phase), it was significantly higher than the methylmercury uptake rate reported for green algae species *Selenastrum capricornutum* (5.28 nmol/g-h in stationary phase). Therefore, the generalization that the blue-green algal species possesses a lower methylmercury uptake rate compared to all the green algal species is not exactly reflective of the study's results. In order to clarify this position, we would recommend that the appendix author reproduce Table 1 from Moye et al. (2002) as was done for Miles et al (2001).

Implications for the Florida Everglades—The algal communities in the nutrient-poor portions of the Florida Everglades are dominated by cyanobacteria (blue-green algae), particularly *Schizothrix calicola* and *Scytonema hofmanni*. Under phosphorus-limiting conditions, these blue-green algae surround themselves with an adhesive lipopolysaccharide layer that permits the formation of the calciferous algal mats predominant in these regions (Swift 1984). In the northern regions of the Everglades where surface water inputs bring available phosphorus, the biomass of the blue-green algae in the water is higher than that found in the southern oligotrophic regions. However, under these nutrient conditions the blue-green algae do not produce the lipopolysaccharide layer and therefore the calciferous mats do not form (Hall and Rice 1990). Other changes seen in the algal community in the northern, high-phosphorus regions of the Everglades include a predominance of green filamentous algae including *Oedogonium sp.* and *Ulothrix sp.*, whose growth is severely limited under low-phosphorus conditions. Quantitative analysis of the algal communities between these two areas show an increase in the concentration of biomass in the high phosphorus region, a shift in the predominant species from green algae in the high-phosphorus regions to blue-green algae under low-phosphorus conditions, but no significant difference in level of algal diversity between the two areas (Rader and Richardson 1992).

The actual observations reported in Miles et al. (2001) indicate that green algae will bioaccumulate methylmercury to a greater extent when grown under phosphorus-sufficient conditions compared to the same species grown under phosphorus-limiting conditions. Furthermore, green algae will bioconcentrate methylmercury to a greater extent than will blue-green-algae. Hence, with uniform concentrations of methylmercury in the water of the Everglades, it would be expected that higher methylmercury bioconcentration rates would be found in the northern high-phosphorus regions as compared to the southern low-phosphorus regions. This, however, is not the case. Analysis of the District data indicates that the concentrations of mercury in fish are lower in the high phosphorus regions (Figure 2). This contradiction suggests that other factors, such as overall increase in biomass as the result of an increase in phosphorus availability, are overwhelming this effect of nutrient-specific differences in bioconcentration, such that the field conditions run opposite to what would be predicted from the laboratory results.

Mechanistic Modeling Analysis of the Biodilution Phenomenon

Exponent feels that it is perhaps not appropriate for the author of the appendix to include this section in the report. We are concerned that the application of a mathematical simulation as evidence for an event places the authors in danger of taking highly erroneous positions. We feel this is particularly true in this situation for the following reasons: 1) the E-MCM model is not available for review by the public and therefore the purported results cannot be verified independently, and 2) the E-MCM model, to our knowledge, has never been validated for any application in the Everglades.

We recommend that the E-MCM model not be used in a regulatory context as a predictive tool until it is completed and publicly available for review.

Analyses Not Submitted with the Draft

The draft indicates that the District intends to include univariate and multivariate regression analyses in the final version of Appendix A-2B-95 (Attachments 1 and 2; noted as not supplied with this review draft). This unfortunate circumstance has occurred before and has resulted in portions of the document being finalized without the required public review being fulfilled. We suggest that if such analyses cannot be made available for proper public review and comment, then perhaps it would be more appropriate to reserve the analyses for inclusion in the 2004 Annual Report.

References

Babiarz, C.L., J.P. Hurley, S.R. Hoffman, A.W. Andren, M.M. Shafer, and D.E. Armstrong. 2001. Partitioning of total mercury and methylmercury to the colloidal phase in freshwaters. Environ. Sci. Technol. 35:4773–4782.

Crozier, G.E., and D.E. Gawlik. 2002. Avian response to nutrient enrichment in an oligotrophic wetland, the Florida Everglades. Condor 104:631–642.

Fink, L. 2002. Personal communication (e-mail to G. Bigham, Exponent, Bellevue, WA, dated September 6, 2002, regarding data sets for correlations and regressions). South Florida Water Management District, West Palm Beach, FL.

Gawlik, D.E., and G.E. Crozier. 2002. Factors affecting wading bird populations in the Everglades. Testimony to Environmental Regulation Commission Hearing, August 28, 2002. West Palm Beach, FL.

Gilmour, C.C., G.S. Riedel, M.C. Ederington, J.T. Bell, J.M. Benoit, G.A. Gill, and M.C. Stordal. 1998. Methylmercury concentrations and production rates across a trophic gradient in the northern Everglades. Biogeochemistry 40:327–345.

Gilmour, C., D. Krabbenhoft, W. Orem, and G. Aiken. 2002. ACME dry/rewet study, MeHg production in Everglades soils during drying and rewetting cycles. Winter/spring 2002: Comparison of soils from STA2 Cell 3 and WCA 3 Site 15. Preliminary Data Report.

Hall, G.B., and R.G. Rice. 1990. Response of the Everglades marsh to increased nitrogen and phosphorus loading. Part III: Periphyton community dynamics. Everglades National Park, Homestead, FL.

Miles, C.J., H.A. Moye, E.J. Phlips, and B. Sargent. 2001. Partitioning of monomethylmercury between freshwater algae and water. Environ. Sci. Technol. 4277–4282.

Moye, H.A., C.J. Miles, E.J. Phlips, B. Sargent, and K.K. Merritt. 2002. Kinetics and uptake mechanisms for monomethylmercury between freshwater algae and water. Environ. Sci. Technol. 36:3550–3555.

Pickhardt, P.C., C.L. Folt, C.Y. Chen, B. Klaue, and J.D. Blum. 2002. Algal blooms reduce the uptake of toxic methylmercury in freshwater food webs. Proc. Natl. Acad. Sci. 99:4419–4423.

Rader, R.B., and C.J. Richardson. 1992. The effects of nutrient enrichment on algae and macroinvertebrates in the Everglades: A review. Wetlands 12(2):121–135.

Rudd, J.W.M., and M.A. Turner. 1983. The English-Wabigoon River system: V. Mercury and selenium bioaccumulation as a function of aquatic primary productivity. Can. J. Fish. Aquat. Sci. 40:2251–2259.

SFWMD. 1999. Everglades interim report. South Florida Water Management District, West Palm Beach, FL.

SFWMD. 2000. Everglades consolidated report. South Florida Water Management District, West Palm Beach, FL.

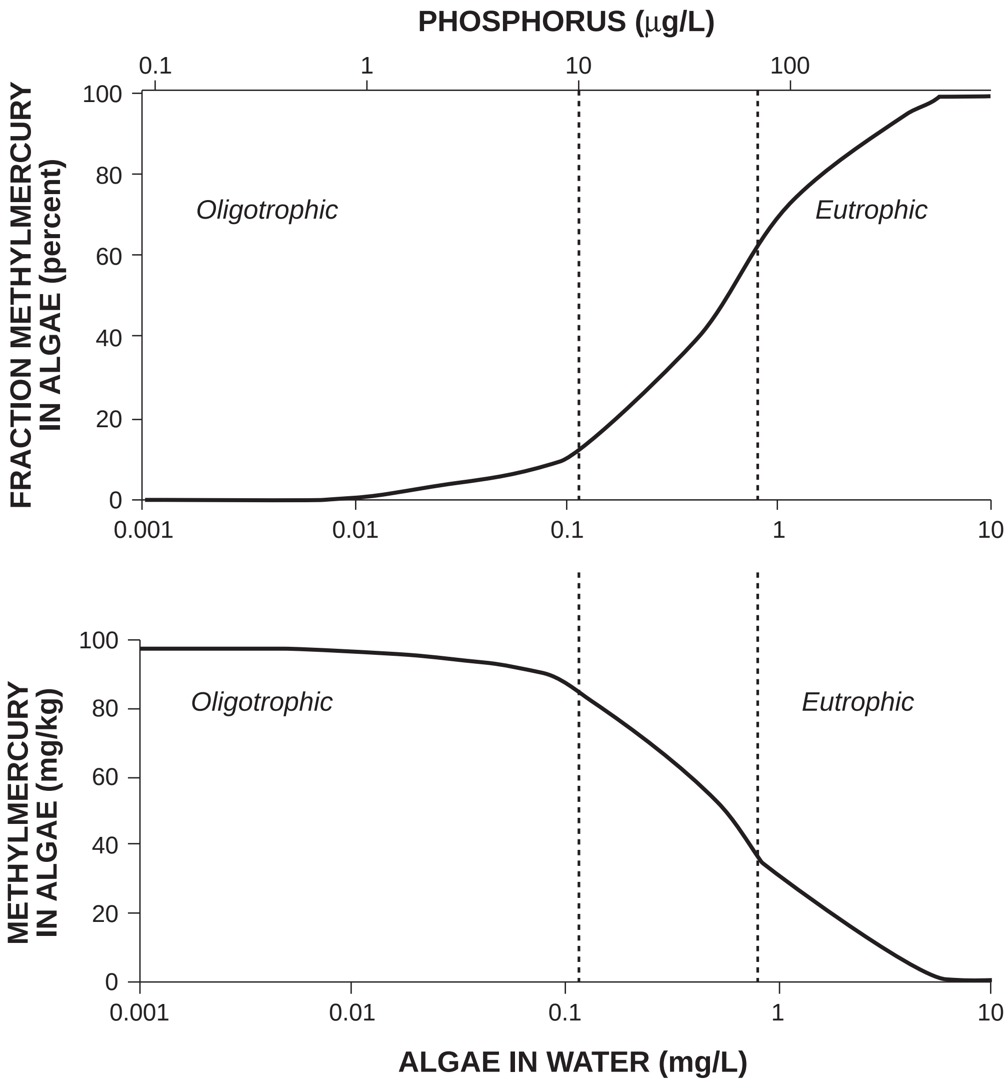
SFWMD. 2001. Everglades consolidated report. South Florida Water Management District, West Palm Beach, FL.

SFWMD. 2002. Everglades consolidated report. South Florida Water Management District, West Palm Beach, FL.

Sundborn, M., and M. Meili. 1996. Bioaccumulation of methylmercury by phytoplankton and zooplankton along experimental gradients of lake eutrophication, acidification and liming. p. 405. In: Fourth International Conference on Mercury as a Global Pollutant, Book of Abstracts. August 4−8, 1996. R. Ebinghaus, G. Petersen, and U. von Tumpling (eds). GKSS-Forschungszentrum Geesthacht, Germany, and Oak Ridge National Library, USA.

Swift, D.R. 1984. Periphyton and water quality relationships in Everglades Water Conservation areas. p. 97−117. In: Environments of South Florida, Present and Past. P.J. Gleason (ed). Miami Geological Society, Miami, FL.

Figures



Note:
Algae = *Scenedesmus quadrecauda*
C_0 = 100 ng/L CH_3 Hg

Figure 1. Methylmercury biodilution mechanism as observed by Sundborn and Meili (1996)

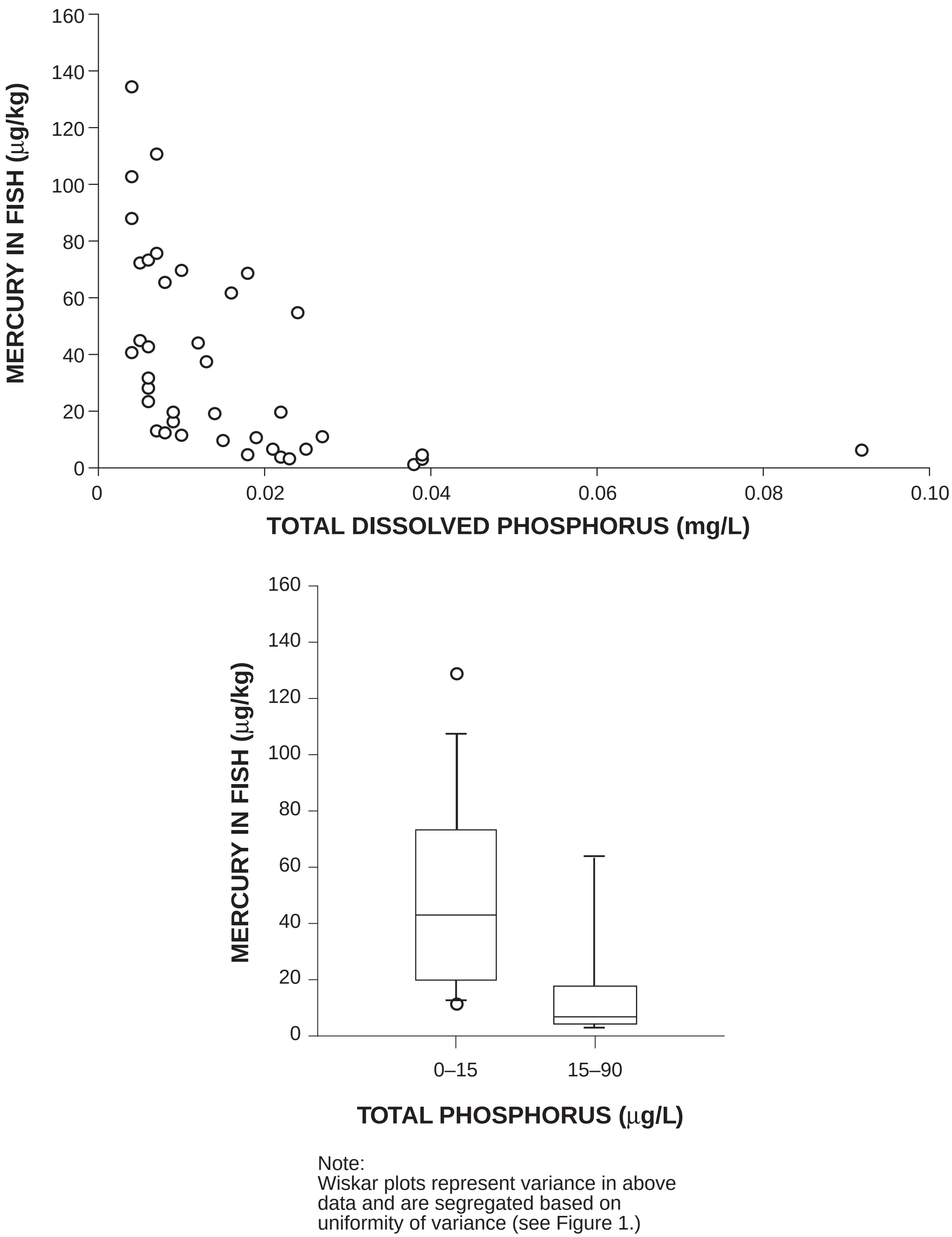


Figure 2. Fish methylmercury content in Gambusia along the phosphorus gradient based on SFWMD transect data

