Appendix 1-3: 2003 Everglades Consolidated Report Authors' Responses to Comments

After the public workshop and receipt of the final report from the Peer Review Panel, authors revised their chapters responsively. The documents in this appendix are the author's summaries of their responses to review comments.

Chapter 2A: Reponses to Peer Review and Public Comments

PEER REVIEW PANEL COMMENTS

Comment: *Chapter 2A clearly acknowledges that changes have been made in the way data are analyzed to determine water quality standard compliance. The data analysis method changes in Chapter 2A, in many ways, reflect maturation of water quality monitoring as the Everglades restoration program moves from a strong research orientation toward a more long-term management focus. While the effort to introduce a means to quantify risk in making statements about standard excursion rates are to be lauded, the manner in which the change has been made raises concern (i.e. with what appears to be little exploration of the existing literature on the subject).*

It must be pointed out that the science behind standard compliance determination is not well defined (i.e. there are no widely agreed upon methods). There are many alternative methods available to analyze data to determine standard compliance and there is a lively debate underway on which method is 'best'. Different authors and groups advocate, promote or suggest specific methods for standard compliance determination. In choosing a method for the Everglades water quality management program, the science behind the debate surrounding that method should be summarized with an explanation of why a specific method was chosen over the many others available.

Response: In the interest of brevity, the draft chapter did not fully document the range of considerations and review undertaken by the authors during the development of the new data analysis methodology. The authors did review a wider range of literature than was discussed in the draft. Furthermore, subsequent to the draft the authors have reviewed additional literature recommended by the peer review. In consideration of peer review and public comments the authors have expanded the discussion of this issue to better and more fully document the basis for the change and for selecting the specific method.

Comment: *When a new method is used in preparation of Chapter 2A, is this method also being used in other standard compliance calculations, e.g. BMP and STA performance measurement, within the Everglades restoration program? Was the change in Chapter 2A agreed to by authors of other chapters? If not, how will the difference in standard compliance computations be acknowledged in the report?*

Response: The analysis methodology utilized in Chapter 2A is appropriate for the evaluation of ambient water quality conditions within a water body. Water quality evaluations in the referenced BMP and STA chapters are primarily concerned with permit compliance and determining the level of treatment being achieved by the BMPs and STAs. The calculations used to evaluate the effectiveness of the BMPs and STAs are specified by Rule or in the Permits themselves. Given the different purpose of the BMP and STA evaluations, it would not be appropriate to apply the same methodology as used to evaluate ambient water quality conditions. During the development of the new evaluation methodology utilized in Chapter 2A, the authors did interact with the Department's Standards and Everglades permitting staff to discuss potential consistency issues. No conflicts or inconsistencies were identified.

Comment: *In Chapter 2A, the methods used to obtain data follow strict protocols (e.g. SFWMD Quality Assurance manual and Standard Operating Procedures), but the methods to analyze the data do not appear to be documented in protocols outside Chapter 2A. Is this the case? If so, why are data collection and laboratory methods documented and strictly followed, while the methods for analyzing the data are not documented outside Chapter 2A? Is the annual review of Chapter 2A by the Peer Review Panel the only external review of the data analysis methods employed in Chapter 2A?*

Response: Data acquisition (collection and laboratory) standards are specified by rule (Chapter 62-160, Florida Administrative Code), which largely form the basis of data collection and laboratory protocols. With the exception of the state's Impaired Waters Rule, no rule addressing data analysis protocols exists. The data analysis methods were selected for the purposes of this report; that is, identify water quality parameters, in addition to phosphorus, which exceed state water quality standards or are causing or contributing to adverse impacts in the Everglades Protection Area. At this time the protocol utilized with Chapter 2A is only documented within the Chapter. The annual peer, inter-agency, and public reviews are the only external reviews of the methodology. Unfortunately, as the peer review panel noted, little guidance exists regarding a standard methodology to such determinations. The authors reviewed and utilized where appropriate any existing guidance, including the Impaired Waters Rule, literature, and USEPA documents. More specific guidance is slowly being developed by both state and federal efforts. The authors will endeavor to track all relevant guidance regarding this issue.

Comment: *Did the methodology for evaluating pesticide results include the changes for the other constituents? Why is this the case? Is it because of the small sample size? This apparent exception to the new methodology needs more explanation.*

Response: Because the USEPA 10 percent exceedance frequency guidance only applies to conventional pollutions (e.g., dissolved oxygen, ions, turbidity) the new methodology does not apply to pesticides or constituents of human health concern. The revised chapter provides a more detailed discussion of the basis for these exceptions.

Comment: *The minimum number of samples for beryllium was not available to support the new standard compliance methodology for this water quality constituent, but determination of standard compliance was made anyway. Should the calculations be expanded to include this situation? This same situation appears to occur in Table 2A-2, where, for the WCA- 3 inflow, the excursion analysis was performed with less than 20 samples. Is this the case? The same situation appears to occur in Table 2A-3 for inflow, total silver; interior, total copper and total lead; and rim, total iron.*

Response: The current state criterion for beryllium is based on human health, therefore the revised analysis methodology does not apply and any exceedances of the criterion should be further investigated. The chapter has been revised to clarify this point.

The results for total silver, total copper, and total lead are preliminary and associated with substantial statistical uncertainty. The evaluation methodology was changed slightly to better account for constituents with small sample sizes. If a constituent exhibits an excursion rate of 20% or more it is now preliminarily identified as a Concern for that year regardless of sample size and requires a further evaluation based on a longer time period to obtain a sufficient sample size. The revised chapter presents a refined review of these results based on small sample size.

Comment: *On page 2A-12, a problem noted with the raw score approach used in previous Everglade Consolidated Reports is its inability to account for sample size. Yet, in describing*

application of the new data analysis method, sample size is restricted to a minimum of 20 (page 2A-13). Would a similar sample size restriction make the raw score approach more comparable to the binomial approach? It should be noted that the CALM Guidance document, in Appendix D, suggests a minimum of 28 samples to make a standard compliance determination using the binomial distribution method and concludes that the binomial model is no panacea for inadequate sample size. Thus, it appears that regardless of the data analysis method, employing small sample sizes to assess standard compliance is a problem.

Response: The authors concur with the comment that "the binomial model is no panacea for inadequate sample size." Analysis of a small sample size is problematic regardless of statistical methodology, due to substantial and unacceptable error rates (both type I and II). However, as noted in the revised chapter, due to the high type I error rate associated with the raw score approach, sample size restrictions will not make the raw score comparable to a binomial approach. The revised chapter provides a more thorough discussion of sample size considerations and adopts the CALM recommended minimum sample size.

Comment: *Are the data screened for statistical outliers, even though outliers are not excluded?*

Response: Because the chapter and data analysis methodology utilized robust non-parametric statistics, which are largely unaffected by outliers, no statistical outlier screening was conducted. Furthermore, the authors do not believe it is appropriate to censor data merely because these data lay at the tail ends of the distribution as these extreme values may represent real natural or anthropogenic influences. Part of the QA/QC data screening does involve a review of the field and laboratory notes for information that could indicate that the sample or results do not accurately represent ambient conditions.

Comment: *Was the impact of employing an arithmetic mean to be the one observation for multiple samples, acquired in one day, on the variance of the observation, compared to other single observations, evaluated?*

Response: Averaging data acquired in one day helps insure data independence and avoids biasing results towards dates with greater sample replication. Furthermore, the analysis presented with the chapter is primarily concerned with spatial and temporal variance components. Sample variance is largely a quality assurance issue (i.e., replicate error).

Comment: *How are missing values in the data record handled? Is there concern for a string of missing values in a key season biasing the results?*

Response: Since samples tend to be fairly evenly distributed between the seasons (wet and dry) there is little concern for potential bias. The analysis is chiefly concerned with describing water quality conditions when the marsh is flooded (wet). The primary cause for a string of missing values during an extend period (season) would be low or no water. Data for the remaining portion of the year would still be representative of the period in which the marsh was wet. The more in depth data analysis performed to support the discussion of the WY02 results does include a review of the temporal distribution of data collection and water quality criteria excursions. This detailed review would identify any seasonal bias, which would be noted in the report.

Comment: *Are references to support the decisions for preparing data records for statistical analysis provided? More generally, are the methods for screening data documented? Given the changes in data screening this year, how are future changes to data screening methods considered and approved before incorporating them into the results of the report?*

Response: Data screening methods have largely remained unchanged from year to year, however, based on comments received in previous years, a concerted effort was made in the 2003 ECR to better document these screening methods. Accuracy and precision limits are established in quality assurance plans based on the Department's Quality Assurance Rule (Chapter 62-160, F.A.C.), NELAC (The National Environmental Laboratory Accreditation Conference) guidelines, and Department of Health approval. Guidance documents do exist and were reviewed including:

FDEP. 2000. A Tiered Approach to Data Quality Assessment (DEP EAS 00-01, October 2000).

FDEP. 2001. Data Quality Assessment Elements for Identification of Impaired Surface Waters. (DEP EAS 01-01, April 2001).

Any future changes in data screening methods will be considered based on changes in either rules or guidance documents. We agree that standardize data analysis protocols should be developed and adhered to in the future.

U.S. DEPARTMENT OF THE INTERIOR

Comment: *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 both 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.*

Response: All maps depicting the A.R.M. Loxahatchee NWR presented within Chapter 2A have been updated to include the full spatial extent of the Refuge.

Comment: *In general, we are concerned that this chapter gives too much credibility to the Florida Class III Criteria. Why is anti-degradation of outstanding Florida Waters (OFWs) not equally considered for those water bodies that are designated OFW?*

Response: This report was developed in accordance with the Everglades Forever Act, which establishes the purpose of this chapter as the identification of water quality parameters, in addition to phosphorus, which exceed state water quality standards or are causing or contributing to adverse impacts in the Everglades Protection Area. In addition to Florida Class III criteria numerous other parameters (nitrogen, sulfate, and pesticides) of potential management concern are evaluated within the chapter. Anti-degradation and OFW determinations require more exhaustive evaluations than are possible within this chapter and are beyond the scope of the ECR. Furthermore, both these evaluations require the establishment of baseline conditions, which have not been determined for most water quality constituents.

Comment: *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 <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 >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.

Response: The authors have continued to note over at least the last three reports that the Refuge is naturally a soft-water system where current Class III alkalinity and conductivity are inappropriate. We also agree that in order to maintain the unique character of the Refuge this soft-water condition must be protected. It is not yet clear whether derivation of a SSAC is the most appropriate means to recognize these facts. Regardless of the means utilized to deal with this issue, baseline conditions will need to be developed. At a minimum these baseline conditions will need to be agreed upon by the Department, District and Department of the Interior. It may be appropriate to review this issue with the TOC principals.