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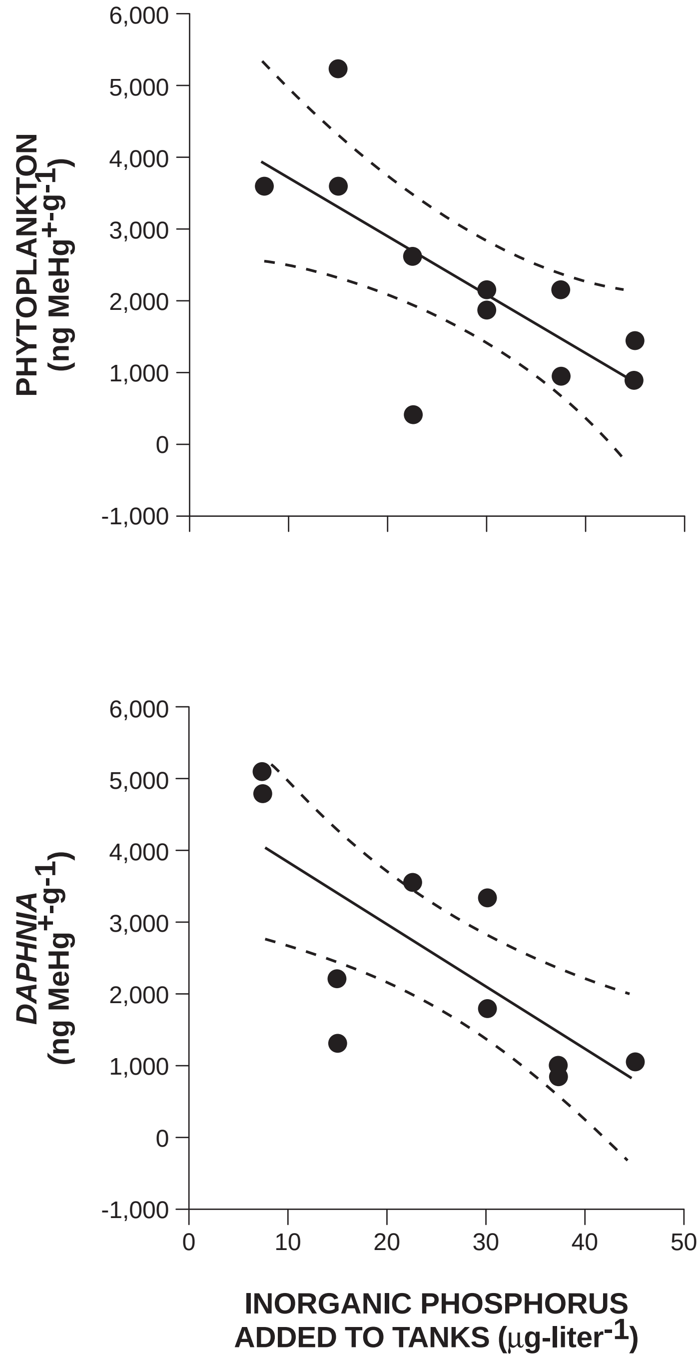

Source: Pickhardt et al. (2002)

Figure 3. Biodilution of methylmercury relative to phosphorus concentrations in water

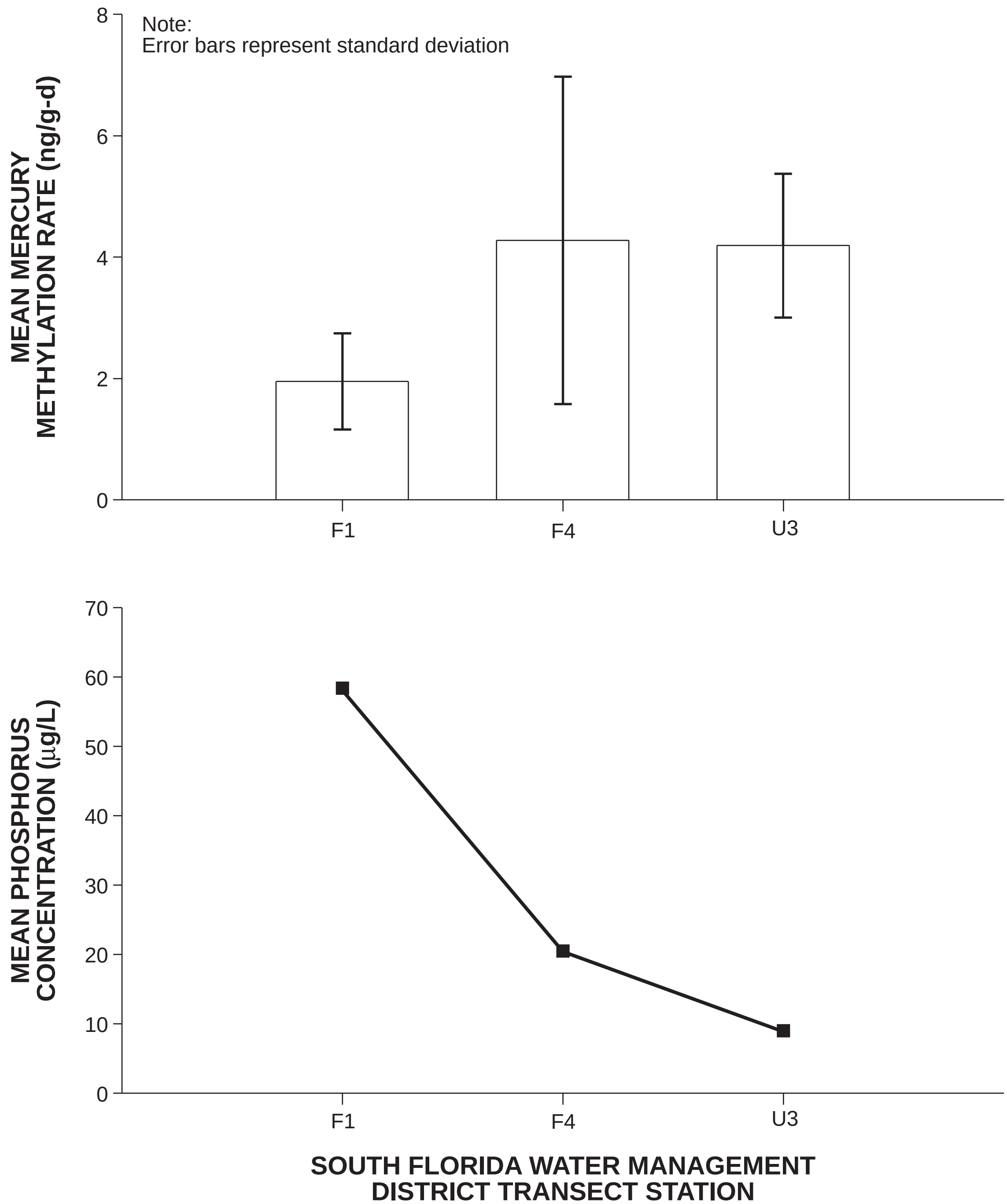

Figure 4. Mercury methylation rate in sediments and phosphorus concentration in surface water. Methylation rates taken from Gilmour et al. (1998).

Table

Table 1. Exerpt of methylmercury partition results from WCA-2A F1 (high phosphorus) and WCA-2A U3 (phosphorus poor)

			Methylmercury					
	Unfiltered	Mass Balance	Particulate > 0.4 μm		Colloidal 0.4 μM-10 kDa		Dissolved < 10 kDa	
Name	(ng/L)	(percent)	(ng/L)	(percent)	(ng/L)	(percent)	(ng/L)	(percent)
Florida Everglades (F1)	0.36	87.9	0.24	68.0	0.002	6.6	0.05	13.3
Florida Everglades (U3)	0.77	114.3	0.13	17.2	0.28	36.8	0.46	60.3

Note: Exerpt of results presented in Babiarz et al. (2002), Table 2.

Attachment A

Algal blooms reduce the uptake of toxic methylmercury in freshwater food webs

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Mercury accumulation in fish is a global public health concern, because fish are the primary source of toxic methylmercury to humans. Fish from all lakes do not pose the same level of risk to consumers. One of the most intriguing patterns is that potentially dangerous mercury concentrations can be found in fish from clear, oligotrophic lakes whereas fish from greener, eutrophic lakes often carry less mercury. In this study, we experimentally tested the hypothesis that increasing algal biomass reduces mercury accumulation at higher trophic levels through the dilution of mercury in consumed algal cells. Under bloom dilution, as algal biomass increases, the concentration of mercury per cell decreases, resulting in a lower dietary input to grazers and reduced bioaccumulation in algal-rich eutrophic systems. To test this hypothesis, we added enriched stable isotopes of Hg to experimental mesocosms and measured the uptake of toxic methylmercury ($CH_3{}^{200}Hg^+$) and inorganic $^{201}Hg^{2+}$ by biota at several algal concentrations. We reduced absolute spike detection limits by 50–100 times compared with previous techniques, which allowed us to conduct experiments at the extremely low aqueous Hg concentrations that are typical of natural systems. We found that increasing algae reduced CH_3Hg^+ concentrations in zooplankton 2–3-fold. Bloom dilution may provide a mechanistic explanation for lower CH_3Hg^+ accumulation by zooplankton and fish in algal-rich relative to algal-poor systems.

Nutrient enrichment with subsequent eutrophication is one of the most important problems impacting lakes worldwide (1, 2). Increased nutrient concentrations produce algal blooms, which in turn alter concentrations of nutrients, gases, pH, and metal ions in the water (3). It is our hypothesis that by increasing algal abundance, nutrient enrichment also alters Hg inputs to lake food webs. Mercury concentrations in fish have been related to metal burdens in their zooplankton prey (4–8), but the connection between Hg accumulation by zooplankton and increasing algal density under nutrient enrichment has not been established. It is critical to discern this association because algae can concentrate Hg from the aqueous phase (e.g., by 100–10,000+ times) and thus provide the greatest inputs of Hg to the food chain (9, 10). Here we report how an induced algal bloom affects the accumulation of methyl and inorganic Hg in the cladoceran *Daphnia* after 2 and 3 weeks of grazing on algae labeled with stable isotopes of Hg. *Daphnia* is a common zooplankton herbivore and known to be a major food for planktivorous fish (11), therefore factors affecting Hg burdens in this "keystone" (12, 13) prey taxon may have important ramifications for predicting CH_3Hg^+ burdens in fish across lakes of varying trophic status.

We experimentally tested the hypothesis that at equal initial concentrations of aqueous Hg, an increase in algae will result in a decrease in Hg uptake—by zooplankton grazers. Our rationale for this hypothesis was that the concentration of metal per cell would be lower in dense algal blooms (hereafter, bloom dilution) because the same amount of metal would be distributed among a greater number of algal cells. A related but different phenomenon, *growth biodilution* of trace metals, is observed in rapidly growing phytoplankton, whereby biomass-specific concentrations of metal diminish as cells divide (14). How either process of dilution with the phytoplankton affects the zooplankton, however, is not known. Possible bloom dilution has been observed for polychlorinated biphenyls (15, 16), As (17), Po, Cd, and Co (18) but has not been reported for Hg. To our knowledge, this is the first experimental manipulation to test bloom dilution in freshwater plankton.

Materials and Methods

Preparation of Algal Density Gradient. To test for effects of algal density on mercury accumulation in algae and on *Daphnia* subsequently grazing on those algae, 12 mesocosm stock tanks were used. The 550-liter resin tanks were scrubbed clean with a low detergent, low trace metal soap, rinsed, and then filled with approximately 450 liters of low ionic-strength water from a crystalline bedrock well. Samples of well water were first analyzed for trace metals by means of magnetic sector inductively coupled plasma-MS to ensure that the well water was low in metals and there were no significant differences between tanks (P.C.P., unpublished data). To buffer the systems from fluctuations in pH and to provide an adequate microbial community, 50 g (wet weight) of leaves (locally collected *Fagus grandifolia*, *Betula papyrifera*, *Acer saccharum*, and *Quercus rubra*) were added to each tank (Fig. 1*A*). Tanks were covered securely with fiberglass window screening to reduce unwanted colonization by invertebrates and to minimize airborne nutrient inputs. Water in the tanks was equilibrated with the atmosphere for 48 h before further additions. Each tank was then inoculated with phytoplankton and microzooplankton, by adding 3 liters of 48 μm of filtered Post Pond (Lyme, NH) water (Fig. 1*B*). Baseline nitrogen and phosphorus were measured after phytoplankton had been in the tanks for 48 h (Fig. 1*C*). Twenty-four hours after baseline nutrient measurements, tanks were randomly assigned to one of six nutrient levels with two tanks at each level. The lowest phosphorus level was 7.4 μg of P·liter^{-1} with inorganic nutrients doubling at each of the subsequent nutrient levels to a maximum of 44.6 μg of P·liter^{-1} at level six. Additions of nitrogen and phosphorus in the form of dissolved $NaNO_3$ and K_2HPO_4 (2.51 and 36.72 g·liter^{-1}, respectively) were made so as to achieve the desired atomic ratio of 30:1 (N:P) (Fig. 1*D*). Phosphorus concentrations added to the tanks corresponded to concentrations found routinely in lakes in the northeastern U.S. (19). Standing stocks of phytoplankton within the 12 tanks were left to develop for 9 days after the application of the 6 inorganic nutrient levels (Fig. 1 *D* and *E*).

Adding Hg Isotopes and Zooplankton. On day 14 (Fig. 1*E*), stable isotopes were added to the tanks. A stock solution of 50 mg·liter^{-1} enriched ^{201}Hg (Oakridge National Laboratory, 98.11% ^{201}Hg) was prepared in 0.01 M HCl. Enriched mono-

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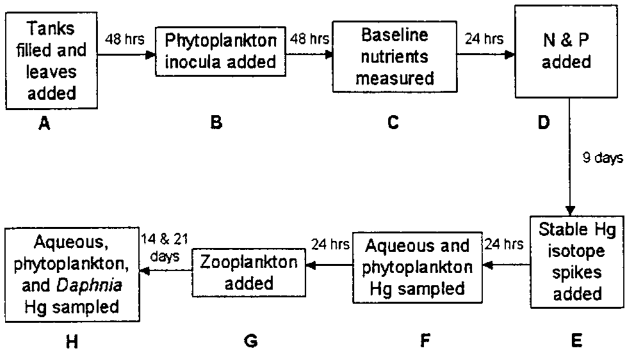

Fig. 1. Chronology of mesocosm tank experiments. Time intervals given between boxes indicate time elapsed between the respective procedures. Note that the sampling described in *H* was conducted at two separate periods after zooplankton addition (*G*).

methylmercury, $CH_3{}^{200}Hg^+$ (Oakridge National Laboratory, 96.41% ^{200}Hg), was synthesized by methylating $^{200}HgCl_2$ with methylcobalamin (20). After extraction with CH_2Cl_2 and back extraction into dilute HCl, a stock solution of 8 mg·liter^{-1} $CH_3{}^{200}HgCl$ in 0.01 M HCl was made. Of the $^{201}HgCl_2$ and $CH_3{}^{200}HgCl$ stock solutions, 1.00 and 1.25 ml, respectively, were added and thoroughly mixed with a wooden paddle to each of the 12 tanks to achieve an initial tank water concentration of 100 ng·liter^{-1} ^{201}Hg and 20 ng·liter^{-1} $CH_3{}^{200}Hg$ (Fig. 1*E*). Forty-eight hours after the stable isotope spikes, macrozooplankton collected from Post Pond with an 80-μm net were added at approximately 2 times the natural density to allow for mortality in transition.

Tank Monitoring. Physical conditions in all of the tanks were monitored throughout the experiments. Specific conductivity, dissolved oxygen, temperature, and tank water pH were measured every 48 h between 13:00 and 15:00. By the addition of small volumes of dilute (2.0 M) H_2SO_4, the pH was maintained between 7.8 and 8.2 for all tanks. Samples for phytoplankton biomass (by means of chlorophyll *a* samples) were collected 24 h after mercury spike additions (Fig. 1*F*) and at the two zooplankton sampling periods (Fig. 1*H*). Samples for zooplankton taxonomy, density, length, and biomass were also collected when zooplankton were sampled for Hg (Fig. 1*H*).

Collection and Inductively Coupled Plasma (ICP)-MS Analyses of Isotope Samples. The isotope spike analyses were performed by continuous-flow cold-vapor generation magnetic sector-ICP-MS (8, 21–23). Collection and digestion of samples for $CH_3{}^{200}Hg^+$ and $^{201}Hg^{2+}$ in water, particulates, and zooplankton were conducted as follows. Sampling equipment and sample vials were acid-cleaned in sequential 1 M nitric acid, 1:5 hydrochloric acid, and trace metal-grade (distilled) dilute nitric acid with ultra-pure water rinses before and after each acid bath (8). Aqueous mercury samples were collected in borosilicate glass vials with Teflon septa and preserved to ≈pH 1 with Seastar Baseline HNO_3 (Seastar Chemicals, Sidney, BC, Canada). Particulate samples (particles >0.45 μm and <45 μm) were collected by filtering 100 ml of tank water on to cellulose acetate filters that had been rinsed with dilute (≈0.33 M) distilled nitric acid and ultra-clean water. Cellulose acetate filters with sample were immediately transferred to Teflon vials. Aqueous and particulate samples were collected 24 h after metal spike additions (Fig. 1*F*) and again when live zooplankton were sampled (Fig. 1*H*). Live zooplankton were field-sorted into Teflon vials under a dissecting scope 2 and 3 weeks after metal spike additions (Fig. 1*H*). *Daphnia* mercury burdens were calculated for two tanks at each respective treatment level with two samples (10–20 *Daphnia mendotae*) from each tank. All samples were stored in the dark at ≈4°C before digestion and analysis. Particulate and zooplankton samples were digested for 10–12 h at 70°C with a mixture of HNO_3 and HCl (2:1; Seastar Baseline acids). Acidified water samples were not digested further (8).

The quantification of the enriched isotope spikes of ^{200}Hg and ^{201}Hg was performed by standard-sample-standard bracketing with certified external Hg standards of natural isotopic abundance. The natural background of ^{200}Hg and ^{201}Hg was subtracted based on the measured ^{198}Hg/^{200}Hg and ^{198}Hg/^{201}Hg ratio of the bracketing standards. The external calibration of the ^{200}Hg and ^{201}Hg spike concentrations was based on the atomic mass fraction of ^{200}Hg and ^{201}Hg in the natural abundance standards (46.24 g ^{200}Hg·mol^{-1} Hg and 26.54 g ^{201}Hg·mol^{-1} Hg). The procedural detection limits by isotope dilution were a function of the precision of the isotope ratio measurements (about 0.1%) and the background concentrations. Our method allows for the unambiguous tracking of picograms/femtomols of CH_3Hg^+ and Hg(II) from aqueous spikes into algae and zooplankton.

Detection Limits. Twenty-four hours after the stable isotope additions, aqueous Hg concentrations were close to our method detection limits for water samples (0.5 ng·liter^{-1} for ^{200}Hg and ^{201}Hg). These extremely low aqueous Hg concentrations met our goal of conducting experiments at dilute concentrations typical of most lakes (8). We achieved detection limits of the isotopically labeled Hg species for the particulate and zooplankton samples for ^{200}Hg and ^{201}Hg of 1 ng·liter^{-1} or 0.5 pg, respectively, which is a 50–100-fold improvement over traditional analytical techniques using additions of isotopically unlabeled Hg or radioactive Hg tracers (8, 24).

Statistical Analyses. We adopted a gradient approach with our mesocosm experiments wherein we traded off lower replication at each treatment level ($n = 2$) in favor of increasing the number of treatment levels ($n = 6$). This design is intended for regression analysis and allows for a more robust examination of trends and overall effects of a treatment in the face of high variation within treatments. This gradient approach was ideal for our goal to identify the general direction and magnitude of nutrient addition and increasing algal biomass effects on mercury uptake by grazers. The strength and generality afforded by this approach to ascertain the overall effect of treatments on specific dependent variables has made it a common approach for experiments involving ecological gradients (25, 26). Treatment effects were assessed by means of regression analysis [*F* test comparison of model mean squares divided by error mean squares, JMP (version 4.04, SAS Institute, Cary, NC)]. Least squares regression lines and 95% confidence intervals are plotted for variables only when the relationship is significant at the $P \leq 0.05$ level.

Results and Discussion

As expected, 9 days after the inorganic nutrient gradient was applied to the mesocosms there were significant differences in standing algal biomass measured as chlorophyll *a* (Fig. 2*A*) across tanks. Temperature, conductivity, and pH did not vary across treatments although there was a significant increase ($R^2 = 0.17$, $P < 0.0001$) in dissolved oxygen at higher nutrient concentrations as expected with increased algal density (P.C.P., unpublished data). The conditions in the tanks at the time of zooplankton addition (11 days after inorganic nutrient additions as per Fig. 1) were well within the range of conditions experienced in the pelagic water of oligotrophic to mesotrophic lakes in temperate North America (7, 19).

Our first important finding was that at the time of zooplankton addition there was considerable bloom dilution of the Hg spikes

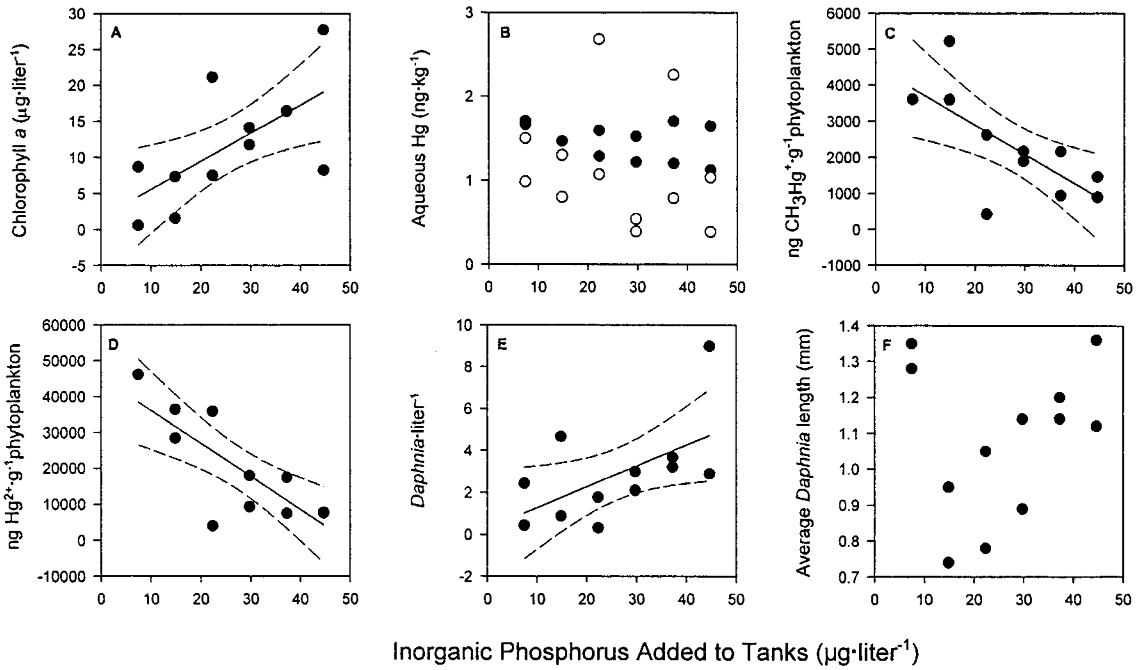

Fig. 2. Effect of added phosphorus on chlorophyll *a* concentrations at the time of the metal spike additions (*A*). $n = 12$, chlorophyll $a = 0.389(\mu g\ P\ added \cdot liter^{-1}) + 1.69$, $R^2 = 0.431$, $P < 0.021$. Aqueous concentrations of $CH_3{}^{200}Hg^+$ (●) and $^{201}Hg^{2+}$ (○) 24 h after additions to experimental tanks (*B*). For aqueous $CH_3{}^{200}Hg^+$: $n = 12$, $CH_3Hg^+ = -0.005(\mu g\ P\ added \cdot liter^{-1}) + 1.59$, $R^2 = 0.102$, $P > 0.311$. For $^{201}Hg^{2+}$: $n = 12$, $Hg^{2+} = -0.010(\mu g\ P\ added \cdot liter^{-1}) + 1.41$, $R^2 = 0.035$, $P > 0.558$. Effect of nutrient additions to CH_3Hg^+ (*C*) and Hg^{2+} (*D*) associated with algal biomass 24 h after metal spike additions. For CH_3Hg^+ (*C*): $n = 11$, $CH_3Hg^+ = -80.14(\mu g\ P\ added \cdot liter^{-1}) + 4502$, $R^2 = 0.499$, $P < 0.016$. For Hg^{2+} (*D*): $n = 11$, $Hg^{2+} = -917(\mu g\ P\ added \cdot liter^{-1}) + 45290$, $R^2 = 0.623$, $P < 0.004$. Algal biomass derived from chlorophyll *a* concentrations at time of metal spikes, assumes 1.25% of algal biomass is chlorophyll *a* (27). For both *C* and *D* an outlier from the lowest phosphorus addition level was excluded from regression analyses—in each case, the measured value exceeded 1 SD from a 0.99 confidence interval. Effect of nutrient additions on adult *Daphnia* density 3 weeks after metal spikes (*E*): $n = 12$, *Daphnia*$\cdot liter^{-1} = 0.74(\mu g\ P\ added \cdot liter^{-1}) + 0.27$, $R^2 = 0.323$, $P < 0.054$. Effect of nutrient additions on the mean length of adult *Daphnia* 3 weeks after metal spikes (*F*): $n = 12$, mean *Daphnia* length $= 0.02(\mu g\ P\ added \cdot liter^{-1}) + 1.01$, $R^2 = 0.030$, $P > 0.59$. Total nitrogen and total phosphorus addition were kept at the atomic ratio of 30:1 as described in *Materials and Methods*. The 95% confidence intervals (---) are plotted for significant regressions.

under reasonable levels of nutrient enrichment. Twenty-four hours after the mercury spikes were added there was no detectable difference in aqueous Hg concentrations across tanks (Fig. 2*B*). Yet organic CH_3Hg^+ and inorganic Hg^{2+}spike concentrations in particulates were 10^3-10^4 times greater than in the water after 24 h of exposure to the isotope spikes (Fig. 2 *C* and *D*), demonstrating the rapid and successful incorporation of the isotope spikes into algal biomass. Moreover, there were significant differences in total algal Hg measured across the nutrient gradient after 24 h (Fig. 2 *C* and *D*). In general, tanks with greater nutrient enrichment had greater algal biomass and lower Hg per gram of algal material (Fig. 2 *A*, *C*, and *D*). This evidence is a sound demonstration of bloom dilution.

Our second major finding was that as hypothesized, bloom dilution of Hg in algae initiated different mercury uptake dynamics in the zooplankton under high- vs. low-nutrient enrichment. Specifically, methylmercury concentrations were consistently and significantly lower in *Daphnia* from the high nutrient, high initial algal biomass tanks compared with *Daphnia* from the low nutrient, and low initial algal biomass tanks at 2 and 3 weeks after zooplankton additions (Fig. 3 *A* and *C*). Correspondingly, low algal abundances resulted in a 2–3-fold increase in the accumulation of CH_3Hg^+ in *Daphnia* from low-nutrient tanks (Fig. 3 *A* and *C*). From these results, we infer that the concentration of CH_3Hg^+ in *Daphnia* across treatments was related to the concentration of CH_3Hg^+ (Fig. 2*C*) in the algal cells they ingested, which was in turn affected by algal biomass; e.g., that bloom dilution drives a diminution of metal in the zooplankton. We also observed similar results for effects of bloom dilution on calanoid and cyclopoid copepods (P.C.P., unpublished data). This result has important implications for trophic transfer of toxic CH_3Hg^+ to fish in oligotrophic lakes.

Despite the highly significant relationships measured in *Daphnia* CH_3Hg^+ burdens across the nutrient gradient, there is a substantial amount of unexplained variation in our data. Varying *Daphnia* ages, feeding rates, the number of developing embryos in *Daphnia* brood pouches, or possible genetic differences are possible factors contributing to this unaccounted variance. Moreover, there are other possible explanations for our finding. For example, as hypothesized for rapidly growing algae [e.g., growth biodilution (14)], a diminution of the mass-specific metal spike in animals could result whenever there are rapid increases in zooplankton density or biomass (i.e., when the production of new tissue outpaces the uptake of metal). Growth biodilution cannot explain our results at 2 weeks because there were no differences in zooplankton density across treatments even though marked differences in methylmercury levels of individuals were evident. Growth biodilution did not occur by means of increases in body size either, because there were no significant body-size differences in *Daphnia* with increasing nutrient addition 2 and 3 weeks after spike additions (see Fig. 2*F* for lengths at week 3). However, 3 weeks after the zooplankton additions there was a marginally significant trend for lower methylmercury concentrations in treatments with higher *Daphnia* densities (Fig. 2*E*). This pattern provides some support for the hypothesis that growth biodilution leads to lower mass-specific CH_3Hg^+ in *Daphnia* at high density over time.

Finally our third significant finding was that unlike CH_3Hg^+, bloom dilution of inorganic Hg^{2+} concentrations in the algae (Fig. 2*D*) had no measurable influence on the accumulation of

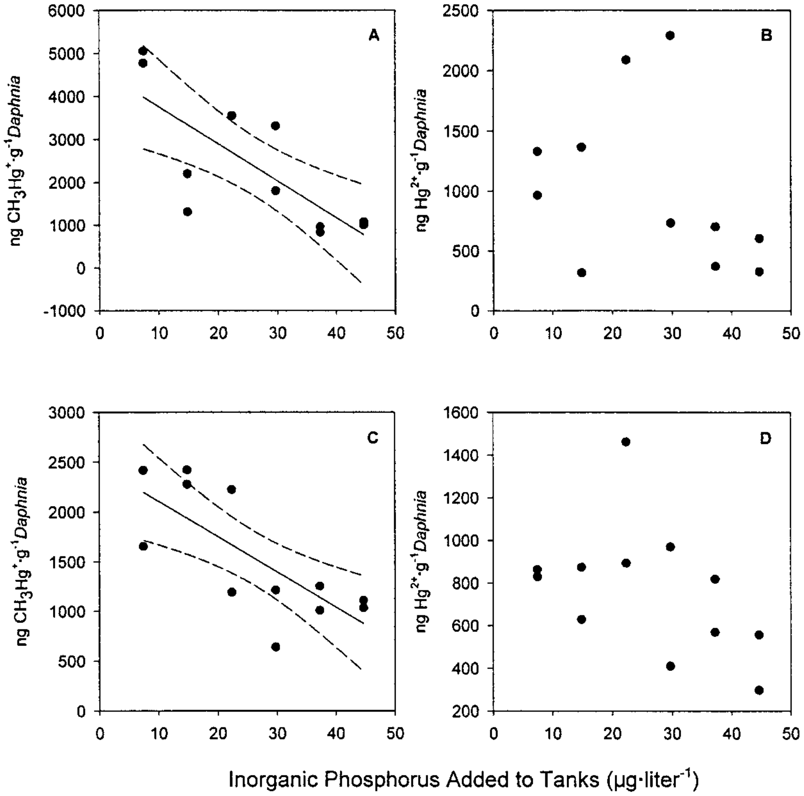

Fig. 3. Mean CH_3Hg^+ (*A*) and Hg^{2+} (*B*) concentration in *Daphnia* (g^{-1} dry weight *Daphnia*) against phosphorus addition (as per *Materials and Methods*) 2 weeks after zooplankton were added to the tanks. For CH_3Hg^+: $n = 12$, $CH_3Hg^+ g^{-1} = -643(\mu g\ P\ added \cdot liter^{-1}) + 4630$, $R^2 = 0.583$, $P < 0.0063$. For Hg^{2+}: $n = 12$, $Hg^{2+} g^{-1} = -125.4(\mu g\ P\ added \cdot liter^{-1}) + 1455$, $R^2 = 0.115$, $P > 0.306$. Mean CH_3Hg^+ (*C*) and Hg^{2+} (*D*) concentration in *Daphnia* with nutrient addition 3 weeks after zooplankton were added to the tanks. For CH_3Hg^+: $n = 12$, $CH_3Hg^+ g^{-1} = -265(\mu g\ P\ added \cdot liter^{-1}) + 2465$, $R^2 = 0.554$, $P < 0.0056$. For Hg^{2+}: $n = 12$, $Hg^{2+} g^{-1} = -78.7(\mu g\ P\ added \cdot liter^{-1}) + 1041$, $R^2 = 0.213$, $P > 0.130$. The 95% confidence intervals (---) are plotted for significant regressions.

inorganic Hg^{2+} in *Daphnia* (Fig. 3 *B* and *D*). To our knowledge, this is the first study to experimentally demonstrate the preferential accumulation of CH_3Hg^+ relative to inorganic Hg^{2+} in grazing invertebrates feeding on an intact phytoplankton assemblage. Preferential accumulation of CH_3Hg^+ in zooplankton is reasonable to expect because zooplankton show the greatest assimilation rates of Hg from algal cytoplasm (28), where CH_3Hg^+ is concentrated in algal cells (6, 9, 10). In contrast, inorganic mercury tends to remain surface-bound and thus is less likely to be assimilated (10).