The development of DO SSAC should not be viewed as lower of standards. It is being developed to allow differentiation between natural background DO conditions and areas with impaired DO function. Because the current criterion is clearly inappropriate for the Everglades marsh, it cannot provide such delineation and is thus not protective of the system. An appropriate SSAC will provide a management tool and an enforceable standard.

Comment: *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. 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.*

Response: We agree that monitoring of atrazine at STA inflow and discharge sites should continue; in fact, Weaver (2001) recommended a review of sources and sinks within the watershed to facilitate a thorough risk assessment. The authors and Department are aware of the recent USEP atrazine guidance. USEPA recently concluded that a four-day average atrazine concentration of 12 µg/L would be protective of freshwater animals and should also be protective of most freshwater plants (USEPA 2001). It further found that cyanobacteria had EC50 values for various exposure durations of 30 µg/L or greater, while EC50 values for green algae, diatoms and cryptomonads were 15 µg/L. An initial review of the available guidance does not seem to support the idea that Everglades periphyton or macrophyte communities are especially sensitive to atrazine. When the draft USEPA guidance is finalized the Department will consider including atrazine in its triennial review process. Consideration of particularly sensitive communities will be a component of that process, if the available information warrants such an evaluation. Input into this standard setting exercise, including additional directed research, from the Department of Interior will be welcomed.

Comment: *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.

Response: The review appears to be primarily concerned with type II statistical error (probability of false negative result). Using the binomial statistical approach of this chapter, type II error rates can be managed at acceptable levels at sample sizes of at least 28 values. It should be noted that the previously utilized raw-score test did not provide adequate control of type II error rates at small sample sizes. Using either test, the type II error rate is primarily controlled by the pervasiveness of exceedances and the sampling programs probability of detecting these exceedances, which is a function of sampling frequency. Furthermore, type I errors are un-acceptably high for the raw-score approach regardless of sample size. The comment also ignores the fact that the analysis methodology presents a tiered approach to categorize parameters. Given the tiered approach parameters with excursions not categorized as concern are likely categorized as potential concern. The system can be thought of as a report card with passing (A-B), satisfactory (C), unsatisfactory (D), and failing (F) grades. Not only does it provide the public and decision makes with a measure of the overall water quality of the Everglades, but it also guides and prioritizes further review. There is sufficient information to warrant further evaluation of parameters characterized as either concern or potential concern. The revised chapter provides a better justification for the appropriateness of these statistical methods.

The uncertainty principal proposed by the reviewer must also consider risk and cost. When the risks to human health and the ecosystem are known to be high (e.g., carcinogens, potentially acutely toxic compounds) then there is a need exercise greater caution. This evaluation of risk is the primary justification for not using a frequency based approach for pesticides and parameters with human health based criteria. Furthermore, "tracking" down and attributing causes to every single excursion within the system regardless of frequency comes at the cost of time, resources, and at the expense of in-depth evaluations of more pervasive problems. Although the authors do not completely agree with the reviewers' view of the protective principle the final chapter uses a refined excursion analysis methodology, which exercises greater caution when uncertainty is high (i.e., small sample sizes). This refined methodology did not alter any of the chapter's conclusions.

Comment: *The maximum specific conductance (conductivity) reported for the Refuge interior was 3686 µmhos/cm. This observation is clearly beyond the bounds of values anticipated and leads to questioning the procedures used in data quality assurance.*

Response: The value in question occurred at station LOX15 on November 6, 2001. This station is near the S10C structure and Hillsboro canal and therefore may periodically be influenced by high ionic strength canal water. An initial and subsequent data quality review found no QA/QC qualifiers or flags associated with this value. However, based on this recommendation by Refuge staff the datum was deemed beyond reasonable natural values and was excluded (screened out) from the analysis. The datum likely represents a data transcription error with the correct value possibly being 368.6 µmhos/cm.

Comment: *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.

Response: The more complex Everglades DO response referenced by the reviewer is discussed within a proceeding paragraph. This discussion clearing indicates that Everglades marsh waters tend to naturally exhibit DO levels below 5 mg/L. It further explains areas impacted by P-enrichment exhibit DO levels reduced substantially below the natural Everglades level. The paragraph has been modified to clarity the mean of "low" (i.e., < 5 mg/L).

Comment: *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.*

Response: The Department is very interested in the input from the Department of Interior on any revised Everglades DO standard. However, we believe the proceeding comments are based on an incomplete understanding of the currently developed SSAC. As stated in the chapter, when applied to Everglades monitoring data the SSAC very acceptably balances type I and II error rates. Specifically, it correctly identifies and passes sites exhibiting natural background conditions while also correctly identifying sites exhibiting low DO levels resulting from nutrient enrichment, groundwater infiltration, or other anthropogenic impacts. The test does appear to be sensitive to even occasional low levels (acutely low). The authors acknowledge that additional evaluation and modeling to test the sensitivity of the SSAC may be required. Several years of additional data are now available to further test, evaluate, and refine the SSAC where necessary.

The SSAC was developed with consideration of monitoring and measurement methodologies. Because long-term continuous diel monitoring is not practical or cost effective within the marsh, DO monitoring is typically conducted at monthly intervals utilizing grab collection method (single sample in time). Unless, compliance is determined across a network of stations or across multiple years, monthly sampling frequencies do not provide sufficient data to accurately characterize exceedance frequencies, except where impairment is pervasive. Furthermore, basing a SSAC on the low end of the distribution is not protective of the high end of the distribution. Native flora and fauna are likely adapted to periodic low DO levels; however, in order to persist these organisms require higher levels (>chronic levels) over the longer term. For example: although the WCA-2 reference sites exhibit DO concentrations below 2.0 mg/L over 10% of the time these stations are above 3.5 and 5 mg/L over 50% and 25% percent of the time, respectively. A single number protecting the low end (acute level) of the distribution would not protect the vital high end of the distribution, but a measure of central tendency can account for both ends of the distribution. Additionally, it may be necessary to incorporate a lower never to exceed (be below) concentration into the SSAC to provide adequate protection against acute affects.

Comment: *For the purposes of compliance monitoring, it would be beneficial to provide data on loads coming from individual structures into the EPA.*

Response: More detailed summary appendices have been added to the final report to address this concern. Additionally, Chapter 8 provides data on loads passing through individual structures.

Comment: *The Everglades Round Robin (ERR) laboratory analyses were described in the 2002 ECR (although no ERR results presented), but text of 2A and Appendix 2A-1 addressed the issue of laboratories.*

The ERR program was mentioned in the 2002 ECR to provide peer reviewers with assurances that the issue of inter-laboratory data comparability were being addressed. The results of the ERR are reported at the Department's Internet site (http://www.dep.state.fl.us/labs/everglades /index.htm). Since these results are already widely available there is no need to reproduce them within this report.

Comment: *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)?*

Response: All results presented in this years version of Chapter 2A, including those for previous water years, are handled in a consistent manner using the ½ MDL replacement for MDL values . Therefore, in terms of handling data less than the MDL, results presented in Tables 2A-5 and 2A-7 are fully comparable among years. However, results presented in previous ECRs are not fully comparable due to different MDL handling protocols.

Comment: *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.*

Response: The authors are aware of Dr. Walker's report. However, this report is still in draft form and is undergoing continued discussion among the TOC principals; therefore it would be premature and inappropriate to discuss this report at this time.

Comment: *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.*

Response: The referenced analysis is a matter of discussion among TOC principals and relates to the Federal Settlement Agreement. These analyses are being conducted and reported upon on a quarterly basis. The most recent report can be found at the District's web site. Since this report follows a different reporting and review cycle it would inappropriate to include it within this chapter.

Comment: *When specific conductance (conductivity) is reported, you should in at least one place in the chapter state whether this is a lab or field measurement, and state that this is (or is not) temperature compensated.*

Response: Specific conductance as reported is based on *in situ* measurement and is temperature corrected to 25° C. This will be acknowledged at the appropriate place in the chapter.

U.S. ENVIRONMENTAL PROTECTION AGENCY

Comment: *The existing WCA2A water quality monitoring stations shown in this figure are different than those shown in Figure 5-4 on page 5-25.*

Response: The maps in Chapter 2A depict the existing ambient monitoring network including sites in canals and water control structures. It is not yet clear what portion of this network would be appropriate to the criterion measurement methodology. Figure 5-4 provides only a base map of the current SFWMD Environmental Monitoring and Assessment (EMA) network. It was presented as a straw man to initiate discussions among the interested parties.

Comment: *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.*

Response: Use of ½ the MDL provides an estimate of the distribution of values below the MDL as was recommended by Lin and Niu (1998). Replacement with the MDL value produces a biased (high) estimate of this distribution. All data including historic periods were handled consistently to avoid artificial bias. The SFWMD laboratory did in fact recently report a change in their TP MDL from 4 ppb to 2 ppb. The issue of handling multiple detection limits will need to be address, possibly through TOC.

MICCOSUKEE TRIBE

Comment: *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.*

Response: This is a concern raised by several reviewers, both formal and informal. Two appendices have been added to the report to address this concern. Water year 2002 concentrations and loads are now summarized in Appendices 2A-3 and 2A-4, respectively.

LITERATURE CITED

Lin P. and X. Niu. 1998. Comparison of Statistical Methods in Handling Minimum Detection Limits. Technical Report submitted to the Florida Department of Environmental Protection.

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

Weaver, K. 2001. Evaluation of Chronic Toxicity Based Guidelinesfor Pesticides and Priority Pollutants in the Florida Everglades. Appendix 4-4 in G. Redfield, editor. 2001 Everglades Consolidated Report. South Florida Water Management District, West Palm Beach, FL.

Nov. 7, 2002

Reply to USEPA comments (Richard Harvey to Garth Redfield) on Chapter 2B, ECR, 2003

Text in *italics* represent replies from Don Axelrad

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.

Some of the suggested text has been added to the chapter.

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.

Top down and bottom up explanatory text has been added to the chapter.

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.

Citation corrections made.

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

Citation corrections made.

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.

Analysis of FAMS and SoFAMMS results as discussed in the chapter indicate that the majority of atmospheric mercury deposition is from locally sourced mercury. Additional research is necessary to further quantify local vs. global deposition, and to refine the E-MCM. Any discussion as to necessary remediation to achieve elimination of fish consumption advisories, or ensure absence of wildlife effects is speculative, as the action level re the former is under review, and no good reference dose exists for the latter. But a little speculation is not necessarily out of place in the chapter.

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).

"*Sulfate" was changed to "sulfide in anaerobic sediments" in the chapter to clarify the point. Gilmour's data indicate that over 100 micromolar sulfide in sediments inhibits mercury methylation*.

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

Agreed, but comments from Exponent before and after completion of the draft ECR 2003 dwell on biodilution, and the issue needs to be fully discussed.

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.