Our study did not include data for mercury accumulation by nonalgal particulate matter, which is known to be a significant Hg source to nonselective grazers such as *Daphnia* in some natural systems (29). In these experiments, the tanks were low in nonalgal particulates. Another important determinant of mercury cycling in aquatic systems that we did not quantify was the scavenging of mercury compounds by suspended particulate matter and detritus (30).

We conclude that CH_3Hg^+ transferred to grazing zooplankton, and eventually to fish and other vertebrates, will be influenced by nutrient pulses and algal blooms. More specifically, algae effectively and rapidly concentrate both inorganic and organic Hg, but the metal burden per cell decreases in algal blooms. Bloom dilution of CH_3Hg^+ in algae results in a substantial reduction of CH_3Hg^+ uptake by cladocerans in high-nutrient, high-algae conditions. Conversely, cladocerans feeding within low-nutrient, low-algae treatments accumulate more CH_3Hg^+. Further, zooplankton that graze on algae preferentially accumulate CH_3Hg^+ relative to inorganic Hg^{2+}. This difference is instrumental in the efficient trophic transfer of CH_3Hg^+ relative to inorganic Hg to vertebrates. A final, unique feature of this research is demonstration of the value of using specific, stable isotope spikes of Hg to unambiguously track mercury through the food web near ambient concentrations. In particular, we tracked spikes of CH_3Hg^+ and inorganic Hg^{2+} and obtained exceptionally low absolute detection limits of those isotopic spikes (0.5–1 pg), which represents a significant improvement over traditional natural Hg or radioisotope methods.

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1. Carpenter, S. R., Caraco, N. F., Correll, D. L., Howarth, R. W., Sharpley, A. N. & Smith, V. H. (1998) *Ecol. Appl.* **8,** 559–568.
2. Smith, V. H., Tilman, G. D. & Nekola, J. C. (1999) *Environ. Pollut.* **100,** 179–196.
3. Moore, M. V., Pace, M. L., Mather, J. R., Murdoch, P. S., Howarth, R. W., Folt, C. L., Chen, C. Y., Hemond, H. F., Flebbe, P. A. & Driscoll, C. T. (1997) *Hydrol. Process.* **11,** 925–947.
4. Hall, B. D., Bodaly, R. A., Fudge, R. J. P., Rudd, J. W. M. & Rosenberg, D. M. (1997) *Water Air Soil Pollut.* **100,** 13–24.
5. Westcott, K. & Kalff, J. (1996) *Can. J. Fish. Aquat. Sci.* **53,** 2221–2228.
6. Lawson, N. M. & Mason, R. P. (1998) *Biogeochemistry* **40,** 235–247.
7. Stemberger, R. S. & Chen, C. Y. (1998) *Can. J. Fish. Aquat. Sci.* **55,** 339–352.

8. Chen, C. Y., Stemberger, R. S., Klaue, B., Blum, J. D., Pickhardt, P. C. & Folt, C. L. (2000) *Limnol. Oceanogr.* **45,** 1525–1536.
9. Mason, R. P., Reinfelder, J. R. & Morel, F. M. M. (1995) *Water Air Soil Pollut.* **80,** 915–921.
10. Mason, R. P., Reinfelder, J. R. & Morel, F. M. M. (1996) *Environ. Sci. Technol.* **30,** 1835–1845.
11. Brooks, J. L. & Dodson, S. I. (1965) *Science* **150,** 28–35.
12. Ives, A. R., Carpenter, S. R. & Dennis, B. (1999) *Ecology* **80,** 1405–1421.
13. Mittelbach, G. G., Turner, A. M., Hall, D. J., Rettig, J. E. & Osenberg, C. W. (1995) *Ecology* **76,** 2347–2360.
14. Sunda, W. G. & Huntsman, S. A. (1998) *Sci. Total Environ.* **219,** 165–181.
15. Dachs, J., Eisenreich, S. J. & Hoff, R. M. (2000) *Environ. Sci. Technol.* **34,** 1095–1102.
16. Larsson, P., Okla, L. & Cronberg, G. (1998) *Can. J. Fish. Aquat. Sci.* **55,** 1926–1937.
17. Chen, C. Y. & Folt, C. L. (2000) *Environ. Sci. Technol.* **34,** 3878–3884.
18. Jeffree, R. A. & Szymczak, R. (2000) *Environ. Sci. Technol.* **34,** 1966–1969.
19. Stemberger, R. S. & Miller, E. K. (1998) *Environ. Monit. Assess.* **51,** 29–51.
20. Imura, N., Sukegawa, E., Pan, S.-K., Nagao, K., Kim, J.-Y., Kwan, T. & Ukita, T. (1971) *Science* **172,** 1248–1249.
21. Klaue, B. & Blum, J. D. (1999) *Anal. Chem.* **71,** 1408–1414.
22. Klaue, B., Kesler, S. E. & Blum, J. D. (2000) in *11th Annual International Conference on Heavy Metals in the Environment*, ed. Nriagu, J. (Univ. of Michigan, School of Public Health, Ann Arbor, MI), Contribution 1101.
23. Lauretta, D., Klaue, B., Blum, J. D. & Buseck, P. R. (2001) *Geochim. Cosmochim. Acta* **65,** 2807–2818.
24. Hook, S. E. & Fisher, N. S. (2001) *Mar. Biol.* **138,** 1131–1140.
25. Diehl, S. & Feissel, M. (2000) *Am. Nat.* **155,** 200–218.
26. Sarnelle, O. (1999) *Limnol. Oceanogr.* **44,** 357–370.
27. Reynolds, C. S. (1984) *The Ecology of Freshwater Phytoplankton* (Cambridge Univ. Press, Cambridge, U.K.), pp. 37–39.
28. Reinfelder, J. R. & Fisher, N. S. (1991) *Science* **251,** 794–796.
29. Plourde, Y., Lucotte, M. & Pichet, P. (1997) *Can. J. Fish. Aquat. Sci.* **54,** 821–831.
30. Montgomery, S., Lucotte, M. & Cournoyer, L. (2000) *Sci. Total Environ.* **261,** 33–41.

United States Department of the Interior

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September 24, 2002

Dr. Jeffrey L. Jordan, Professor and Panel Chair
2003 Everglades Consolidated Report Peer Review Panel
Dept. of Agricultural and Applied Economics
University of Georgia
Griffin, GA

Dear Dr. Jordan:

Please find enclosed comments on the draft *2003 Everglades Consolidated Report*. These comments were provided by staff of the Everglades Program Team (U.S. Department of Interior), the Arthur R. Marshall Loxahatchee National Wildlife Refuge, and Everglades National Park. These comments are technical comments and do not necessarily represent official policy of the Department of Interior.

We appreciate all of the hard work that the authors have done to prepare their chapters, and we commend the SFWMD, FDEP, and the other agencies and entities involved for developing a comprehensive report.

We would like to encourage the Review Panel to recommend that all review comments to the draft *2003 Everglades Consolidated Report* be published in the final version. The *2000* and *2001 ECR* included both an appendix of all public comments and an appendix of chapter authors' response to public comments. The *2002 ECR* eliminated the appendix of public comments, presumably as a function of printing costs. As the *2003 ECR* will be published in electronic form, this constraint should no longer be an issue. Additionally, as a minor point, the draft version of future *ECR*s would be easier to review (and for authors to address review comments) if there were line numbers present (which could be removed from the final version).

Chapter 1: Introduction

1. In the second paragraph of the Introduction, the Everglades Protection Area is defined. The Arthur R. Marshall Loxahatchee National Wildlife Refuge needs to be included in this definition. The Everglades Forever Act of 1994 and the 1991 Federal Consent Decree specifically define the Everglades Protection Area as including <u>both</u>

2. WCA-1 and the A.R.M. Loxahatchee NWR. These areas are not completely interchangeable (as written throughout much of the draft *2003 ECR* {e.g., 5-5; Fig. 4A-7; 8B-6}), as there are areas of the Refuge that fall outside of the levee system that form the boundaries of WCA-1. This is important distinction because areas such as the Cypress Swamp fall within the water quality requirements established by the EFA. (See attached Figure.)

   Although it appears to be a semantic point, the Refuge should be referred to as the Arthur R. Marshall Loxahatchee National Wildlife Refuge (or A.R. M. Loxahatchee National Wildlife Refuge) as this is its formal name (it is correct throughout Chapter 1, but the discrepancies begin in Fig. 2A-2 and scattered throughout the text of the draft *2003 ECR*). We encourage the Peer-Review panel to ask authors of all chapters to check this in the text/figures of their respective chapters and make corrections as necessary. As the *2003 ECR* will be used as the "template" for subsequent reports, it is important that this correction be made at the same time for all components of the report.

3. Table 1-1 is the same table from the *2002 ECR*. Are there changes other than updating the dates (and the few typos {e.g., "Comprehensive Ecosystem Restoration Program"}) in this table?

Chapter 2A: Status of Water Quality in the Everglades Protection Area

1. In general, we are concerned that this chapter gives too much credibility to the Florida Class III Criteria. From the perspective of resource and ecosystem protection, it is inadequate to focus exclusively on standards that are known to be inappropriate or not protective.

   Some of the Class III Criteria are clearly inappropriate, and worse, not protective or sufficiently protective of our unique Everglades ecosystem. The minimum conductivity criterion of $\leq$1275 µmho/cm not to be exceeded is far above historic values for most of the ecosystem, and is completely un-protective of the Refuge interior. The alkalinity criterion of $\geq$20 mg/L is also inappropriate for the naturally soft water dominated Refuge. Prior to construction of drainage canals and agricultural land use changes, much of the rest of the Everglades was probably also soft water with low alkalinity. It is troubling that a major standards development effort is directed toward developing an SSAC (site specific alternative criterion) for DO that will lower standards, while little effort is being directed toward developing more stringent criteria for inadequately protective standards.

2. Because of its wide distribution within the EPA, and current controversy and uncertainty about effects, we urge the continued monitoring of atrazine at STA inflow and discharge sites, as well as at sites throughout the EPA. Atrazine is a water-soluble herbicide that selectively controls broadleaf weeds in agriculture fields. It is said to be the most widely used herbicide in the world. Atrazine is of particular economic importance to sugar farmers in South Florida. Atrazine is also widely used for other

agricultural purposes and for weed control in lawns. Atrazine may also be discharged from point sources such as sugar mills (Chung et al. 1996). Although atrazine is relatively recalcitrant, it can be mineralized in wetlands (Chung et al. 1996) and removed by constructed wetland systems (Alvord and Kadlec 1996), however, there is little evidence of atrazine mineralization in the STAs.

The rate of atrazine exceedances monitored in the EPA during this reporting period increased relative to past years. The criterion used in these evaluations is based on protection of human health, as listed in the *2001 ECR*. The USEPA now has proposed guidance for setting criteria for protection of aquatic life (USEPA 2001a; USEPA 2001b). Detenbeck et al. (1996) found that periphyton, *Ceratophyllum demersum*, *Zizania aquatica*, and *Daphnia* were significantly affected by atrazine. This suggests that Everglades communities may be especially sensitive to this pesticide, and that specific criterion should be developed to protect the ecosystem from chronic atrazine exposure. We believe research directed at establishing appropriate atrazine criteria for the EPA should be initiated (also see our comments on chapter 4A).

3. We see only reference to Class III Criteria in this chapter. Why is anti-degradation of outstanding Florida Waters (OFWs) not equally considered for those water bodies that are designated OFW.

4. When uncertainty is higher we need to be more (not less) conservative and protective of the ecosystem and human health. We find the statistical approaches used in this chapter troubling because it violates this principal. We understand the desire for consistency with other evaluations including the Florida Impaired Waters 303d designations and understand that it may reduce the required effort and increase efficiency. However, no justification is developed in the chapter that these methods are appropriate for the purposes of this report.

   The “Excursion Analysis” technique introduced here tests the null hypothesis that the frequency of excursion is less than 10%. Using a binomial probability distribution, a 95% confidence that the null hypothesis can be rejected then is required to list a group of stations as “of concern.” This process has the effect of requiring substantial proof of a problem before it is brought forward as a concern. This may be appropriate in some regulatory programs, but is not appropriate here. For the purposes of this report, as well as for the purpose of managing the quality of the Everglades ecosystem, we should have concern for all stations where we cannot reject the null hypothesis that the excursion frequency is greater than 10%.

   The example provided on page 2A-12 clearly illustrates the problem with the excursion analysis approach applied in this report. It is stated that:

   > “*For example, one of six measurements above the criterion is clearly a weaker case for impairment than six of 36; however, both cases result in an excursion frequency of 16.7 percent.*”

   From a quality management perspective, the case of one in six is of greater potential concern because, under a binomial hypothesis, we may have a failure rate much larger than 16.7%, perhaps 33%, and with this limited number of samples we cannot reject this possibility.

The excursion analysis approach proposed in the report would lead to the result that any reduction in sampling frequency would likely reduce the number of identified sites of concern. This violates the principle that where greater uncertainty exists we need to be more cautious in making environmental management decisions.

5. The maximum specific conductance (conductivity) reported for the Refuge interior was 3686 µmhos/cm. This observation is clearly beyond the bounds of values anticipated (see comment # 2 in Chapter 3 and comment #3 in Chapter 4A), and leads to questioning the procedures used in data quality assurance. What procedures are used to identify anomalous samples? The report states that values that are far outside the anticipated range are dropped. However, more sophisticated identification of anomalous samples could improve the analysis. For example, chloride, TDS, field conductivity, and lab conductivity are highly correlated. It is straightforward to flag any samples where these parameters fall outside a historically derived relationship (Hughes et al. 1994). Cation – anion balance can also be applied to identify errors (Hara et al. 1986). Have such procedures been applied or considered?