As in Stober et al. 2001, methylation occurs in periphyton in the northern eutrophic Everglades, but Gilmour and Krabbenhoft claim this does not occur south of F1. At very eutrophic sites, there is little mercury methylation in sediments due to high sulfide concentrations. For the bulk of the Everglades, mercury methylation in sediments exceeds that in periphyton.

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).

Yes, sulfide. Change made.

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.

It is now generally recognized that surface water methylmercury concentrations are not a good predictor of methylmercury bioaccumulation in resident fish. At the 10 sites studied by the USGS In the Everglades, the strongest correlation was between surficial sediment methylmercury concentrations and THg as methylmercury in mosquitofish. Gilmour and Krabbenhoft claim that the net rate of methylmercury production is closely linked to the concentration of methylmercury in surficial sediment and, thence, fish. Further research may be necessary to clarify this issue. MeHg binding to TOC and DOC is via sulfhydryl moieties. DOC competes with TOC for Hg(II) and MeHg. An analysis of District water quality and fish THg data collected over the last four years suggests a moderate to strong inverse relationship with DO in the interior Everglades.

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.

The most recent conceptual model version from Stober et al. 2001 has replaced the older version.

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).

The Stober et al. 1996; 1998, 2001, and the Scheidt et al. 2000 reports have been cited as recommended in the chapter rewrite.

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.

Average concentrations of water column TP decrease from about 7.8 ppb at WCA-2A-U3 to about 6.9 ppb in the northern portion of ENP, so for 95% of the Everglades, the differences in TP concentration cannot be driving the differences in fish THg levels. Nevertheless, comments from Exponent before and after completion of the draft ECR 2003 dwell on biodilution in the 5% of the Everglades where the TP gradient is steepest, and the issue needs to be fully discussed.

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.

So noted, and in the next version of the chapter new verbiage will have the USEPA references cited re the sulfate gradient.

Response to Comments on **Appendix 2B-5** by Exponent, Inc., for the Sugar Cane Grower's Cooperative of Florida

Final Draft 11/17/02

Overview

The Sugar Cane Growers Cooperative of Florida (the Cooperative), via its consultant, Exponent, Inc., of Bellevue, Washington, has submitted comments on **Chapter 2B** and **Appendix 2B-5** of the draft Everglades Consolidated Report for 2003. Commenter is concerned that the Florida Department of Environmental Protection (the Department) and the South Florida Water Management District (the District) have misunderstood the Cooperative's biodilution hypothesis as it applies to the alleged biologically significant, unintended adverse mercury consequences of attaining the proposed 10 ppb Water Quality Criterion (WQC) for total phosphorus (TP) in the nutrient-impacted areas of the Everglades. To the contrary, the District has devoted a disproportionate fraction of its discretionary mercury monitoring, research, and modeling resources to understanding the influences of surface water, pore water, and sediment quality on the mercury cycle. However, a complete analysis of the issue requires the consideration of all the possible effects of phosphorus on the mercury cycle, including biodilution and the stimulation or suppression of methylmercury production. Consideration of one influence in isolation (e.g., biodilution) results in an unbalanced assessment of the overall effect of phosphorus on the mercury cycle.

Numerous scientific studies conducted over the last decade by or for the District, the Florida Department of Environmental Protection (the Department), the U.S. Geological Survey (USGS), the U.S. Environmental Protection Agency (USEPA), and the Academy of Natural Sciences Estuarine Research Center (ANSERC) support the District's alternative hypothesis that sulfur, not phosphorus, is the primary determinant of the rate of MeHg production, that the rate of MeHg production, not biodilution, is the primary determinant of the concentration of MeHg in surficial peat soils, and that the concentration of MeHg in surficial peat soils, and not biodilution, is the primary determinant of the magnitude of MeHg bioaccumulation in the aquatic and terrestrial food chains. In this conceptual model, phosphorus plays a secondary role via its indirect influences on the sulfur cycle through the carbon and oxygen cycles. The most recent results for the Everglades mesocosm studies conducted by the USGS and ANSERC provide further support for the District's hypothesis.

Commenter has not provided any new Everglades-specific monitoring, research, or modeling results in support of the phosphorus-biodilution hypothesis or contrary to or confounding of the District's sulfur-methylation hypothesis (PTI, Inc., 1994, 1995a,b, 1997; Exponent 1998, 1999, 2000, 2001, 2002). By contrast, the District has provided Everglades-specific monitoring, research, and modeling data contrary to or confounding of the phosphorus-biodilution hypothesis in a series of published analyses (Fink et al., 1999; Fink and Rawlik, 2000; Fink, 2001; Fink, 2002; **Appendix 2B-5** this report). **Appendix 2B-5** summarizes the results of those analyses, as well as new analysis, research, and modeling results that appear to support or appear contrary to or confounding of the District's hypothesis. On balance, the District has concluded that the weight of scientific evidence continues to support the District's hypothesis.

From a policy perspective, the question is whether the eventual attainment and maintenance of the proposed 10 ppb TP WQC in the already impacted areas of the Everglades will cause or contribute to an increase in MeHg risks to fish-eating wildlife that will outweigh the benefits of the restoration of a balance in flora and fauna. The District has concluded that this is highly unlikely. The District based this conclusion on worst-case ecological risk assessments (ERAs) using data from a well-studied, unimpacted area of the Everglades (Rumbold et al., 1999; Rumbold, 2000). The EIR and ECR Peer Review Panels reviewed the District's ERAs and found them to be scientifically sound and credible (EIR Peer Review Panel Report 1999; ECR Peer Review Panel Report 2000). To date there has been no successful refutation of the District's conclusion that, even in a worst case, when the eutrophic sites become like the unimpacted sites, the magnitude of MeHg exposure in fish-eating wildlife, including the endangered wood stork, and the associated risk of MeHg toxic effects, will not be unacceptable (Fink et al., 1999; Fink and Rawlik, 2000). The results of modeling by USEPA's Office of Research and Development using a modified version of MERC5 (Ambrose and Araujo, 1998; Fink, 1999) supported the conclusion that there was a large margin of safety in the District's worst-case risk assessment. More recent modeling by Tetra Tech, Inc., using a modified version of E-MCM(II) provides further support for this conclusion (Tetra Tech, Inc, 2002; **Appendix 2B-5**, this report). The Department concurred with the District's conclusions that: (1) the Cooperative's predicted adverse mercury outcome from the attainment and maintenance of the 10 ppb TP WQC is highly unlikely, and (2) the benefits of phosphorous reduction would likely substantially outweigh the increased mercury risks, even in a worst case.

The District has summarized a substantial body of evidence from Everglades-specific monitoring, research, and modeling study results to support the conclusion that the unintended adverse mercury consequences of Everglades restoration brought to its attention by the Cooperative are highly unlikely (Fink et al., 1999; Rumbold et al., 1999; Fink and Rawlik, 2000; Rumbold, 2000; Fink, 2001; Fink, 2002). Commenter has presented no new evidence that would require the District to change that conclusion. To the contrary, the analysis, integration, and synthesis of the monitoring, research, and modeling studies carried out in Reporting Year 2002 further support the District's position. Based on that substantial, unrefuted body of evidence, the District has concluded that there is no need to raise the TP WQS, limit the area of its application, or delay its implementation from a mercury perspective. The Department concurs with the District's conclusion.

Hereafter, the comments from Exponent on behalf of the Cooperative are in italics. The replies of the author of Appendix 2B-5 are in brackets.

September 21, 2002

Comments on the 2003 Everglades Consolidated Report
Executive Summary

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."*

[The conceptual model developed in **Appendix 2B-5** attempts to integrate the results of all of the Everglades-specific monitoring, research, and mass budget calculations into a self-consistent, qualitative representation of the key physical, chemical, and biological processes governing the transport, biogeochemistry, and bioaccumulation of MeHg and the conditions and factors that influence the directions and rates at which these processes proceed. The transport, distribution, transfer, storage, transformation, and bioaccumulation of mercury species in the Everglades (the Everglades mercury cycle) are processes with complex interrelationships with each other and the carbon, oxygen, iron/manganese, phosphorus, and sulfur cycles. Phosphorus influences the oxygen, sulfur, and iron/manganese cycles indirectly via its direct influence on the carbon cycle. In addition to iterating and organizing the available information on the mercury cycle and its Everglades manifestation, the conceptual model also allows one to evaluate in a qualitative way the expected effect of changing water quantity or quality on one or more elements of the mercury cycle within this reasonably complete, self-consistent, mechanistic framework. Biodilution is taken into account by the District's conceptual model in a reasonably complete, self-consistent way within that framework.

In the District's conceptual model, in locations where phosphorus, and not light, is the limiting factor, phosphorus regulates the rate of primary production and the generation of organic particles. Hg(II) in runoff and rainfall is sorbed to settling organic particles and transported to the surficial sediments. DOC has a strong affinity for Hg(II) and competes with organic particles for the newly delivered Hg(II). In the highly colored waters of the Everglades, the high DOC concentration has the effect of reducing the rate of transfer of Hg(II) from the water column to the sediment by transferring some of the Hg(II) that would otherwise be sorbed to settling organic particles to DOC, a process not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998). Some of the organic particles decompose and release the sorbed Hg(II) back into the water column, either in the truly dissolved phase or associated with DOC generated during decomposition. In the District's conceptual model, phosphorus not only stimulates the production of organic particles but their decomposition by aerobic and anaerobic bacteria as well (Reeder and Davis, 1993; Richardson et al., 1994; Newman et al., 2001). This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998).

The remaining Hg(II) sorbed to undecomposed settling organic particles deposits on the surficial sediment. The increased flux of undecomposed organic particles increases the rate of sediment accumulation or peat accretion. This has the effect of diluting the Hg(II) already present in the soils. However, in the District's conceptual model, the rate of MeHg production is proportional to the metabolic activity of the primary methylating bacteria, sulfate reducing bacteria (SRB), and the concentration of bioavailable Hg(II). The concentration of bioavailable Hg(II) is proportional to the absolute concentration of Hg(II), but the constant of proportionality is determined by the pore water sulfide concentration in surficial sediment (Gilmour et al., 1998b; Gilmour et al., 1999; Benoit et al., 1999a,b; Jay et al., 2000; Benoit et al., 2001a). In the Everglades, the areas where organic particle production is highest are the areas where the concentration of surface water sulfate and pore water sulfide are also highest (Fink, 2001; **Appendix 2B-5**, this report). This mutes the effect of increased production of organic matter or biodilution on MeHg production. This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998).

The production of MeHg in the shallow, subtropical Everglades occurs primarily in the sediment (Gilmour et al., 1998a,b; 1999), although the production of MeHg in the periphyton mats of eutrophic WCA-2A-F1 has also been observed (Cleckner et al., 1999). However, because the area covered by periphyton mats in the highly eutrophic areas is small due to shading by the dense cattail canopy (Grimshaw et al., 1997; McCormick et al, 1999), the actual flux of MeHg from eutrophic periphyton mats into the water column and the food is expected to be low relative to the supply of MeHg from other sources (Fink and Rawlik, 2000; Appendix 2B-5 this report). Some of the newly deposited Hg(II) is bioavailable to sulfate-reducing bacteria (SRB) for methylation under anaerobic conditions (Gilmour et al., 1992; Gilmour et al., 1998a,b; Gilmour et al., 1999). Rooted aquatic plants can also take up Hg(II) and MeHg either from the soil or the water column (Ribeyre 1993; Ribeyre and Boudou, 1994). USGS ACME data from the Everglades (USGS unpublished data, 1999) indicate that emergent rooted macrophytes such as sawgrass (*Cladium jamaicense*) and cattail (*Typha domingensis*) are likely to be bioaccumulating Hg(II) and MeHg primarily from the soil, not the water column, and that these plants biodilute the Hg(II) but bioconcentrate the MeHg in soil. Cattail are about ten times more efficient at bioconcentrating MeHg from soil at WCA-2A-F1 as sawgrass is at WCA-2A-U3 (Appendix 2B-5 this report). It can be conjectured with reasonable confidence that, when the plant tissue contaminated with this soil Hg(II) and MeHg decomposes, some of the Hg(II) and MeHg will be released back into the water column or transferred to the detrital food chain. In the most eutrophic areas where cattail is densest and most rapidly growing, the biodilution and recycling of Hg(II) and the bioconcentration and recycling of MeHg are likely to have their most significant impacts on MeHg production and bioaccumulation, if any. This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998). The Hg(II) that is not taken up by rooted plants or methylated can be reduced to elemental mercury (Hg(0))(Gustin et al., 2002) with the potential for subsequent uptake by the roots of plants and evasion from the leaves (Lindberg et al., 1999). If this phenomenon is occurring in the most eutrophic areas of the northern Everglades where cattail is densest and most rapidly growing, one could speculate that the mining and evasion of Hg(0) could contribute to the reduction in the average concentration of THg in the soil column. The effect of this phenomenon, a lower average THg concentration in the soil column, cannot be distinguished from soil biodilution via more rapid peat accretion stimulated by excess water column total phosphorus. One could further speculate that the production and evasion of Hg(0) could compete with SRB for bioavailable Hg(II). This would have the effect of reducing

the rate of MeHg production unrelated to the sulfur cycle or biodilution. This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998). The remaining Hg(II) is shunted to microenvironments where it is chemically transformed so as to be unavailable for methylation, reduction, or root uptake. This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998).

In the USGS ACME study, the emergence of zoobenthos from the sediment at night was accompanied by a substantial increase in the flux of MeHg to the water column (Krabbenhoft et al., 2001). The observed flux exceeded that which could be supported by diffusive processes under the otherwise quiescent conditions encountered, based on a calculation from site-specific concentration data and the application of Fick's Law (R. Harris, Tetra Tec, Inc., personal communication, 2001). Some of the MeHg that is transferred to the water column during this emergence cycle is sorbed to DOC, some to settling organic particles, some to living periphyton cells, and some is transported away with the flowing water. As with the settling organic particles, DOC competes with the cells of the periphyton mat for the MeHg released into the water column from the sediment. This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998). Sunlight degrades MeHg, but the highly colored waters absorb the photoactive wavelengths of sunlight required for this transformation, so the significance of this process may be muted. This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998).

In the shallow, subtropical Everglades, the water column averages about 0.6 m (~2 feet deep), with periods of drawdown and occasional dryout, followed by periods of deeper water 1-1.6 m (~3 to 5 feet) (Sklar et al., 2002). Unlike deep, stratified, northern temperate and subarctic lakes, in open waters the sediment is always readily accessible to all trophic levels of the food chain. Aquatic fauna living on or in the surficial sediment take up MeHg directly from the pore water or the ingestion of soil particles (Odin et al., 1995). It has also been hypothesized that it is the benthic fauna and not the limnetic flora that are the primary vectors for MeHg into the Everglades food chain—that is, the Everglades is primarily a decaying plant-based, not living plant-based food chain (Loftus et al., 1998). This hypothesis is supported by the strong correlation between the concentration of THg in mosquitofish and the concentration of MeHg in surficial sediment (Fink, 2001; Orem et al., 2002). The direct transfer of MeHg produced in the sediment to benthic fauna and thence the limnetic food chain short-circuits the effect of increased primary production on the dilution of the concentration of MeHg on suspended and settling organic particles (**Appendix 2B-5** this report), which is the primary manifestation of biodilution (Exponent, 1998). This process is not taken into account in the conceptual model that supports the biodilution hypothesis as set forth by Exponent (1998). In the most impacted areas of the Everglades, where primary production and sulfate concentrations are high and dissolved oxygen is low, the highly sulfidic waters associated with these conditions could be toxic to pollution-intolerant plants and sessile benthic fauna and intolerable to pollution-intolerant, mobile species. Such conditions would favor a simplified food web and lower MeHg bioaccumulation factors relative to highly oligotrophic sites farther down stream.

The above recitation of the District's conceptual model of mercury cycling in the Everglades is not complete. For example, it leaves out the sulfur biogeochemistries of water, pore water, and soil and their influences on mercury speciation, distribution, transport, transformation, and

bioaccumulation. However, it is sufficiently complete to characterize the primary interactions of the carbon cycle and the mercury cycle in a way that places the phenomenon of biodilution in proper conceptual context, as manifested in the Everglades. It also underscores why one cannot extrapolate the results of studies of mercury cycling in deep-water lakes from temperate and subarctic climates or experiments designed to mimic such environments to the shallow, subtropical marsh that is the Everglades.

The conceptual model is intended to guide the development and application of a mechanistic mathematical model capable of quantifying these complex interrelationships in a realistic way to assist in the development of an appropriate course of action. Focusing solely on the effect of phosphorus on organic particle production in isolation overemphasizes its importance to the detriment of sound restoration decision-making. The Everglades Mercury Cycling Model, while not a finished product, is constrained by the conceptual model discussed above and does use all of the Everglades-specific data to calculate the appropriate values of the coefficients that dictate the rates of the key processes and the conditions and factors that influence the magnitudes of those rate coefficients. The E-MCM, appropriately initialized and calibrated with Everglades-specific monitoring and research data, predicts that a reduction in biodilution, when water column TP decreases from an average 70 ppb in the already impacted area to 10 ppb, will cause an increase in fish MeHg bioaccumulation on the order of ~150% (Ambrose and Araujo, 1998) to 150% to 250% (Tetra Tech, Inc, 2002), not the nine-fold increase predicted by Exponent's empirical model (Exponent, 1998) or the twelve-fold increase assumed by the District in its worst-case ecological risk assessments (Rumbold et al., 1999; Rumbold, 2000). Although not definitive, the modeling results suggest that there was a large margin of safety in the District's conclusion that any loss of biodilution when the STAs are completed would not likely be harmful to fish-eating wildlife from a mercury perspective.]

"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 Hintelmann [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 purpose of the study of Miles et al. (2001) was to measure the MeHg equilibrium partition coefficients between water and various species of algae under controlled conditions. Among the many strengths of the partitioning study by Miles et al (2001) was its rigorous identification of and control for potential experimental design artifacts and confounding variables. Early in the development of the experimental design, it became apparent that the algae cultures were not sorbing added MeHg. The P.I. quickly discovered that the problem was the bacteria growing in the culture medium. With their high surface-area-to-volume ratios and large numbers, the bacteria were absorbing the vast majority of MeHg. When the cultures were sterilized, the added MeHg sorbed to the algae, as intended (C. Miles (deceased), UF, personal communication, 1999). Dissolved organic carbon (DOC) is another potential confounding variable. Miles et al. (2001)

calculated that the low concentration of DOC present in the experimental containers (< 2 mg/L vs 15-45 mg/L in Everglades water) would not confound the interpretation of the study results. This conclusion was based on a model calculation carried out using MINEQL, which solves for the distribution of MeHg among various phases using literature values for MeHg solubility, particle sorption, and DOC complexation. The important point here is not that the DOC concentration in the experimental containers was high enough to confound the interpretation of the experimental results, but rather that it was important to verify that this would not be the case, precisely because scientists carrying out such rigorous studies of MeHg partitioning recognize that DOC can compete with algae for MeHg in aquatic ecosystems. However, while the effect is weak, the results of the study suggest that the expected inverse relationship between algae partition coefficient and DOC was, in fact, observed. As this was not the purpose of the study, however, for purposes of clarification, the text will be modified to include the recommended quote and the above interpretation of the results.]

"The author of the appendix hypothesizes that DOC may compete with particulates for available methylmercury."

[Not may compete, does compete. This is not a hypothesis but the consequence of the application of thermodynamic laws to the partitioning of substances with affinity for both DOC and organic particles in aquatic ecosystems in general and the Everglades in particular. Hg(II) and MeHg are strongly associated with organic particles, but Hg(II) (Benoit et al., 2001b; Drexel et al., 2002; Haitzer et al., 2002) and MeHg are also strongly complexed by dissolved organic carbon (DOC). The law of conservation of mass requires that the Hg(II) or MeHg associated with DOC cannot be associated with organic particles at the same time. The theory of thermochemical equilibrium requires that the addition of DOC to a system with a fixed concentration of organic particles will shift some of Hg(II) or MeHg sorbed to organic particles off of the particles on onto DOC. If DOC settles at all, it settles much more slowly in a gravitational field than negatively buoyant organic particles, so the addition of DOC to a system with a fixed concentration of negatively buoyant organic particles must necessarily reduce the net settling rate of Hg(II) and MeHg from the water column to the sediment. One of the manifestations of biodilution is an increase in the concentration settleable organic particles. This shifts some of the Hg(II) and MeHg from the truly dissolved phase to the particle-bound phase, with an associated increase in the settling rate of Hg(II) and MeHg from the water column to the sediment. The addition of DOC to the system competes with the organic particles for some of the Hg(II) and MeHg. This has the effect of holding up a greater fraction of Hg(II) and MeHg in the water column and reducing the rate of particle-mediated settling of Hg(II) and MeHg to the sediment. If this does not occur, then the physical laws that govern the conservation and distribution of matter and energy must be rewritten to accommodate commenter's conceptual model, in which only the concentration and flux of organic particles play a significant role in mediating Hg(II) and MeHg removal from the water column and MeHg bioaccumulation. The high DOC concentration in Everglades water is not typical of the northern temperate lakes that dominate the biodilution literature. Nor are the high concentrations of calcium and magnesium, again due primarily to the contribution of EAA runoff. To add to the complexity, pH, alkalinity, calcium and magnesium are known to mediate the complexation of Hg(II) and MeHg by DOC, when that complexation occurs via carboxylic acid moieties and/or electrostatic charge (Stumm and Morgan, 1996).