6. It is stated that: "*It is widely accepted that DO concentrations are normally low in periphyton-dominated marsh environments, such as the Everglades, due to the natural processes of photosynthesis and respiration (Belanger and Platko, 1986; McCormick et al., 1997).*"

   This quoted statement is unclear because "low" implies lower than some other community or habitat. It has long been accepted that DO is lower in colored waters draining from wetlands (Crisman et al. 1998). The Everglades exhibit a more complex DO response, and are unusual in this respect. DO is often higher at un-impacted Everglades sites dominated by periphyton than in moderately impacted sites that have lost their periphyton community. Increased P concentrations are therefore associated with reduced average DO concentrations.

7. A second concern with the DO analysis presented here is the use of annual averages. For many organisms average annual DO has little meaning. Although some chronic stress may result from low average DO, the more essential requirement is to completely avoid lethal acute episodes of very low or near zero concentrations. Many wetland organisms, both plant and animal, have likely evolved mechanisms to deal with these stresses, especially chronic low DO. Episodes of one or two weeks of hypoxic conditions may not significantly reduce observed annual average DO but may kill even well adapted species. Thus, for data collection at a monthly interval it is more indicative of ecosystem disturbance to use a measure of the lower end of the distribution (e.g. first quartile, 20 percentile, or minimum) rather than a measure of central tendency such as annual average.

8. For the purposes of compliance monitoring, it would be beneficial to provide data on loads coming from individual structures into the EPA. This was done in the *2000 ECR*; however, these data were not included in the *2001* and *2002 ECR*. At the August 1, 2002 Technical Oversight Committee meeting, two of the five principals (Refuge and Park) requested that these data be provided in the *2003 ECR*. At that meeting, Garth Redfield responded that these data are available and would be included in the *2003 ECR*.

9. The Everglades Round Robin (ERR) laboratory analyses was described in the *2002 ECR* (although no ERR results presented), but text of 2A and Appendix 2A-1 addressed the issue of laboratories. There is no discussion/presentation of the ERR in Ch. 2 of the draft *2003 ECR*. This is of interest/relevance for some of the STA optimization results presented in Chapter 4B and should be included.

10. In the draft *2003 ECR*, MDL values were handled as ½ MDL, based on a suggestion from the peer review panel from the *2002 ECR* (Appendix 1-1-11 to 1-1-12, *2002 ECR*), while data less than MDL in the *2002 ECR* were assigned the value of the MDL. How does this influence the readers' ability to compare summary data among years (e.g., Tables 2A-5 through 2A-7)?

11. When discussing possible reasons for higher TP values in the Park (page 2A-28), it would be useful to cite Walker's recent draft report to the TOC that discusses possible reasons. Some of these possibilities should be included in this chapter, such as the effects of changed water patterns due to the Cape Sable Seaside Sparrow emergency.

12. The SFWMD as required by the Modified Consent Decree and requested by TOC has been collecting dual samples at the C-111 and Coastal Basin inflow points to ENP. One set is the old stations of S-18c, S-332 and S-175 and the other is the new stations S-18c, S-332D, and S-174. This has been done for over a year. It would be beneficial if an analysis of these data was included in this ECR. (See comment # 2 in Chapter 8B.)

13. When specific conductance (conductivity) is reported, you should in at least in one place in the chapter state whether this is a lab or field measurement, and state that this is (or is not) temperature compensated. (See comment # 5 in Chapter 4A.)

14. The reference on page 2A-12 to a publication by Smith et al. (2001) is not listed in the Literature Cited section for the chapter.

15. SSAC (first used on page 2A-19) is not defined in the Acronyms appendix. Many readers may not be familiar with this term.

16. We are unable to locate the report by Belanger and Platko (1986, cited on page 2A-19) on the SFWMD publications web site.

<u>References Cited:</u>

Alvord, H. H., and Kadlec, R. H. (1996). "Atrazine fate and transport in the Des Plaines wetlands." *Ecol. Model.*, 90(1), 97-107.

Chung, K., Ro, K. S., and Roy, D. (1996). "Fate and enhancement of atrazine biotransformation in anaerobic wetland sediment." *Water Research*, 30(2), 341-346.

Crisman, T. L., Chapman, L. J., and Chapman, C. A. (1998). "Predictors of Seasonal Oxygen Levels in Small Florida Lakes: The Importance of Color." *Hydrobiologia*, 368, 149-155.

Detenbeck, N. E., Hermanutz, R., Allen, K., and Swift, M. C. (1996). "Fate and effects of the herbicide atrazine in flow-through wetland mesocosms." *Environmental Toxicology and Chemistry*, 15(6), 937-946.

Hara, H., Sekigichi, K., and Ujiie, A. (1986). "A quantitative index of the ion balance for precipitation chemistry." *Environ. Technol. Lett.*, 7(11), 597-600.

Hughes, S. G., Taylor, E. L., Wentzell, P. D., McCurdy, R. F., and Boss, R. K. (1994). "Models for conductance measurements in quality assurance of water analysis." *Analytical Chemistry*, 66(6), 830-835.

USEPA. (2001a). "Ambient Aquatic Life Water Quality Criteria for Atrazine - Draft." *EPA-822-D-01-006*, USEPA, Washington, DC.

USEPA. (2001b). "Fact Sheet: Atrazine - Aquatic Life Water Quality Criteria - U.S. EPA." *EPA-822-F-01-006*, USEPA.

Chapter 2B: Mercury Monitoring, Research and Environmental Assessment

1. p. 2B-25, last paragraph, line 8: Shouldn't the p value read "$p > 0.05$"?

2. p. 2B-34, final bullet under "Key Findings…": The significance of this bullet is not clear. Although light may be the factor limiting primary productivity in the most enriched areas, phosphorus likely remains the limiting factor in areas that are moderately enriched, e.g., water-column TP near 30 ppb. Therefore, there probably is a relationship between P, primary production, and, therefore, biodilution (to the extent that this process is even important) across much of the "enriched zone."

3. p. 2B-35, final bullet under "Summary": Does it follow from this statement that the agencies do not recommend reducing sulfate loads to the EPA as a means of reducing the MeHg problem?

4. p. 2B-37, final bullet: While the WCA-3A-15 site may not be influenced by EAA P loads, are there water quality data to support or refute the possibility that this location is impacted by sulfate loading? Gradients of more conservative elements introduced to the Everglades by EAA discharges extent much further into the EPA than do those for P.

Chapter 3: Performance and Optimization of Agricultural Best Management Practices

1. p. 3-19, first paragraph, line 3: Although the EFA mandates a continued program of BMP research, is there any regulatory mechanism that requires landowners to optimize their BMPs as long as the basin continues to achieve a 25% load reduction? What is the economic incentive to improve BMP performance when, as stated on page 3-2, the agricultural privilege tax has already been reduced to the minimum value? Does the District periodically revise the suite of BMPs and the associated point system if continued research indicates that certain BMPs are less effective than originally estimated? If so, are landowners required to modify their BMP practices accordingly to ensure that they continue to meet the "point requirements" of the BMP program?

2. p. 3-20, first paragraph: The fact that conductivity on research farms in the EAA generally meets the state Class III standard does not constitute evidence that the EAA basin is not contributing to elevated conductivity levels in the EPA. Please state how the "background" condition for conductivity was determined. Within the northern EPA, the least impacted region (i.e., "background") with respect to conductivity is the Refuge, which has ambient surface water conductivity near 100 μmhos. WCA-2A to the south has much higher conductivity (near 1000 μmhos) as a result of inputs of canal waters and does not represent the historical "background" condition for the EPA. (See comment #5 in Chapter 2A and comment # 3 in Chapter 4A.)

3. p 3-20, third paragraph: Is there a report or other publication that can be cited to support the statements made in this paragraph?

4. p. 3-21, final paragraph: The future directions outlined here are vague and highlight a problem with the entire chapter. There is considerable mention of ongoing research, education, and outreach. However, nowhere is information presented to show that landowners have actually attempted to optimize BMP performance since 1996. Similarly, what concrete results have "aggressive education and outreach" produced since the BMP rule became effective? If basin load reductions are being met and there are no further regulatory requirements or economic incentives for landowners to reduce loads at the farm scale, it seems that continued research, education, and outreach are a waste of resources. Clear documentation needs to be provided that these efforts continue to produce the tangible result of continued reductions in P runoff at the farm level. As commented in the *2002 ECR*, further analysis of individual sub-basin phosphorus data might provide useful information with respect to reducing phosphorus discharge from "hot spots". In Appendix 1-2-7 of the *2002 ECR*, the response was that "there is no direct statistical relationship established between the farm-level data and the EAA Basin-data". In the draft *2003 ECR*, it is argued that this is, in part, because of water recycling within the EAA basin. It should also be noted that the presence of "hot-spots" from permit-level data can drive the statistical non-significance cited above. The *2003 ECR* should include the

spatially explicit permit level load and concentration maps presented in the *2002 ECR* (Figures 3-9, 3-10).

Chapter 4A: STA Performance and Compliance

1. It would be easier to evaluate and compare STA operations if information provided in text in the subsections titled "Total Phosphorus" was presented in an integrated table or tables showing total inflow, total outflow, inflow flow-weighted mean TP concentration, etc. for all of the STAs.

2. The sub-sections titled "Non-Phosphorus Parameters" actually present phosphorus data for orthophosphorus and TDP. These sub-sections might be more appropriately titled "Other Water Quality Parameters."

3. In table 4A-3, the Class III Criterion for specific conductivity is listed as "Not greater than 50% of background or greater than 1,275 µmhos/cm." This should say "Not greater than 50% of background or greater than 1,275 µmhos/cm, *whichever is greater*." Because the value of 1275 is far above typical interior levels, this standard is not protective of the Refuge ecosystem (see comment # 5 in Chapter 2A and comment # 2 in Chapter 3).

4. Table 4A-4 and similar tables in the chapter is titled "Summary of annual arithmetic averages and flow-weighted means for all parameters other than total phosphorus monitored in STA-1W." Are these averages of values measured "in" the STA or measured in the STA effluent? Is this, in fact, "all" of the other parameters measured, or a selection of the parameters? Specifically, do you also measure effluent calcium, color, TKN, dissolved TKN, mercury, methylmercury, nitrate, nitrite, total reactive phosphorus, potassium, sodium, TDS, and TSS?

5. When specific conductance (conductivity) is reported, you should in at least in one place in the chapter state whether this is a lab or field measurement, and state that this is (or is not) temperature compensated. (See comment # 13 in Chapter 2A.)

6. Atrazine is a water-soluble herbicide that selectively controls broadleaf weeds in agriculture fields around the country. It is said to be the most widely used herbicide in the world. Atrazine is of particular economic importance to sugar farmers in South Florida. Atrazine is also widely used for other agricultural purposes and for weed control in lawns. Atrazine may also be discharged from point sources such as sugar mills (Chung et al. 1996). The levels of atrazine measured at the STA outfalls are of concern to us. The increase in exceedances (reported in chapter 2A) this year heightens this concern. Although atrazine is relatively recalcitrant, it can be mineralized in wetlands (Chung et al. 1996) and removed by constructed wetland systems (Alvord and Kadlec 1996). Because of its wide distribution within the EPA, and current controversy and uncertainty about effects, we urge the continued monitoring of atrazine at STA inflow and discharge sites. Is atrazine used in STA

vegetation management? (See comment # 13 below. Also see comment # 2 in chapter 2A.)

7. As with the *2002 ECR*, the draft 2003 ECR "does not provide performance and compliance information within the context of impacts to downstream biota and potential risks to wildlife from MeHg production" (Peer Review Panel Final Report, App. 1-1-22, *2002 ECR*), inasmuch as "STA performance could be given a context by referring to the condition of downstream biota and the potential impacts from MeHg production." (Peer Review Panel Final Report, App. 1-1-22, *2002 ECR*)

8. The downstream monitoring transect(s) for STA-1E should be presented. We would hope that the downstream monitoring in the Refuge be by the STA-1E discharge (as is the case for STA-2) and not a significant distance away from the discharge structures (as is the case for STA-1W; see comment below).

9. The downstream monitoring stations for STA-1W (Transects X, Y, and Z; Fig. 4A-7) are inadequate to examine potential effects on the Refuge. Downstream monitoring stations from other STAs (STA-2, Fig. 4A-13; STA-5, Fig. 4A-21) are sited immediately adjacent to the STA discharge points. The X, Y, Z transects were initiated for a nutrient gradient study and not for STA discharge monitoring for DEP permitting purposes. As there is no distinct berm/levee, there are multiple entry points where water can enter the Refuge along the L-7 canal between the STA-1W discharge and the X, Y, and Z transects (see attached Figure). New monitoring transects should be considered near STA-1W (in addition to maintaining the X, Y, Z transects for comparability), especially as the operation goals of STA-1W have changed since the ENR project was initiated.

10. Figure 4A-7 needs to be edited. First, it is the "A.R.M. Loxahatchee National Wildlife Refuge" and not just "Loxahatchee …". This was correctly identified in the figure caption, but not in the figure itself. Second, the EPA boundary drawn in this figure only includes WCA-1A, and not the entire Refuge as defined in the EFA and the Consent Decree (see comment # 1 in Chapter 1 and attached Figure).

11. For clarification, are there downstream monitoring transects for STA-3/4 planned? STA-6 downstream monitoring occurs where?

12. For several STAs, there are instances in WY02 where phosphorus outflow concentrations were greater than inflow concentrations (e.g., STA-2, Mar/Apr 2002, Fig. 4A-12; STA-5, Dec 2001 /Jan 2002, Fig. 4A-19). Is this driven by higher inflow loads/concentrations in prior months (i.e., a retention time issue)? Text should be included to discuss these occurrences. If a retention time function drives this statistical result, is there a better way to present these data (perhaps present data on retention times within the STAs)?

13. The Everglades Forever Act requires reporting of information regarding efforts to remove "undesirable" vegetation (as cited on pages 4A-6, 4A-20, 4A-34, 4A-48 of

the draft *2003 ECR*). Did any of this involve removing exotics being considered for use in STAs (e.g., *Hydrilla* removal from STC-4 and STC-9, page 4C-7)? If so, a discussion on where and why *Hydrilla* is being treated in some areas and not in others is suggested.

14. The Corps and SFWMD have constructed additional pumps and a stormwater reservoir (S-332B) in the C-111 Basin. Water quality data has been collected. No mention of this structure or their performance has been reported here (or Chapters 8A or 8B).

References Cited:

Alvord, H. H., and Kadlec, R. H. (1996). "Atrazine fate and transport in the Des Plaines wetlands." *Ecol. Model.*, 90(1), 97-107.

Chung, K., Ro, K. S., and Roy, D. (1996). "Fate and enhancement of atrazine biotransformation in anaerobic wetland sediment." *Water Research*, 30(2), 341-346.

Chapter 4B: STA Optimization

1. The failure to post Appendix 4B-1 makes a definitive review of Chapter 4B impossible.

2. Lab results from part of this STA optimization research has been challenged by FDEP, as the lab reported higher means (and levels of variance) than other labs in split samples from STA optimization research. {Examples of discussion include an 8 January 2001 laboratory audit letter to lab from FDEP; discussion at Biological Technology Workgroup Meeting 16 May 2001; discussion at 21 May 2001 TOC meeting). If results from this lab are included in the presentation of the STA optimization information in this chapter, they need to either be removed, or clearly identified. If these lab results were not considered in the presentation of this chapter, then for clarification, discuss this (as lab QA/QC is discussed in Chapter 2A). We are pleased that this lab has made changes such that it currently does well in the Everglades Round Robin sampling (e.g., the recent ERR TP-11); however, the issue of presentation/interpretation of previous data needs to be addressed in the *2003 ECR*.