The question is not whether but to what extent DOC competes with organic particles for MeHg in aquatic ecosystems. This can be quantified using the following equations (McCarthy and Black, 1988:

$$Fd = (1+KPalgae \times [algae] + KDOC \times [DOC])^{-1}$$

$$Falgae = (KPalgae \times [algae]) \times [1+KPalgae \times [algae] + KDOC \times [DOC])^{-1}$$

$$Fdoc = (KDOC \times [DOC]) \times [1+KPalgae \times [algae] + KDOC * [DOC])^{-1}$$

$$Fd^* = (1+KDOC \times [DOC])*(1+KPalgae \times [algae] + KDOC \times [DOC])^{-1}$$

Where:

Fd = fraction dissolved (actual)

Falgae = fraction on algae particles

Fdoc = fraction on DOC

Fd* = fraction dissolved (apparent) = Fd + Fdoc = 1 - Falgae

Kpalgae = algae/water partition coefficient (L/Kg)

[algae] = concentration of algae (Kg/L)

KDOC = DOC/water partition coefficient (Kg/L)

[DOC] = concentration DOC (Kg/L)

The power of Everglades DOC to compete with algae particles for MeHg is depicted in **Figure 15**, summarizing calculations prepared in response to a later comment. It depicts the effect of Everglades DOC on algae concentrations found in the experiment by Pickhardt et al. (2000), when Kpalgae is the average partition coefficient calculated from the data published by the investigators and KDOC is calculated from data collected at WCA-2A-U3 by Krabbenhoft et al., (2001) using a blue-green algae KP of 5E6 L/Kg, which is in the range obtained by Miles et al., (2001). Even at a DOC concentration as low as 0.75 mg/L, most of the MeHg is calculated to be complexed with DOC, not associated with algae particles. In the eutrophic Everglades, where suspended particles average an order of magnitude higher than in the experiment by Pickhardt et al., (2002), the DOC averages about 45 mg/L, so only about 15% of the MeHg is associated with particles at WCA-2A-F1.]

"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."

[Babiarz et al. (2001) calculated the concentration of Hg(II) and MeHg on particles by difference. This tends to overestimate the concentration on particles relative to direct measurements (e.g., 34% by difference vs 8% by direct measurement at F1; D. Krabbenhoft, USGS, unpublished data, 2002). In addition, Babiarz et al. (2001) based their conclusions on one set of measurements in July 1997, while Krabbenhoft et al. (2001) based their conclusions on eleven sampling events between March 1995 and July 1998. Nevertheless, if the findings of Babiarz et al. (2001) are typical of the Everglades at F1, U3, 3A-15, and 2BS, then biodilution could not be the explanation for the much higher concentrations of MeHg in mosquitofish at oligotrophic U3 than eutrophic F1, because biodilution requires that the MeHg concentration on particles at eutrophic F1 be lower than on particles at oligotrophic U3. The phosphorus-rich regions have higher average TSS than the phosphorus-poor regions. For example, TSS averages 9 mg/L at site F1 and less than 4 mg/L at site U3, while DOC averages about 45 mg/L at F1 and 38 mg/L at U3. All other things being equal, one might then expect the concentration of MeHg on particles to be lower at F1 than U3. This, in fact, is what was observed by Krabbenhoft and co-workers during the period March 1995 and July 1998.

One might then infer that biodilution is occurring at F1 relative to U3. This would be an incorrect inference for several reasons:

- The majority of the particles at F1 are of allocthonous (external), not autocthonous (internal) origin, so while high the TSS concentration in inflowing water is generally associated with high water column TP concentrations, the latter is not causing the former.
- DOC competes with organic particles for the physicochemically available methylmercury in the water column, and the DOC concentration and affinity for MeHg are both higher at F1 than U3 (G. Aiken, USGS, personal communication). This tends to shift methylmercury from organic particles and the filterable fraction to DOC complexes and the nonfilterable fraction. This would give the appearance of a biodilution effect where none exists. In fact, high DOC concentrations would weaken the biodilution effect by decreasing the concentration of Hg(II) and MeHg on settling organic particles, while increasing the concentrations of Hg(II) and MeHg in the water column.
- MeHg production is likely to be substantially higher at U3 than F1, because the average concentration of MeHg in surficial soil is about 4 times higher at U3 than F1, while the Hg(II) concentration is only 50% higher (District analysis of ANSERC data reported in Gilmour et al., 1999), suggesting that more of the Hg(II) depositing to the sediments at U3 is being converted to MeHg than is the case at F1. While Gilmour et al. (1999) were unable to demonstrate a

difference in the gross rate of MeHg production between F1 and U3 on average, one cannot conclude from this that there was no difference in the net MeHg production rate between F1 and U3. The inferred increase in net MeHg production rate from the measured increase in the average MeHg concentration of surficial sediment between F1 and U3 is also consistent with what was required to calibrate the E-MCM model at sites F1 and U3 (R. Harris, Tetra Tech, Inc, personal communication, 2002).

- Most important, based on the District's mass balance calculations, biodilution is higher at U3 than F1 due to severe light limitation on periphyton growth at F1. If one normalizes the concentration of MeHg on particles to the inferred MeHg production rates at F1 and U3, the results are consistent with the District's calculations.
- Gut content studies of mosquitofish conducted by the Wisconsin DNR for the USGS ACME Study (1995-1998) indicated that the mosquitofish at F1 are feeding primarily on organisms living on or in the surficial sediment, and that there guts contain a disproportionate amount of sediment material, whereas mosquitofish at U3 are foraging primarily on organisms living on or in the periphyton mats, and their guts contain a disproportionate amount of periphyton. This suggests that some of the apparent increase in the THg in mosquitofish between F1 and U3 can be attributed to changes in foraging preferences unrelated to biodilution but related to a discontinuous improvement in water and habitat quality. This is supported by the District's observation that the fraction of THg that is MeHg in mosquitofish increases exponentially from about 25% at F1 to greater than 85% at U3 (P. Rawlik, SFWMD, personal communication, 2001).

The only self-consistent explanation for this set of observations is that MeHg import and internal production, not biodilution, are the primary determinants of the MeHg concentrations in water, on particles, in surficial sediments, and in mosquitofish at highly eutrophic F1, where classical biodilution is supposed to be manifest. Conversely, at highly oligotrophic U3, where classical biodilution is supposed to be absent, increased periphyton biomass turnover probably biodilutes some of the increased MeHg flux from U3 sediments, but this effect is probably more than offset by the change in foraging preferences in the mosquitofish. The result is an observed exponential increase in mosquitofish THg as MeHg with downstream distance, driven primarily by a four-fold increase in the concentration of methylmercury in sediment and an additional step in the mosquitofish food chain, neither of which is a primary manifestation of biodilution or directly affected by the concentration of TP in the water column. Only a model that can capture these complexities has predictive value in quantifying the potential post-restoration increase in the concentration of THg as MeHg in aquatic animals and the attendant post-restoration increase in MeHg exposures and risks to fish-eating wildlife. That model is E-MCM.

However, the development of a food web more typical of the unimpacted Everglades at U3 is likely a response to the improved water quality at U3 relative to F1, and this is precisely the change that the Everglades Forever Act has mandated. This will necessitate some but not likely an ecologically significant increase in the concentrations of MeHg at each step in the food chain based on the District's worst-case ecological risk assessment (Rumbold et al., 1999; Rumbold, 2000). The best way to mitigate this effect is to continue to press for local air emissions control, not to increase the proposed TP WQS from 10 ppb to 15.6 ppb, limit the area of its application, or delay its implementation.]

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."

[Miles et al. (2001) did not conduct a dose-response study of the effect of phosphate addition on the partitioning of MeHg to various algae species. Rather, the study differentiated among the effects of static growth, P-unlimited exponential growth, and P-limited exponential growth conditions on the magnitude of the partition coefficient for *Selanastrum capricornutum*, the best studied of the green algae available for laboratory culture. The table of partition coefficient values indicates that *Selenastrum* in P-limited growth phase has a lower average log K value (log K = 5.88 average of three identical reps) than *Selanastrum* in a static growth condition (log K = 6.72) or in P-unlimited exponential growth (log K = 6.66). As noted in the discussion of the study results in Appendix 2B-5, the interpretation of these results is confounded by the observation that *Selanastrum* changes its cell structure in response to phosphate addition. Nevertheless, the results suggest that the effect of the stimulation of exponential growth, a condition associated with bloom dilution, on MeHg partitioning to algae, may be muted to some extent by the changes in algae cell structure for algae that behave like *Selenastrum capricornutum*.]

"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."

[It is not clear whether commenter is suggesting that the rates of aerobic and anaerobic decomposition of plant tissue cannot have an effect the production rate or character of DOC, a form of undecomposed organic carbon, that DO cannot have an effect on aerobic vs anaerobic decomposition rates, or that DOC or DO cannot have an effect MeHg production, decomposition, or bioaccumulation. None of these statements would be accurate. Nor is it accurate to say that the District has not cited evidence either from the published literature or Everglades studies for the effect of DOC or DO on methylmercury production or bioaccumulation. The discussion of the influences of the carbon, sulfur, oxygen, and phosphorus cycles on the mercury cycle and the interactions among those cycles were discussed in some detail in Chapter 7 of EIR 1999 (Fink et al., 1999). Commenter's claim that DO did not increase with decreasing water TP concentration along the "F" Transect was also refuted in Appendix 1 of ECR (1999). DO affects the rate of uptake of MeHg from the water column by governing the gill purging rate of aquatic animals that take up dissolved MeHg directly from the water column (Norstrom et al., 1976). However, under conditions favoring low feeding rates during colder temperatures, even larger fish may take up MeHg primarily via the gills rather than the gut (Post et al., 1996). Several studies (Winfrey and Rudd, 1990; Monson and Brezonik, 1999; Snodgrass et al., 2000; Choi et al., 1998) support the hypothesis that DOC decreases the concentration of truly dissolved MeHg in the water and on

organic particles, which, in turn, reduces the steady state ratio of the concentration of MeHg in the organism to that in the water, the so-called bioaccumulation factor (BAF).

Numerous field observations of aquatic ecosystems support the hypothesis that microbially mediated MeHg production occurs almost exclusively under anaerobic conditions, i.e., in the absence of DO, in surficial sediments (Wood, 1969; Jensen and Jernelov, 1969; Compeau and Bartha, 1984; Pak and Bartha, 1998). The rate of MeHg production was observed to be higher in eutrophic than oligotrophic sediments in artificially fertilized enclosures in the English-Wabigoon system (Rudd and Turner, 1983), the upper Wisconsin River (McCallister and Winfrey, 1986) and three New Jersey lakes (Pak and Bartha, 1998). Rudd and Turner (1983) attributed the increase in the rate of MeHg production to a higher flux of readily decomposable organic carbon to the sediment. The ability of DO to suppress MeHg production has been demonstrated in controlled chemostat (Compeau and Bartha, 1984; Pak and Bartha, 1998), lake sediment microcosm (Regnell et al., 1996), a model lake system (Regnell, 1994), and Everglades microcosm (Marvin-DiPasquale et al., 2001b). The Onondaga Lake mercury model (Henry et al., 1995) also assumes that MeHg production occurs "at low dissolved oxygen concentrations (in the hypolimnion)." More recent studies support the hypothesis that microbially mediated MeHg decomposition also occurs under anaerobic conditions (Oremland et al., 1991; Marvin-DiPasquale and Oremland (1998); Marvin-DiPasquale et al., 1999; 2001), but DO did not appear to suppress demethylation in an Everglades sediment slurry (Marvin-DiPasquale et al., 2001). The rates of aerobic and anaerobic sawgrass, cattail, and periphyton decomposition have been shown to increase with increasing water TP (Davis, 1989; Reeder and Davis, 1993; Richardson et al., 1995), but alkalinity and hardness appear to mediate this rate, perhaps via the influence of DOC on the sorption of the exogenous enzymes involved in this process (Newman et al., 2001). Some of the organic carbon released during plant tissue decomposition is dissolved organic carbon (DOC). DOC strongly complexes MeHg (Hintelmann et al., 1995; Amirbahman et al., 2002) and Hg(II) (Frimmell et al., 1984; O'Driscoll and Evans, 2000; Benoit et al., 2001b; Drexel et al., 2002; Haitzer et al., 2002). The decomposition of leaves may stimulate MeHg production (Balogh et al., 2002), as can the direct addition of DOC to an Everglades mesocosm (Orem et al., 2002). The effects of DOC on MeHg production and bioaccumulation are discussed in detail in **Appendix 2B-5**. Therefore, the author does not believe that any further modifications to the text are necessary in this regard. If commenter has specific studies that establish the unimportance of either DO or DOC in the transport, biogeochemistry, or bioaccumulation of MeHg in aquatic systems in general or the Everglades in particular, the author requests the relevant citations.]

"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:"

[No conclusions have been drawn from the exploratory data analysis regarding the importance of the various physical, chemical, and biological processes governing mercury transport, biogeochemistry, or bioaccumulation in the Everglades. However, **Appendix 2B-5** does discuss the consistency of the results with the conceptual model of mercury transport, biogeochemistry, and bioaccumulation set forth in **Appendix 2B-5**. At commenter's request, to address the issue of

inter-seasonal differences in temperature and sunlight intensity on plant growth rates, we have parsed the mosquitofsh THg data set for the "F" Transect into wet and dry season subsets and recalculated the correlation relationship between mosquitofish THg and the average of water column TP for the preceding 3 months. This approach was chosen to more accurately represent the water quality conditions to which rooted, floating, and attached plants were responding, rather than the instantaneous concentration encountered at or about the time of mosquitofish collection. In addition, it had the benefit of reducing the variability in the water quality data. To address the issue of changing variance over time, we have carried out an analysis of the USGS mosquitofish data from collections at WCA-2A-F1 between July 1995 and July 1998 and compared them with the District's data collections at F1 from September 1997 to August 2000. To address the issue of the effect of atypical events on the strength of the regression relationships, we have carried out the regressions with and without the post-dry anomalous mercury events in June 1999 and May 2000.

The time trends in the mean concentrations of water TP at F1 through F5 and U3 are summarized in **Figure 4** and the normalized standard errors for water column TP at F1 through F5 and U3 are summarized in **Figure 5**. It is more likely that the variability has increased at the high TP concentration sites rather than at U3. The corresponding mosquitofish concentrations for 9/97 to 5/98, 5/98 to 3/99, 6/99 to 2/00, and 2/00 to 8/00 are 98 +/- 35 ug/Kg wet wt, 64 +/- 26 ug/Kg wet wt, 87 +/- 30 ug/Kg wet wt, 135 +/- 125 ug/Kg wet wt. Clearly, the variability in the total mercury concentration in mosquitofish increased from +/- 30 ug/Kg wet wt in the period 6/99 to 2/00 to +/- 125 ug/Kg wet wt in the period 2/00 to 8/00, while the variability in water TP decreased from +/- 0.004 mg/L to 0.002 mg/L at U3. More clearly, something other than water TP is the primary influence on methylmercury bioaccumulation in mosquitofish at highly oligotrophic U3. One such factor is dryout and rewetting, as discussed in **Appendix 2B-1**. Another such factor is the sulfate and sulfide concentrations in surficial sediment pore water, as discuused in **Appendix 2B-2**. Most clearly, this underscores why one cannot use a regression relationship between water TP and mosquitofish THg without a strong mechanistic basis to predict post-ECP impacts of P reduction on methylmercury production and bioaccumulation with the accuracy, precision, and reliability required for effective resource management decision-making.

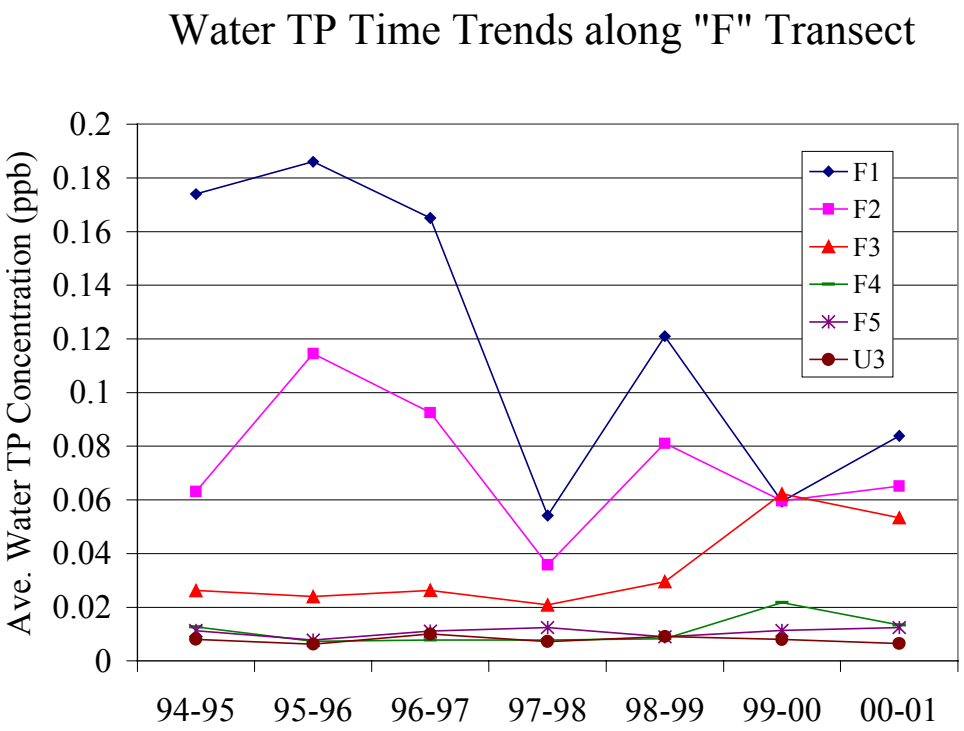

Figure 4. Time Trends of Average Surface Water TP Concentrations along WCA-2A "F" Transect for period May 1997-April 2001

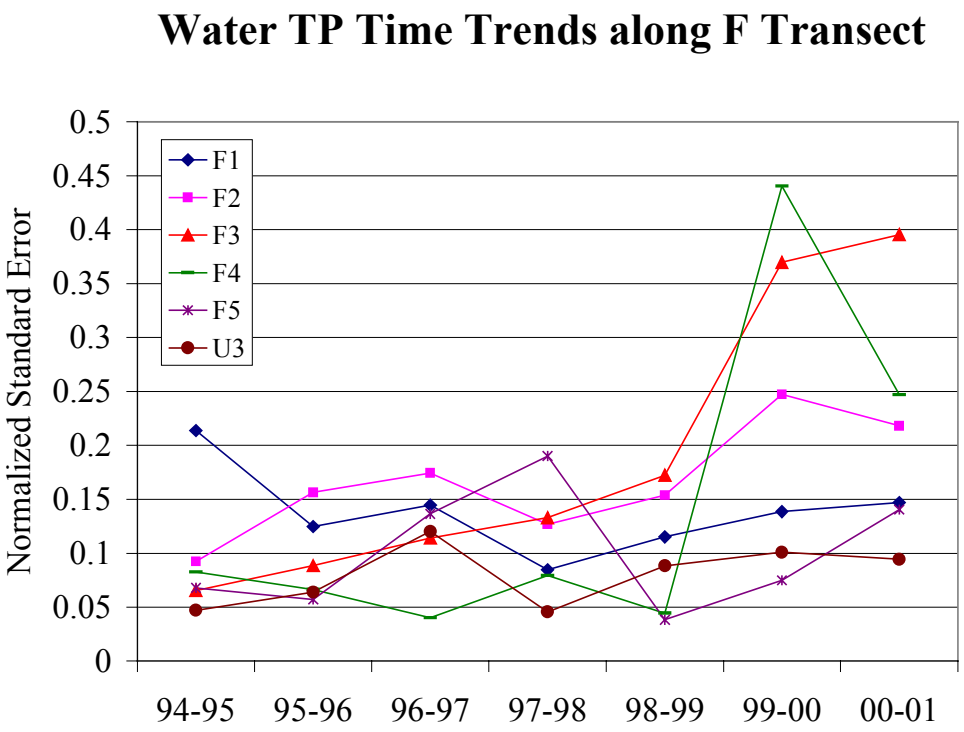

Figure 5. Time Trends of Normalized Standard Error [(σ/μ)/(√(2*n)] of Average Surface Water TP Concentration along WCA-2A "F: Transect]