3. The duration of the individual experiments was insufficient to even begin to remove the effects of the previous experiment. The duration of individual experiments (e.g., Table 4B-1) needs to be reported. Were there “stabilization” periods between experiments? With mean HRTs ranging from 21.9 – 49.7 days in control test cells (Tables 4B-2 and 4B-3), it is unlikely that the duration of one particular experiment was sufficient to even begin to remove the effects from the previous experiment. In the *2002 ECR*, the authors discuss a statistical manipulation to address concern that the first two low-HLR experiments were of too short a duration (Page 4B-12, *2002 ECR*); however, this approach prevented a complete analysis of both experiments. Are these efforts being re-examined or repeated to avoid the problems of interpretation?

4. Difficulties with flow measurement are clearly demonstrated in this chapter. Tracer discharge measurements (Kilpatrick and Cobb 1985; Wilson et al. 1986) might prove to be a valuable technique for calibration and testing of structure flow equations. Uncalibrated structure flow equations are not reliable.

5. In the calculation of water budgets, what is “error” and how is it calculated. In order to calculate meaningful water budgets or apply models (DMSTA for example) you must adjust the water budget to conserve water mass. This is very commonly done. Presenting only an unbalanced water budget is not adequate for further analysis.

6. You need to also present mass budgets for conservatives and near conservatives such as chlorides, sulfate, TDS or conductivity as a TDS surrogate, and atrazine. It is common to use such mass calculations to test and help adjust water balance. This comment is repeated from previous years.

7. The high error in STA-1W cell 3 suggests that the water balances presented here are not as accurate as needed to support research, or compliance monitoring.

8. It is troubling that this chapter reports the third year of increased outflow concentration from STA-1W (see comment # 2a in Chapter 8A). This is particularly true because cell 5 received significant flow for the first time during this reporting year. In spite of this expansion, the STA performance appears to be deteriorating.

9. It is essential to report not only the flow and mass of P flowing into each STA, but also to report flow and P load bypassing each STA over the reporting year. STA performance cannot be evaluated in an unbiased way without this information. We again request that bypass loads and flows be reported.

10. Pulsing would, in theory, have different effects on STA efficiency depending on frequency. Pulses that are roughly of duration equal to the HRT would approximate batch operation and could approach the efficiency of plug flow. Pulsed operation could increase or decrease STA efficiency depending on frequency (Behrends et al. 2001). This is similar to the theory of sequenced batch reactors (SBRs) used in other treatment systems.

11. The delay in instrumentation of STA-2 is regrettable (page 4B-18). Data on the development of STA-2 might have provided valuable information on STA startup.

12. Are the April 1 2001 tracer studies described in the *2002 ECR* (Table 2B-1, page 4B-11, *2002 ECR*) the same as the July 24, 2001 studies described in Table 4B-1 (page 4B-9) of the draft *2003 ECR*?

13. On page 4B-14, tracer curves with $n > 4$ are referred to as tending toward plug flow. The case of $n = 4$ is actually quite far from plug flow. Curves in Fig. 4B-6 are far from ideal plug flow.

14. Comments from our technical review of the *2002 ECR* are still adequate for the draft *2003 ECR*: "In general, the amount of information and synthesis presented on STA optimization experiments is too small, and does not provide the reader with enough information to evaluate the results. We recognize the need for brevity, but one short paragraph each for various HLR experiments, for example, is not enough detail to be useful. Also, statements of results were made without accompanying data (figures or tables). It would be very helpful if SFWMD worked toward publishing these types of data in independent, peer-reviewed journals. Such publication would ensure the quality and independence of the science being conducted, and would decrease the need to put experimental details and data in the Consolidated Report."

15. We applaud the incorporation of descriptive information on submerged aquatic vegetation community structure in STA-5 (Table 4B-6) and STA-6 (page 4B-28) as might relate to STA optimization. Are there general comments that can be made about an STA cell's biological community and observations on P removal? There are brief notes about how the environmental conditions within STA-5 and STA-6 influenced plant communities, but no mention of how these plant communities potentially influenced nutrient levels. To match up with Chapter 4A, we suggest including information on plant communities in the other STAs. Additionally, those species that are exotics (e.g., *Hydrilla*) should be identified here as it relates to community composition vs. STA performance and any potential EFA permitting requirements to remove "undesirable" vegetation. (See comment # 13 in Chapter 4A.)

16. As asked in previous years, are there plans for dealing with exotic aquatic plant invasions? What is the submerged aquatic vegetation management plan for STA-1E (active/passive management?), and will the Refuge expect to receive fragments of *Hydrilla* from STA-1E discharge? Have plans for STA-3/4 been identified? (See comment # 13 in Chapter 4A.)

17. The last line on page 4B-1 should read "… Water Conservation Area 1 (WCA-1), which is a part of the Arthur R. Marshall Loxahatchee National Wildlife Refuge." This is because the Refuge includes more land than simply WCA-1. (See comment # 1 in Chapter 1).

18. References for this section are missing and cannot be examined. Are those listed in Chapter 4C (pages 4C-63 to 4C-67) for all of Chapter 4. If so, then Chapters 4A and 4B do not function as "stand-alone" products.

<u>References Cited:</u>

Behrends, L., Houke, L., Bailey, E., Jansen, P., and Brown, D. S. (2001). "Reciprocating constructed wetlands for treating industrial, municipal and agricultural wastewater." *Water Science & Technology*, 44(11-12), 399-405.

Kilpatrick, F. A., and Cobb, E. D. (1985). "Measurement of discharge using tracers." Applications of Hydraulics, Book 3, Chapter A16, U.S. Geological Survey.

Wilson, J. F. J., Cobb, E. D., and Kilpatrick, F. A. (1986). "Fluorometric procedures for dye tracing." Applications of Hydraulics, Book 3, Chapter A12, U.S. Geological Survey.

Chapter 4C: Advanced Treatment Technologies

Concerns about this chapter mirror some of the concerns mentioned later for Chapter 6. Much research has been conducted by the District investigating various technologies. However, there is not enough detail or data presented in this chapter to allow a reader to perform sufficient independent evaluation of the District's interpretation. This concern is similar to concerns that DOI has expressed about this chapter in previous years.

The greatest concern is that very little discussion is provided subsequent to presentation of results. It is important that District staff provide their interpretations of the results, and discuss the potential implications to meeting the requirements of the Consent Decree and the Everglades Forever Act. Quoting from Chapter 1 of the Introduction, "Information is this Report will be used by the District and the FDEP for making decisions affecting implementation of the Everglades Construction Project (ECP) and other restoration and management activities." In order to provide the very best scientific foundation for decision makers, District scientists and engineers should use the ECR to discuss potential implications and relevance of scientific studies to the decision-making process.

Using two examples to illustrate this point (there are others in the chapter): 1) A statement is made on p. 4C-8 that average P mass removal rate and storage were significantly higher in north (post-BMP) than south (post-STA) test cells. A table is presented with summary data, but the entire presentation ends there. What is the significance of this pattern? What could it mean regarding the performance of SAV? What are the effects of varying HLR? What should a resource manager know from this type of information? 2) On p. 4C-60, it is discussed that combined CTSS/SAV discharge from a downstream SAV-dominated wetland results in elevated chloride and total aluminum concentrations to receiving waters, as compared to the inflow to the CTSS facility. Are there potential impacts of such discharges? Could such chloride and aluminum discharges result in changes to Everglades flora or fauna, despite reductions in P? Should a resource manager consider these potential impacts, or are the chances of such impacts remote? These are the types of discussion issues that should be included in a report as important as the ECR.

The bottom line is that this chapter, and all chapters in the ECR, should stand alone on their presentation of relevant data, their scientific rigor, their linkage of science to management. If there is not enough detail presented to allow readers to assess the validity of statement made, the report does not accomplish its goal to "update and summarize available data" (p. 1-15). If the linkage to management is absent, managers are left to make their own assessments in technical fields that possibly are outside of their training and expertise.

1. The draft *2003 ECR* does not provide a summary of ATT cost comparisons (note: this was not in the *2002 ECR* as only 2 STSOCs were completed by then. STSOC data for SAV and PSTAs are presented in the draft *2003 ECR*, but the reader has to go digging in the *2002 ECR* for CTSS costs. An examination of the relative cost comparisons for all ATTs should be presented together in tabular form to assist the reader and decision makers in the evaluation of ATTs.

2. p. 4C-1: Eight technologies have been examined, but only are presented in Table 4C-1 (p. 4C-2).

3. p. 4C-8, Table 4C-3: Why were these periods of study selected? What are the implications of these results?

4. p. 4C-9: "It appears that outflow concentrations (in northern test cells) decreased with increasing inflow TP concentrations (Table 4C-5)." This is not clear from the table as the first two time periods overlap (8/4/00 – 6/1/01 and 6/1/00 – 9/14/01). The second and third time period does not overlap and there was a decrease in inflow TP concentration (101.1 to 50.0 μg/L), but an increase in outflow TP concentration for both test cells. As these are the same test cells over time, you cannot look back in time to reverse the statement to state that as inflow TP increases, outflow TP decreases.

5. p. 4C-10, last paragraph: Where are the data to support these statements?

6. p. 4C-11: How does a reduced hydraulic efficiency in the "improved" cell 4 influence its performance? What are the results of the March 2002 tracer study in cell 4 mentioned on page 4C-51?

7. p. 4C-11, sediment stability: Need details on the experimental protocol.

8. p. 4C-11, sediment-water column P fluxes: Data? Same comment for next section.

9. p. 4C-12, cell 5 SAV monitoring: What are the implications of the statements made in the second paragraph?

10. p. 4C-17: First line states 8 constants to be calibrated in PMSAV. Table 4C-9 lists only 7.

11. p. 4C-19: How sensitive are the hydraulic model coefficients (original tracer study before modifying cell 4 in 2000 demonstrated a 51% bypass, model calibration at 44% bypass) in determining TP concentration data? The constant "c" (introduced in Table 4C-8) is not adequately defined to the reader to understand the sensitivity of this constant (in Table 4C-10). It is not clear to the reader why decreasing the goodness of fit in the hydraulic model components from an $r^2 = 0.99$ to $r^2 = 0.70$ causes a significant increase in goodness of fit on TP concentration when the degree

of short-circuiting decreases. None of the P removal data from the cell 4 calibration data set were measured under conditions of low bypass.

12. Table 4C-9: For clarity, label the post-BMP values column as coming from the NTC-15 data set and the post-STA values column as coming from the full cell 4 data set.

13. p. 4C-24: Following all the descriptions of PMSAV, there should be a discussion of how it is being used, or how it will be used in the future.

14. p. 4C-24, third bullet: There are no data, especially ecological data, to evaluate compatibility of the treated water with natural populations of aquatic flora and fauna. The toxicity protocols that were used have been judged by EPA experts to be inconclusive. For example, they were concerned that Everglades water was not used for control treatments. If the toxicity findings (without data) presented in this chapter were derived from studies with improved design, those improvements should be presented explicitly. If no new studies were conducted since the EPA review, this chapter should address EPA's and others' concerns about design limitations.

15. Define "POR" the first time it is used on page 4C-25 instead of on page 4C-36.

16. p. 4C-26: It would be interesting to describe the STA footprint size calculated utilizing current levels of hydraulic bypass (e.g., cell 4 data used for the PMSAV simulations to generate footprint size has significant bypass (51% before "improvement"; ??% after "improvement" {but TIS # dropped from 1.3 to 1.1, page 4C-10}). This would give the reader a better idea about the sensitivity of the PMSAV model in generating these projections.

17. p. 4C-26, feasibility and functionality of full-scale design: What about cell 5 of STA-1W? Hasn't its performance been worse than cell 4? To be balanced and unbiased, that discussion should be presented.

18. p. 4C-26: Add the parenthetical note, "(SAV species specific)" at the end of the sentence, "… the recovery period of the SAV community in the mesocosm study was at least four to six weeks."

19. p. 4C-27: For the "Research on natural SAV systems", consider including work being conducted by the Okeechobee Department. Karl Havens has been examining a data set relating water quality data (including P) to the presence/absence of SAV.

20. p. 4C-27. SAV in lakes: It is not clear how relevant these studies are to SAV in shallow wetlands. The author should do a better job of making that relevance clear. It seems that although correlations can be drawn between P removal and SAV presence in lakes, caution should be exercised in attributing that removal to SAV without supplemental data from cause-and-effect experimental research. For comparative purposes, provide a list of SAV species currently found in existing STAs at the end of the last paragraph.

21. p. 4C-29, last three lines: Incomplete sentence.

22. p. 4C-36: What were the criteria for selection of OPP data? Why were only optimum data used?

23. p. 4C-37: Eight coefficients are listed here, not seven.

24. p. 4C-38: Figure 4C-10 too small to read easily (i.e., difficult to read axes, etc.) Present figure in landscape.

25. p. 4C-41: Table 4C-13 presents only six of the coefficients in the sensitivity analysis (and not in the order listed in page 4C-37).

26. p. 4C-45: *Ceriodaphnia* is misspelled.

27. p. 4C-46: Include the cost per lb of TP removed.

28. p. 4C-50: Display the stage in the shaded areas when water levels are below 11.7 ft. NGVD in Figure 4C-13 to give the reader an ideas as to the extent of the low water conditions in addition to the frequency.

29. p. 4C-52: There appears to be a relatively low level of biological sampling in the PSTA field-scale project (cover monthly, everything else quarterly) shown in Table 4C-20. Are there differences in TP concentrations where the ½ measurements are made relative to inflow and outflow concentrations? It may be difficult to interpret what drives the inflow-outflow TP gradient if periphyton cover at the ½ point is the only biological parameter being measured on a monthly basis when TP samples are collected.

30. p. 4C-53: Given the ongoing high leakage rates, isn't if difficult to use these cells to assess P removal? Because leakage and potential seepage back into the cells cannot be quantified easily, doesn't that make attribution of P removal to the biological community difficult? At the very minimum, these uncertainties should be discussed. Also, the potential management and feasibility implications of such leakage at a full-scale PSTA facility must be addressed.

31. p. 4C-56, CTSS: The statement "Bioassay and algal growth potential studies demonstrated no significant impact on receiving waters…" is not valid and should be removed for reasons presented above (comments referring to p. 4C-24). Again, these studies were inconclusive. The second phrase of the same sentence, "…and residual solids proved to be nonhazardous for disposal," is even more troublesome. This statement must refer to a small study performed earlier by a District contractor where sludge was applied to plots where corn was grown. No data were collected to assess potential ecological effects, and no assessment was made of potential leaching from the sludge application. It is well known that sludge from CTSS operations contains

high concentrations of elements not found normally in soils or sediments. Unless a more comprehensive subsequent study was done (and if it was, it should be cited or data presented), this phrase should be removed.

32. p. 4C-56, background, C-11 West: It would be useful to have some assessment of the potential differences in water quality between the Wellington Basin and the C-11 Basin. Those data are available, and an easy assessment could be made.

33. p. 4C-56, last paragraph: Define "events."

34. p. 4C-57: Again, same comments as before regarding bioassay tests. Were these new studies that took EPA's comments into consideration? Was Everglades water used as the control water? If so, these important points should be included. Otherwise, it is likely that these results are inconclusive.

    And, to repeat a drum well-beaten (see DOI comments on the *2001* and *2002 ECR*s), no ecological work has been conducted to assess the potential impacts of CTSS discharges on native Everglades flora or fauna.

35. p. 4C-57. Refer to the genus name *Cyprinella* the first time *C. leedsi* is mentioned.

36. p. 4C-57: Where is Table 4C-23?

37. p. 4C-58, water residuals: Data?

38. p. 4C-58: What is the estimated cost per lb TP removed for the 70 MGD facility?

39. p. 4C-60: What are the potential implications of increased chloride and total aluminum concentrations to native Everglades flora and fauna?

40. p. 4C-61: Figure 4C-16 missing units on axis legend.

41. p. 4C-62: The range of P values for a given table cell (Tables 4C-24 and 4C-25) are presented as max – min and not "min/max" as labeled.

Chapter 5: Development of a Numeric Phosphorus Criterion for the Everglades Protection Area

Chapter 5 presents a concise overall summary of the criterion development.

1. The demonstration that the 10 ppb criterion is supported by a variety of approaches is particularly effective. The 75th percentile reference site approach (Figure 5-2), based upon a precedent established by the EPA, provides an independent rationale for selecting one particular number for the criterion, given the range of concentrations over which biological responses are observed experimentally and along the WCA-2A gradient.

2. The point is repeatedly made that the criterion is based upon the "long-term" average annual geometric mean. It is likely that marsh stations with long-term values above the criterion (impacted sites) will occasionally have yearly values below the criterion. The measurement methodology (p 5-3 & 5-24) specifies an automatic pass in any year when the GM is below 10 ppb, regardless of the 5-year history. This is inconsistent with the assumption that the criterion and biological impacts are driven by the long-term mean. This feature weakens that test and will cause impacted sites to bounce in and out of compliance from one year to the next as a result of natural variability. If we accept the implied faulty logic that the yearly value should override the 5-year value in classifying a site as un-impacted, then the yearly value should also override the 5-year value in classifying a site as impacted. This leads to the conclusion that the 1-year maximum limit should be 10 ppb instead of 15 ppb. This test would be fair, but not allow for natural variability. The best solution is to reject the faulty logic and strike the 1-year pass provision altogether (paragraph 1 of the measurement methodology description). A station would fail if the 1-year is above 15 ppb or the 5-year is above 10 ppb - period.

3. There is a typographic error in the equation on page 5-20. "t (0.5,n-4.1)" is presumably intended to be t "(0.05, n-1)". Also, this would be a 1-tailed t.

4. The 1-year limit of 15 ppb is described as the "the upper 95^{th} percentile of the long-term annual geometric means". The basis for using the upper 95th percentile as a compliance limit is not specified. Methodologies for determining phosphorus compliance under the State/Federal Consent Decree use the upper 90th percentile (Refuge Marsh P Levels, ENP Inflow Limits). Methodologies for determining compliance with phosphorus load reduction requirements under the EAA Regulatory Rule and C139 Regulatory Rule also use the 90^{th} percentile. The EPA uses the upper 75th percentile of reference sites to set criteria. Using a high percentile here reduces the "Type 1" statistical error (false positive) associated with the test, but increases the "Type 2" error (false negative) and thereby reduces the chance that impacted sites will be detected in the presence of natural variability. Using a 90th percentile instead of the 95th percentile in this case would be consistent the precedent established elsewhere in the Everglades and would reduce the 1-year limit from 15.1 To 13.8 ppb.

5. Figures 5-4 through 5-7 reportedly show the existing marsh monitoring network. This network was not designed to measure compliance with the criterion. It just happens to be there. While maintaining a few historical sites would be useful for tracking long-term trends, historical data would have no relevance for determining compliance in the future. The arbitrarily assumed grid scale automatically determines the amount of impacted area that could occur around the edges of the system without being detected in the network. Even at this course grid scale, the spatial coverage is weak in areas that are likely to be impacted by existing and future discharges. The language at the top of p 5-23 covers these concerns to some extent. The phrase "be generally consistent with" is vague. It should not preclude significant modifications

to the grid scale, the spatial distribution, and the total number of stations.

6. Figure 5-4 does not show the existing transect monitoring stations along the northwest boundary of WCA-2A.

Chapter 6: Hydrology Needs – Effects of Hydrology on the Everglades Protection Area

1. The chapter title is not appropriate given the nature of the contents. As stated in the Summary, the hydrological needs of the Everglades are not discussed in this chapter. It consists largely of literature reviews, and discussion of specific ecological components of the Everglades. In fact, the chapter is more of a description of planned ecological research than anything else.

2. p. 6-1 indicates a "dissection" of hydrologic problems into manageable pieces. This dissection is understandable, but no information is given as to how the choices of "pieces" were made. The authors indicate that the pieces are linked by ecological feedbacks and hydrological dependencies, but do not present these. Is there any sort of top-down approach or conceptual model that presents a broader picture, from which a reader can better understand how the individual parts were selected? If no such approach is presented, readers are left with the impression that no forethought was given to the selection process.

3. Grammatical/style comments: Much of the sentence construction is awkward. Many sentences have phrases inserted near the beginning of the sentences that distract from the main topic of the sentence. Also, we found numerous inconsistencies in how references are listed – some are chronological, some are alphabetical, and some are random. Also, when Latin names are given, the genus name should be given in full the first time, and abbreviations used thereafter. There were instances where the genus name was abbreviated the first time given (e.g., *T. testudinum*, p. 6-4; mangrove spp., p. 6-46; etc). No apostrophes should be used when using the plural of acronyms or dates (e.g., HSIs, not HSI's; 1970s, not 1970's). Lots of switching between SI and English units. There were many other specific comments too numerous to present here. A hand-marked copy of the draft will be provided to the authors for their consideration of many more detailed suggestions.

4. As an example of how editing could clarify the subject matter and shorten the chapter, the following is a suggested revision to the third paragraph on p. 6-31: "All marker horizons deployed at marsh and flooded forest sites were buried over the study period. Burial indicated that deposition was greater than erosion, contributing to accretion of the forest floor. In contrast, markers deployed at dry forest sites either disappeared or were buried slightly."

5. Many of the sections lack data or citations of relevant studies. The text contains many statements that are presumed to be statements of fact – not conjecture – yet no

citations are provided. Therefore, the reader has no way to evaluate and independently judge what is written.

6. Also, some of the sections were organized very differently than others, and there was little consistency of information presented – probably a function of multiple authors and little integration of style and format. For example, the section on p. 6-39 contains a detailed method section, and generally is written in the style of a scientific paper, complete with citations. Most other sections of the chapter lack this content and rigor, which is too bad.

7. Several sections of the chapter seemed out of place, and outside of the scope and objectives of the *ECR*. For example, the section on the CROGEE Florida Bay report contributes little. Readers can be referred to this report in one sentence. This section only summarizes the report, and makes few specific suggestions or responses to it.

8. The section entitled Ecological Trends (p. 6-21) presents few trends and very little data. This section generally is a literature review of several ecological topics. It is not clear why this is included in the *ECR*, and does not match the *ECR*'s objectives. For example, statements are made about lack of improvement in the shift of wading bird colony locations, and in the timing of wood stork nesting. However, no data at all are presented for the reader to make an independent evaluation. The sections on turtles and lizards are particularly weak in reference to hydrological linkages, with only guestimates of potential tree island use provided in the Appendix (A-6-1-2 to A-6-1-11). Finally, strong statements in the section's conclusion are not backed up with any data or references. For example: "It is obvious that the Everglades herpetofauna make up a significant portion of the food web, acting both as predators and prey (p. 6-28)." What constitutes a significant portion? Was the biomass of herpetofauna compared, for example, to fish biomass? What about insects? Even if the statement was supported, what is the relevance to hydrological needs?

9. The *2002 ECR* Review Panel recommended, "that the District focus attention on hydrological temporal and spatial variability so that the importance of variability can be ascertained. Another hydrologic issue deserving attention, and not addressed, is the continues ponding in the southern part of WCAs and the relative dryness of northern WCAs." (App. 1-1-39, *2002 ECR*). We looked forward to a discussion of this in the *2003 ECR*; however, this material was not presented in the draft *2003 ECR*. We suggest that the final chapter include some attention to this.

10. One or more maps would be very helpful to identify locations of sample sites (e.g., stations used to generate data in Table 6-1, etc.). The site names alone frequently are not sufficient for readers to understand where they are located.

11. p. 6-1: What does "consistent with NSM" mean? Instead, would it be more accurate to say that recession rates were consistent with average historical patterns?

12. p. 6-2: The list of ecological studies in the first paragraph under Ecological Trends does not match with what is discussed subsequently.

13. p. 6-4: “formalization” instead of “formalism”?

14. p. 6-5: What is the difference between average and NSM targets? A short explanation within an ecological context would be helpful.

15. p. 6-8: The District did not change the regulation schedule for WCA-2A in the early 1970s to “bring it more in line with NSM”. NSM was not created until the late 1980s.

16. Are standard deviations or ranges available for the data presented in Tables 6-1, 6-2, 6-3, 6-4 that can be presented? This would help the reader assess where WY 2002 falls within the 1970-2002 spectrum.

17. p. 6-12: Use consistent terms, stick with either “marsh” or “wet prairie”.

18. p. 6-19: Figure is poor quality. Axis legends hard to read.

19. Fig. 6-6, bottom panel: It visually appears as though the data point at 53” rainfall and 40 ppt salinity would drive the left end of the regression line higher (and thus reduce the r^2). This analysis needs to be re-checked for accuracy (especially if the regression analysis was run in Excel, known for its problems in statistical algorithms). The label for the 2002 data point is adjacent to 4 data points, which is the correct one? Also, there are only 10 data points in this figure, yet the period of record (1991-2002) has 12 data points. Please clarify.

20. Suggest presenting the regional differences in wading bird nesting efforts (pages 6-12 to 6-22) in tabular format for ease of comparison among years.

21. p. 6-23: Is this study location (southeast Lee County) relevant to CERP?

22. p.6-23: It is not necessary to present R and R^2 data as R values are not often reported, and can easily be generated from R^2 values.

23. p. 6-24: What is the ecological significance of burrow depths? If there is a relationship between burrow depth and water level, the ecological impacts of such a relationship should be presented and discussed.

24. p. 6-30: It is stated that mangrove wetlands need to maintain their intertidal characteristics in the face of sea level rise. If they do not, it is stated that they will disappear. Is it not possible that they may be displaced inland over time, and not disappear?

25. p. 6-30, 1st sentence of last paragraph: check to make sure the sentence makes sense.

26. p. 6-30: Is below-ground production the only mechanism of organic soil formation?

27. p. 6-31: "The burial of the marker horizons indicated a lack of surface erosion and consistent surface deposition at the soil surface…." (paragraph 3). Monitoring only once a year can provide an integrated picture of net accumulation/erosion and is inadequate to determine whether any surface erosion occurred over the year if there was a net accumulation of sediment. Likewise, "consistent surface deposition" over the course of a year is not inferable from a single data point.

28. p. 6-31: What are the shrinking and swelling processes (paragraph 5) referred to?

29. p. 6-31: Where are the data on nutrient inputs (last line on page)?

30. p. 6-32: Check 1st sentence of 1st full paragraph to see if it makes sense. "occurring" instead of "occurred"?

31. p. 6-32: More sweeping statements that ignore other possibilities: "It (marl sediment) was obviously just recently deposited because it was still wet and it formed a litter-free layer almost everywhere." What if it was an existing marl layer from which the overlying litter had been swept away by the storm? Maybe it was moved there from an adjacent location. Most of this section is relatively weak.

32. p. 6-32: Provide the wind strength for Category 2 hurricanes – or provide actual wind strength data if known.

33. p. 6-32, Peat microtopography: Why was the SCT's draft flow paper not cited anywhere in this section? It provides a very thorough reference list that could have been used to support numerous statements made in this chapter that are presently unsupported. The flow paper, although in draft form, has undergone three rounds of internal review and one round of external review. It is at least as authoritative as the manuscripts in press and personal communications that are cited elsewhere in the chapter. In addition, to be consistent with other sections, this section should include a discussion of the negative ecological impacts that result from conversion of ridge/slough landscape to uniform sawgrass. Isn't this chapter supposed to describe linkages between hydrological changes and ecological characteristics of the Everglades? In general, the flow paper could be used to produce an expanded section of this chapter that directly links human-induced hydrological changes to detrimental changes in the Everglades. Why not take advantage of the huge amount of work already put into production of the flow paper? The flow paper (draft) is available on the SCT website at: http://www.sfrestore.org/sct/docs/index.htm

34. p. 6-32: Although underlying bedrock patterns do not match overlying peat elevations, is it possible that bedrock elevations upstream affect peat elevations downstream? For example, the heads of tree islands upstream affect the morphology of tree island tails downstream. In other words, is it possible that because of flow

downstream, that peat elevations downstream reflect, or are at least influenced by, bedrock elevations upstream?

35. p. 6-32: How was the bedrock elevation measured?

36. p. 6-33: Figure numbering should read Fig. 6-10 and 6-11 (last sentence of second full paragraph).

37. p. 6-37, 1st sentence: "It has been hypothesized…" Where was this hypothesis discussed? Sounds like the flow paper again!

38. p. 6-37, 2nd paragraph: What does this paragraph have to do with differentiating ridges from sloughs? Seems like it focuses on nutrient enrichment.

39. p. 6-37, last 2 paragraphs: It is hard to speculate about the effects of faster decomposition without presenting data or trends of primary productivity. It would be the balance between decomposition and production that determines the direction of the carbon balance.

40. p. 6-39, 1st paragraph: Isn't this very long list of citations overkill? Particularly when citations are lacking in so many of the other sections of the chapter?

41. p. 6-39: Other sections would benefit from such a detailed methods section.

42. p. 6-41: Which species are presented in the inset figure of Fig. 6-16 for total biomass? If the data sets for *Eleocharis* and *Rhynchospora* were collapsed together, then this analysis is invalid as there were statistical differences both between species and among treatments for a given species.

43. p. 6-44, Fig. 6-18: How were these data collected? What statistical evidence is there of significant differences between bars? A legend for the X-axis is needed. How many reps were collected? How many sites were sampled? This figure is a good example of how a more rigorous approach to reporting scientific data would greatly improve this chapter.

44. p. 6-45, Table 6-7: n=? Are the last two rows number of leaves per plant? Per m^{-2}?

45. p. 6-45: Where are the data on TN and TP? What are the study transects?

46. p. 6-46: Provide common names for the species mentioned in paragraph 3 (this is done later on p. 6-48).

47. p. 6-46, Fig. 6-19: Better check the match between the figure and the caption.

48. p. 6-47: Fig. 6-20 does show the seasonal production of litterfall; however, there is no indication on the figure which sampling dates were statistically different (or which

months were considered wet or dry season months). Is reproductive litterfall on wet tree islands relatively higher than on flooded tree islands, or does this pattern simply mirror the proportionally higher overall litterfall on wet islands compared to flooded islands (it is hard to tell as the two figures are of different sizes on paper and the spacing between individual bars is not related to the spacing between sampling dates)? If it's just proportional, then why is it "interesting to note"? If not, is there information in the literature as to why this may occur (stressors influence reproductive effort and reproductive output differently for different plant species)? Also, it is important to note in the text that you are looking at the reproductive component <u>in the litterfall</u>. Unless you have direct comparison data (for reproductive structures) between litterfall and what's on the trees, it is difficult to examine why you have may greater actual reproduction effort/output between tree islands under different hydrological regimes.