To test commenter's hypothesis, the District censored the biodilution data to include only summer and fall data (May-September 1997-2000), when the incident sunlight intensity, temperature, and plant densities and production rates are high. It is during the summer, then, that biodilution should be expected to maximize its influence along the WCA-2A nutrient gradient. The results of this analysis indicate that the correlation is weak but positive (r = 0.178), suggesting that water TP is either influencing something that is causing or contributing to an increase in methylmercury bioaccumulation in mosquitofish or is co-correlating with something that is causing or contributing to an increase in methylmercury bioaccumulation in mosquitofish. Both stormwater runoff and rainfall volume increase in the summer, as do the concentrations of inorganic mercury and methylmercury in stormwater runoff and the concentration of inorganic mercury in rain. Based on the results of the USGS ACME mesocosm study, most of the methylmercury being produced and bioaccumulated in the Everglades aquatic food chain is the new mercury added to the system, not the old mercury bound to soil. This means that an increase in the summer loading of inorganic mercury in stormwater runoff and rain should be accompanied by an increase in methylmercury production. If this increase more than offsets the effect of increased plant densities and production rates, then the positive correlation with an increase in summertime TP loads and concentrations would be a consequence of co-correlation, not direct or indirect causation. Under this scenario, the

combined increase in the runoff and rainfall load of inorganic mercury to the system likely fuels an increase in methylmercury production, which, in turn, increases the concentration of methylmercury in surficial soils, which is then transmitted to organisms living on or in the surficial soils, and thence into the mosquitofish that feed on those organisms, while incidentally ingesting soil detritus in the foraging process. Conversely, in the winter, while plant densities and production rates decrease in response to decreasing sunlight, temperature, and water TP loads and concentrations, the rate of production of methylmercury could slow to an even greater extent in response to a reduction in the inorganic mercury load and temperature, such that the biodilution effect is actually more significant in the winter than the summer. To test this hypothesis, the District censored the data to include only the data collected in October-April in 1997-2000. When this is done, the correlation between water TP and mosquitofish THg is moderately negative (r = -0.47). When the data are LN transformed, the correlation increases to the mid range of moderate strength (r = -0.63). Interestingly, the absolute concentrations of total mercury and methylmercury still peak in surface water in the summer (F1: unfiltered THg = 2.86 dry/winter vs 3.57 wet/summer; unfiltered MeHg = 0.25 dry/winter vs 0.34 ng/L wet/summer), despite the inferred higher biodilution, with the average concentrations in the summer exceeding those in winter by 1.25 and 1.36 for THg and MeHg, respectively, at F1 and 1.18 and 1.13 for THg and MeHg, respectively, at U3. This supports the position of the Department, District, and USGS that the influx of fresh inorganic mercury mediated by the concentration of pore water sulfide, and not biodilution mediated by surface water, pore water, or surficial soil TP, are the primary determinants of the rate of methylmercury production, that the rate of methylmercury production, not biodilution, is the primary determinant of the concentration of methylmercury in surficial sediment, and that the concentration of methylmercury in surficial sediment, and not biodilution, is the primary determinant of methylmercury bioaccumulation in fish. It also appears that the higher rate of methylmercury production, and not biodilution, is the primary determinant of the concentration of methylmercury in surface water. However, the smaller difference between wet and dry seasons at U3 than at F1 may be due, in part, to the higher calculated periphyton biodilution at U3 than F1, based on Everglades-specific data and not the general literature for northern temperate lakes (**See Appendix 2B-5** this report).

The most significant improvement in the strength of the correlation between water column TP and mosquitofish THg occurs when the June 1999 and May 2000 trips are removed from the data base. This can be justified on the basis that both trips occurred immediately following an extended period of drawdown and dryout, resulting in surficial soil oxidation, with subsequent atypical release of nutrients, dissolved organic carbon, and sulfate upon rewetting. The biogeochemical and trophic dynamics associated with the 1999 event are discussed in detail in **Appendix 2B-1** of this report. When the database is censored in this way, the Pearson correlation coefficient for the combined sites increased from -0.46 to -0.76. This indicates that the hypothesis of an inverse relationship between surface water TP and methylmercury bioaccumulation in mosquitofish cannot be rejected on the basis of these monitoring data alone. When the correlation is between mosquitofish THg and the average water TP concentration for the three months preceding fish collection, the correlation increases substantially. Parsing the data into wet/summer and dry/winter data sets does increase or decrease the Pearson correlation coefficient substantially. However, when the sites are analyzed individually, the correlation coefficients decrease substantially, suggesting that something other than water TP accounts for most of the variability in the data.

There is another test of the biodilution hypothesis that must also be met. Between 1995-1997 and 1997-2000, the average water column TP concentration at WCA-2A-F1 decreased from about 175 ppb to 70 ppb. If biodilution were the primary determinant of MeHg bioaccumulation in mosquitofish, then the concentration of THg in mosquitofish must have increased substantially between 1995-1997 and 1998-2000. As can be ascertained from the analysis below, it is unlikely that this is the case when the two anomalous MeHg events identified above are removed from the database.

Mosquitofish Mercury Time Trends

Samples of mosquitofish (*Gambusia holbrooki*) have been collected by USEPA Region 4, USGS, and the District at or near site F1 in WCA-2A between 1994 and 2000. During that same period, the average annual concentration of total phosphorus (TP) at F1 declined from about 175 ppb in 1995-1997 to roughly 70 ppb in 1997-2000. In theory, then, it should be possible to determine whether the average concentration of THg in mosquitofish at F1 has increased, decreased, or stayed the same over that period. If the concentration of THg in mosquitofish increased over that period, then, all other things being equal, the hypothesis that mercury in fish must increase with a decrease in the concentration of the average TP in water has not been disproved. Conversely, if the concentration of THg in mosquitofish decreased or did not change over that period, then, all other things being equal, the hypothesis that mercury in fish must increase with a decrease in the concentration of the average TP in water has been disproved. Unfortunately, due to differences in the way the three agencies collected and analyzed the fish, the data are not strictly comparable. This limits the usefulness of the trend analysis. However, with the appropriate caveats, some meaningful observations may be extractable from these combined data sets.

USEPA Region 4 collected mosquitofish samples (n = 5 per site per event) along three nutrient gradients in the Everglades in a one-time event in March of 1994: WCA-1 from the L-7 canal east into the center of WCA-1; WCA-2A from the L-39 canal south into the center of WCA-2A; and southern WCA-3A and northern ENP south from southern WCA-3A into northern ENP. The fish were analyzed by Florida International University (FIU) using a nitric acid digestion in sealed glass ampules for subsequent analysis by a Merlin instrument. This was also the data set used by the consultant to the Sugar Cane Growers Cooperative of Florida (the Cooperative) to quantify and apply the biodilution hypothesis (PTI 1995a,b; PTI, 1997; Exponent, 1998, 1999, 2000, 2001). Unfortunately, all of these data were fatally flagged by USEPA Region 4's Quality Assurance Officer and should not be used for this or any other purpose (data spreadsheet transmitted by D. Scheidt, USEPA Region 4).

The Wisconsin Department of Natural Resources collected mosquitofish samples (n = 3 to 5 per site per event) at F1 and U3 between March 1995 and July 1998 for USGS-Madison. From September 1997 to August 2000, the District collected 75-250 fish for each sampling event and separated the fish into small ($x < 0.07$ g), mid-size ($0.07 \text{ g} < x < 0.28$ g), and large fish ($x > 0.28$ g). These categorizations were based on a study conducted by the District (P. Rawlik, SFWMD, personal communication), which determined that there were two breakpoints in the mosquitofish population: between juvenile fish vs mature males and mature, non-gravid females and between mature males and non-gravid females vs gravid females. The mid-size fish were mixed (composited), homogenized, and the homogenate was subsampled n = 5 times for analysis using the same method as that described above for USGS-Madison but by Frontier Geosciences of

Seattle (FGS), WA. In addition, the District determined that the average of n = 5 subsamples of a homogenate of one, 100-fish randomly selected subpopulation of 200 fish in the mid-size fish category was indistinguishable from the average of n = 10 individual fish randomly subsampled from the other randomly selected 100-fish subpopulation (Rumbold, 1999).

While the analysis of n = 5 individual mosquitofish randomly selected from the catch will not produce as accurate and precise an estimate of the true mean of the concentration of THg in the mosquitofish population as n = 5 subsamples of the 75-250 fish composite homogenate, the difference in the two sets of estimates is not likely to be statistically significant. More importantly, the mosquitofish samples were analyzed using the same method and the results and the estimates of the true average concentration of THg in the sampled population of mosquitofish should be reasonably comparable. However, in a side-by-side comparison of samples collected at the same location and time, the USGS-Madison results (n = 3 events) averaged about 46% lower than the results from FGS. To address this issue, the data will be analyzed with and without adjustment.

To further complicate matters, there were two dryout events at F1 in the spring of 1998 and 2000 that did not occur during the springs of 1995, 1996, 1997, or 1998. These dryouts resulted in anomalously high concentrations of THg in mosquitofish. To address this complication, the data will be analyzed with and without the post-dryout data.

Recognizing these differences and complications, the data analysis proceeded as follows:

- The data were analyzed for trends with all of the unadjusted USGS data and all of the District data for 1995-1997 vs 1997-1999 and 1996-1998 vs 1998-2000.
- The data were analyzed for trends without the two dryout events for 1995-1997 vs 1997-1999 and 1996-1998 vs 1998-2000.
- The data were analyzed for trends without the two dryout events and without March, May, and October data to approximate the semi-annual sampling by USGS for 1995-1997 vs 1997-1999 and 1996-1998 vs 1998-2000.
- The analyses in (1), (2), and (3) were repeated with adjusted USGS data. The results for adjusted and unadjusted data with and without the two dryout events was also be compared to the prediction made using the nonlinear empirical model developed by Exponent (1998).

Results

The averages and standard deviations of the combined District and unadjusted USGS data for 1995-1997 vs 1997-1999, 1996-1998 vs 1998-2000, and 1995-1998 vs 1997-2000 are summarized in **Table 1**, along with the p values for the Wilcoxon nonparametric analysis of signficant difference of the means of these data set pairs. All calculations were carried out using a standard SAS program. With the dryout events, there is a substantial but not a statistically significant increase in the average mosquitofish THg concentration between the unadjusted USGS data collected in 1995-1997 or 1996-1998 and the District data collected in 1997-1999 or 1998-2000. Without the dryout events, there is a modest but not statistically significant decrease for both data sets. When the District data are further censored to approximate the semi-annual sampling carried out by USGS, the preceding pattern is repeated. When the USGS data are multiplied by 1.463 to make it more equivalent to the District data, with the dryout events there is some but not a statistically significant increase in the average mosquitofish THg concentration between the USGS

statistically significant increase in the average mosquitofish THg concentration between the USGS sampling in 1995-1997 or 1996-1998 and the District sampling in 1997-1999 or 1998-2000. Without the dryout events, there is substantial but not a statistically significant decrease. This decrease is also reflected in the data pairs based on the simulation of semi-annual District sampling.

Table 1. Difference in mean concentrations of THg in mosquitofish at WCA-2A-F1 between 1995-1998 and 1997-2000.