49. p. 6-48, Fig. 6-21: Hard to read – needs to be larger.

50. p. 6-50, Fig. 6-22: Units for Y axis? What are vertical lines over bars? SE?

51. p. 6-51 to 6-59: In general, there are poor linkages made between the "effects of hydrology" purpose of this chapter and the presentation of remote sensing technologies.

52. p. 6-51: The explanation of what a tree island is seems silly following such a detailed discussion earlier in the chapter. Points to need for editing and integration.

53. p. 6-54, Fig. 6-23: OK, so this is a cool image. What does it tell us? What are its potential applications?

54. p. 55: Last line should be "person-hours" and not "man-hours".

55. p. 6-56: Instead of spending lots of money investigating new remote sensing techniques, why not spend that money to update earlier vegetation maps using established techniques? For example, there is a dire need of a recent cattail map of WCA-2A. This map would be extremely useful in the ongoing P rulemaking process. The last map was from 1995, and showed a dramatic increase in cattail coverage since 1991. A new map is long overdue, and is a more pressing need than incremental (and possibly expensive) improvements in methodology. Also, where are ground-truthing efforts described to determine accuracy of maps derived from these new technologies? These efforts are expensive and time consuming, but absolutely necessary.

56. p. 6-58: The authors should discuss the current postponement of DECOMP. Given the delay, is the NASA collaboration in these regions the best one to pursue? Again, the same comment made previously – cool technology, but it is not clear what is gained by going through all of this in a chapter on hydrological needs. This information would be more relevant in a research plan or an internal budget document.

57. p. 6-59: Again, great stuff, but not consistent with the objectives of the *ECR*.

58. p. 6-60: Briefly mention why the RSM is not being applied to other areas of the EPA (WCA-1, WCA-2, Refuge).

59. p. 6-60 to 6-61: Discuss limitations of the RSM, as you do for HSIs (p. 6-66).

60. p. 6-63: Where are the data collection efforts described that will be essential for model calibration and verification?

61. p. 6-64: Figure 6-28 is not viewable.

62. p. 6-66: As mentioned before for other sections, this section on communicating model uncertainty is out of place in the *ECR*.

63. Heading for "Rhizotrons" section needed on page A-6-1-12.

Chapter 7A: Update on CERP Implementation

1. There are many acronyms and terms in Chapter 7. Since they are not included in the Acronym (A) or Glossary (G) sections, suggest including a section in Chapter 7 for them. For example, terms like "environmental justice" (7A-18) need to be defined for the reader. The revised Monitoring and Assessment Plan has a good glossary that might be incorporated here.

2. RECOVER needs to be defined the first time it is used (7A-2, line 6).

3. Several times the revised implementation schedule is mentioned (7A-2, 7A-18). Suggest adding a copy of the current implementation schedule (even though it gets revised on a near-annual basis).

4. p. 7A-3: "Also, existing animal and plant populations have adapted to some degree to the altered ecosystem." For example ….? Does this mean that those areas should not be restored? What are the implications of this?

5. Fig. 7A-1: Is it intentional that this line decreases before it increases? For some areas we probably will see a change to a less desirable condition before we see the move to a more desirable state. This is not a bad thing! But it is something that the public and decision makers need to be made aware of.

6. p. 7A-5: "The purpose of the PIR is to recommend the most feasible … means of implementing the project." Not best?

7. p. 7A-15: How much acreage for each project? What % of needed acres? (See comment # 1 in Chapter 8C)

8. p. 7A-27: It is the "A.R.M." Loxahatchee National Wildlife Refuge. (See comment # 1 in Chapter 1)

Chapter 7B: RECOVER Activities

1. Caution should be exercised when making blanket comments about the application of the ELM to address water quality modeling needs (e.g., pages 7B-1; 7B-4). The Model Refinement Team of RECOVER is in the middle of an internal review of the ELM to assess its potential use as a tool. There were ~ 8 comments from RECOVER participants, of which at least 3 provided technical comments suggesting that the ELM in its current state should not be used as a tool to address water quality modeling needs. Not only is the MRT in the middle of their review of the model (which will not be completed until after the final draft of *2003 ECR* is completed), but to the best of our knowledge, MRT has not definitively addressed the issue of how many technical concerns need to be raised before they choose to <u>not</u> recommend application of the current version of a model.

2. The third mission area of RECOVER (Planning and Integration) is not clearly defined in Fig. 7B-1.

Chapter 8A: Achieving Long-Term Water Quality Goals

The effort to compile baseline data for each contributing basin, and develop a synthetic time-series of flow and phosphorus concentration characteristic of that basin (Goforth and Piccone 2001) was a difficult but necessary undertaking in support of the planning studies that are currently underway. This effort was largely successful, and we commend the SFWMD and their staff for the timely completion of this phase of planning.

1. We do have concerns about the process of alternative evaluation. The alternatives presented and analyzed in the supporting Basin Specific Feasibility Study do not generally conclude that expansion of the STA area (footprint expansion) will be needed. This optimistic forecast results in large part to the selection of DMSTA parameters. The DMSTA model parameters being used in these studies are based on the best performance that has been historically observed from a single treatment cell in STA-1W (select data from cell 4). This optimism concerning performance of all of the STAs may prove to be justified, but is more likely to not be achievable and increases the risk of failure. We must avoid basing all plans to meet EFA, CWA, and settlement agreement requirements on highly optimistic model projections. There are two actions that are needed:
    a. Because the primary plan is to meet effluent limits that may be very stringent and not easily met using our current understanding of STA design and operation, the SFWMD needs to redouble efforts at STA research and optimization. This research should include optimization with CERP projects to maximize phosphorus removal and load reduction in the STAs.

   b. We need to now develop alternative basin-specific plans that will address and correct phosphorus pollution in the case that the STAs are unable to perform as well as anticipated in the current planning. That is, we need a "plan B" for each basin in order to reduce the likelihood of failure. This alternative planning would likely include expansion of current STAs to larger areas (footprint expansion). We must be careful that current decisions and actions by CERP and others do not preclude implementing this alternative solution, if needed.

2. The footnote in Table 8A-2 needs to include a notation that the DMSTA model simulations were run using the optimistic 2-year "Cell_4" data set. This clarification is important because:
   a. it was not done with the more realistic "NEWS" data set (see http://wwwalker.net/dmsta/index.htm) and the *2003 ECR* clearly documents that this optimistic data set has not been repeated in other cells, and that recent Cell 4 results are much poorer ("However, cell 4 outflow concentrations have increased over the last three years, double that of its optimal period from WY 1998 to WY 1999."{page 4B-8}; Table 4C-1) with no understanding reasons known, ("Reasons for these increased outflow concentrations are not clear but are being investigated." {page 4B-8}). (See comment # 8 in Chapter 4B).
   b. The PMSAV model development (presented from pages 4C-12 to 4C-24) utilized other data sets. For example, "Phosphorus-removal coefficients for the post-STA model were calibrated to STA-1W, cell 4 data from the period of January 1, 1998 through September 30, 2001 (1,348 days)." (page 4C-18).

3. On page 8A-8, for clarification, the following text needs to be added ", using the optimistic 2-year Cell_4 data" after, "The consultants are using the Dynamic Model for Stormwater Treatment Area (DMSTA)" and before, "to evaluate….".

4. The last section of this chapter (Strategy For Long-Term Solutions, page 8A-10) contains only the first paragraph of this section from the *2002 ECR*, and does not identify the potential environmental/economic risks previously reported. Also, in the *2002 ECR*, there were 13 key information gaps that were identified and discussed by category/importance (pages 8A-7 to 8A-8, *2002 ECR*). These were very useful from a decision-making perspective and should be included in the *2003 ECR*. If some gaps were eliminated or are currently being addressed, a discussion is warranted.

5. The Corps and SFWMD have constructed additional pumps and a stormwater reservoir (S-332B) in the C-111 Basin. Water quality data has been collected. No mention of this structure or their performance has been reported here (or in Chapters 4A or 8B).

6. Citations to reports and documents supporting this chapter should be listed in a *Literature Cited* section at the end of the chapter.

7. Check that acronyms (EAA, EFA, FDEP, ….) are defined in the *Acronyms* appendix.

References Cited:
Goforth, G., and Piccone, T. (2001). "Baseline data for the basin-specific feasibility studies to achieve long-term water quality goals for the Everglades.", South Florida Water Management District, West Palm Beach, FL.

Chapter 8B: The Everglades Stormwater Program

1. In the author’s response comments in the *2002 ECR* (App. 1-2-23, *2002 ECR*), they discussed providing ESP basin-specific activity updates in a tabular format for better readability. We looked forward to seeing this in the draft *2003 ECR*; however, the information was not presented in this manner.

2. The SFWMD as required by the Modified Consent Decree and requested by TOC has been collecting dual samples at the C-111 and Coastal Basin inflow points to ENP. One set are the old stations of S-18c, S-332 and S-175 and the other are the new stations S-18c, S-332D, and S-174. This has been done for over a year. It would be beneficial if an analysis of these data was included in this ECR. (See comment # 5 in Chapter 2).

3. The Corps and SFWMD have constructed additional pumps and a stormwater reservoir (S-332B) in the C-111 Basin. Water quality data has been collected. No mention of this structure or their performance has been reported here (or in Chapters 4A or 8A).

4. High phosphorus concentrations and pesticide (endosulfan and its metabolites) concerns at the Non-ECP Discharge Structure S-178 need to be summarized and explained in the main text rather than Appendix 8B-1. We understand that the District is initiating a study of this structure at present using automatic samplers. This needs to be mentioned.

5. Chlorpyrifos and ethion concentrations were also found at C-111 structures. This needs to be discussed. Atrazine and endosulfan were designated as pollutants of "concern" and "potential concern" in Chapter 2A for the Refuge, C-111, and ENP, but were not discussed in here. These also need some discussion.

6. p. 8B-6: There is a discussion of the Refuge headquarters property and a series of farm sites. A map showing the Refuge headquarters, the levee, and the farms would be helpful in understanding the problem at this location.

Chapter 8C: Land Acquisition in Support of Projects in the Everglades Region

1. Chapter author’s response to *2002 ECR* peer review comments, “I would recommend that future reports consider incorporating information on acres to be acquired for the projects, total acres acquired and % acquisition project is complete; and whether

lands acquired are a priority for infrastructure or for watershed management purposes." (App. 1-2-24, *2002 ECR*) were not addressed in the draft *2003 ECR*.

Chapter 8E: Exotic Species in the EPA

1. Consider including pictures (or line drawings?) of the primary exotic species of concern listed on pages 8E-11 to 8E-23. This may enhance public education about exotics, including those that are potentially "common" on private lands (e.g., melaleuca, Brazilian pepper, and Australian pine).

2. p. 8E-16: "The District plans to conduct experimental applications of herbicides on evergreen Everglades tree islands in the Refuge in 2001." This needs to be updated as this was done (pre-treatment baseline done June 2001; sprayed in October 2001; 6-month monitoring in June 2002; 1-year monitoring coming in October 2002).

3. p. 8E-25: "Relatively little work has been done to investigate the ecological impacts of invasive species in the Everglades Protection Area." Two *Lygodium microphyllum* articles should be added:
    - Brandt, L.A. and Black, D.W. (2001) Impacts of the introduced fern, *Lygodium microphyllum*, on the native vegetation of tree islands in the Arthur R. Marshall Loxahatchee National Wildlife Refuge. Florida Scientist. 64(3): 191-196.
    - Darby, C. and McKercher, L.R. (2002) Bones wrapped in *Lygodium microphyllum* rachis suggest a potential problem for wildlife. Wildland Weeds. Fall, 2002. p. 14.

Respectfully submitted,

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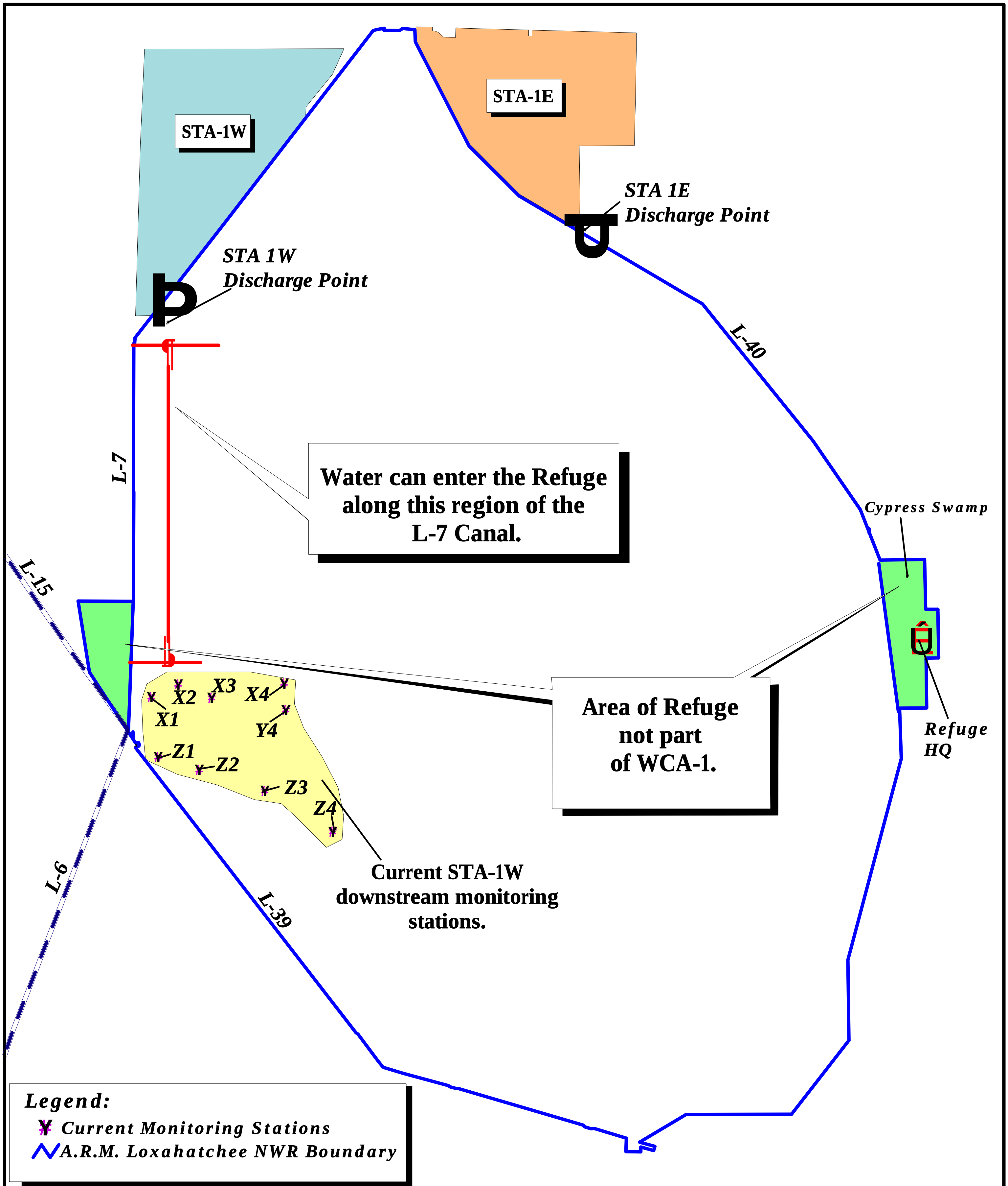

A.R.M. Loxahatchee National Wildlife Refuge