Source	Dates	Manipulation	N1	AVE (S.D.)	Source	Dates	Manipulation	N2	AVE (S.D.)	Wilcoxon Two-Sided p Value
USGS	1995-1997	Unadjusted	5	6.7 (5.0)	SFWMD	1997-1999	Uncensored	8	12.3 (18.5)	0.830
USGS	1995-1997	Unadjusted	5	6.7 (5.0)	SFWMD	1997-1999	Delete anomalous events	7	5.8 (2.0)	1.000
USGS	1995-1997	Unadjusted	5	6.7 (5.0)	SFWMD	1997-1999	… and other than semi-annual data	4	5.9 (2.7)	0.905
USGS	1996-1998	Unadjusted	5	8.4 (3.4)	SFWMD	1998-2000	Uncensored	8	18.3 (23.7)	0.477
USGS	1996-1998	Unadjusted	5	8.4 (3.4)	SFWMD	1998-2000	Delete anomalous events	6	5.6 (2.9)	0.0965
USGS	1996-1998	Unadjusted	5	8.4 (3.4)	SFWMD	1998-2000	… and other than semi-annual data	4	5.9 (3.7)	0.1485
USGS	1995-1997	Adjusted (x 1.463)	5	9.8 (7.3)	SFWMD	1997-1999	Uncensored	8	12.3 (18.5)	0.943
USGS	1995-1997	Adjusted (x 1.463)	5	9.8 (7.3)	SFWMD	1997-1999	Delete anomalous events	7	5.8 (2.0)	0.635
USGS	1995-1997	Adjusted (x 1.463)	5	9.8 (7.3)	SFWMD	1997-1999	… and other than semi-annual data	4	5.9 (2.7)	0.722
USGS	1996-1998	Adjusted (x 1.463)	5	12.3 (5.0)	SFWMD	1998-2000	Uncensored	8	18.3 (23.7)	0.359
USGS	1996-1998	Adjusted (x 1.463)	5	12.3 (5.0)	SFWMD	1998-2000	Delete anomalous events	6	5.6 (2.9)	0.062

USGS	1996-1998	Adjusted (x 1.463)	5	12.3 (5.0)	SFWMD	1998-2000	… and other than semi-annual data	4	5.9 (3.7)	0.1485
USGS and SFWMD	1995-1998	Adjusted (x 1.463)	5	8.6 (5.8)	USGS and SFWMD	1997-2000	Uncensored	8	16.8 (21.2)	0.83
USGS and SFWMD	1995-1998	Adjusted (x 1.463)	5	8.6 (5.8)	USGS and SFWMD	1998-2000	Delete anomalous events	6	6.8 (3.4)	0.64
USGS and SFWMD	1995-1998	Adjusted (x 1.463)	5	9.5 (6.6)	USGS and SFWMD	1998-2000	… and other than semi-annual data	4	7.7 (4.2)	0.92

An analysis of mosquitofish THg vs time was then carried out on the unadjusted USGS data set and the uncensored District data sets separately and then the adjusted USGS data set combined with the uncensored data set, the data set without the anomalous, post-dryout events, and without the post-dryout events and other data sets to approximate the continuation of the semi-annual sampling schedule initiated by the USGS. The significance of the slope of the regression was evaluated using the "t" statistic. All calculations were carried out using a standard SAS program. The regression between the unadjusted and USGS data or the uncensored District data and time produced a positive slope that cannot be distinguished from 0 slope (no relationship) with statistical confidence. When the two anomalous events are removed from the District data and when the data are further censored to approximate semi-annual USGS sampling, the slopes of the regression lines are slightly negative but not statistically significantly different from 0. When the adjusted USGS data are combined with the uncensored District data, District data censored to remove the two anomalous events, and District data censored to approximate the semi-annual sampling carried out by USGS, the regression between mosquitofish THg and time generates a line with a slight positive slope, no slope, or a slightly negative slope, none of which are statistically significantly different from 0. These results are summarized in **Table 2**. The mosquitofish THg vs time for the USGS and District data sets are plotted in **Figures 6** and **7**, while **Figures 8-10** depict mosquitofish THg vs time for the adjusted USGS data combined with uncensored District data, District data censored to remove the anomalous events, and District data without the anomalous events censored to approximate semi-annual sampling.

Table 2. Regression of mosquitofish THg vs time for USGS, District, and combined data sets from 1995-1998 and 1997-2000

TREATMENT	SOURCE	Intercept	SLOPE	_RSQ_	p VALUE	EQUATION
UNADJ	USGS	-66.985	0.005	0.281	0.221	y=-66.9847+0.0054536057(x)
ADJ	USGS	-98.020	0.008	0.281	0.221	y=-98.0196+0.0079803278(x)
UNCENSORED	DISTRICT	-239.527	0.018	0.088	0.375	y=-239.527+0.0177658674(x)
NO ANOMALIES	DISTRICT	16.034	-0.001	0.008	0.817	y=16.0336+-0.0006915003(x)
SEMI-ANN	DISTRICT	8.673	0.000	0.000	0.971	y=8.6725+-0.0001604574(x)
ADJ, UNCENS.	MIXED	20.639	-0.001	0.012	0.691	y=20.6391+-0.0009269312(x)
ADJ, NO ANOM.	MIXED	9.656	0.000	0.000	0.983	y=9.6559+-0.0000654147(x)
ADJ, SEMI-ANN	MIXED	-118.698	0.009	0.101	0.200	y=-118.6975+0.0094057037(x)

Based on the preceding analysis, the weight-of-evidence supports the rejection of the hypothesis that the decrease in the average concentration of water column TP from 175 ppb (May 1995 to April 1997) to about 70 ppb (September 1997 to April 2000) must result in a substantial increase in the average concentration of mosquitofish THg during the same period because of a loss of biodilution. The apparent decrease in mosquitofish THg at F1 between 1995-1998 and 1998-2000 could be attributed to the combined effect of a reduction in the stormwater runoff concentrations and loads of THg and MeHg together with the delayed response to a decrease in wet and dry deposition that began in the mid-1980s and was substantially complete by the mid-1990s. The former reductions in concentrations and loads could be attributed to the 50-75% decrease in the concentrations and loads of THg and MeHg discharged from the ENR Project into the L-7 canal relative to the inflow concentrations and loads from the S-5A Pump Station (Miles and Fink, 1998; Fink, 2000). During its operation from August 1994 to March 1999, the ENR Project treated between one-fourth and one-third of the stormwater runoff that would otherwise have been discharged through the S-5A Pump Station untreated into the L-7 canal and thence the L-39 canal and the S-10 structures into the northeast section of WCA-2A. The latter reduction in load can be attributed to an antecedent and concurrent decrease in local air emissions (T. Atkeson, FDEP, personal communication, 2001).

Exponent, Inc (1998) derived an empirical model of mosquitofish THg vs water column TP concentration (average preceding 90 days) for the Cooperative using USEPA Transect Data: Mosquitofish THg = 5316*[TP in ppb]^(-1.262). The empirical model predicts an average mosquitofish THg concentration at 15.6 ppb and 10 ppb of 166 ug/Kg wet wt and 291 ug/Kg wet wt, respectively; the latter being about 75% higher than the former. The question then arises as to whether these estimates are of sufficient accuracy and reliability to be used to guide restoration decision-making. To do this, the predicted values were compared with observed values not used in the derivation of the empirical model. Using the unadjusted USGS mosquitofish THg data at eutrophic F1 and Exponent's empirical model, the error between observed and estimated mosquitofish THg was calculated for each data pair. This exercise was then repeated with District data and then the combined District and adjusted USGS data.

The average percent error for unadjusted USGS data, District data with the post-dry events, and the District data combined with the adjusted USGS data were 263%, 413%, and 320%. When the post-dry event data are deleted, the results are 263%, 499%, and 363%. The results for the first two

exercises are depicted in **Figure 11**, while the results for the last exercise are depicted in **Figure 12**. The percent error was then paired with the average water column TP concentration for the preceding three months to test the hypothesis that the magnitude of the error increases with decreasing phosphorus. The results of the combined District and adjusted USGS data with and without the post-dry events are depicted in **Figures 13** and **14**, respectively. It is clear that the percent error increases exponentially with decreasing concentration of water column TP. This is an interesting outcome, because most of the data in the USEPA data sets used to derive the empirical model is in the low TP concentration range. Perhaps this is a consequence of using: (1) data fatally flagged by USEPA's Quality Assurance Officers in the development of the empirical model; (2) only 29 data points collected at one time in February and March of 1994 along three nutrient gradients. The results of this analysis supports the conclusion that the model systematically overestimates the observed mosquitofish THg values by an average of between 263% and 499% overall. Therefore, the use of the model is contraindicated for predicting the post-ECP concentration of mosquitofish THg when the water column TP concentration decreases from an average of 175 ppb in 1994-1995 to an average of 30 ppb, 15.6 ppb or 10 ppb with the desired accuracy and reliability to support regulatory decision-making.

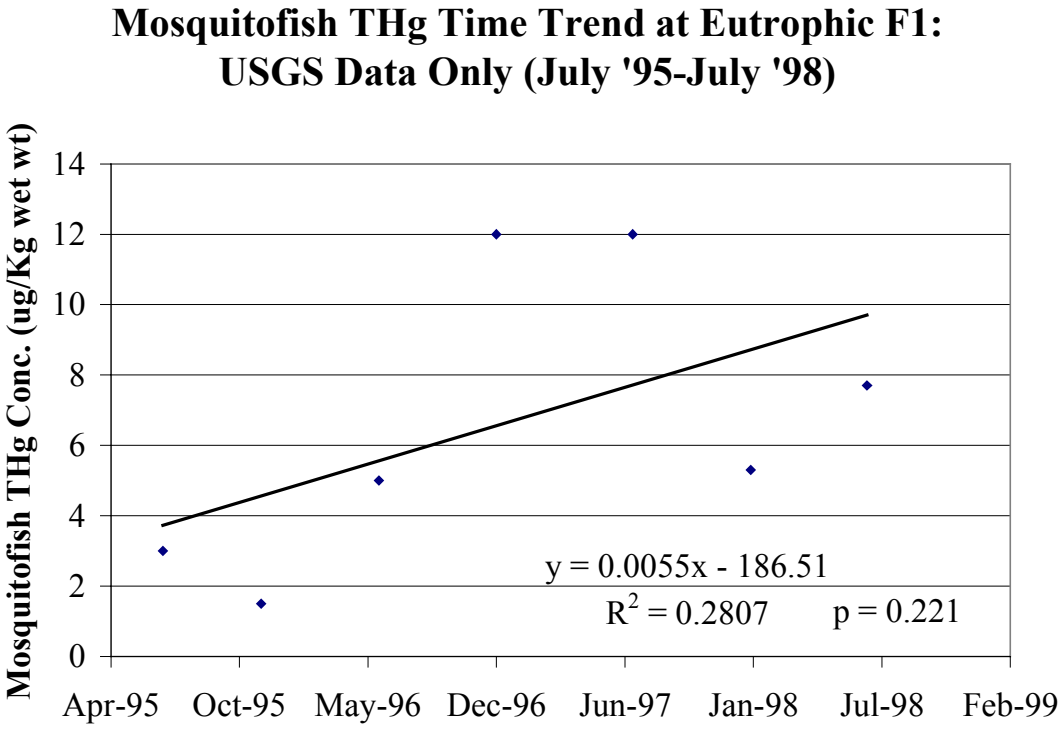

Figure 6. Time trend analysis for uncensored, unadjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 (USGS: July 1995 to July 1998)

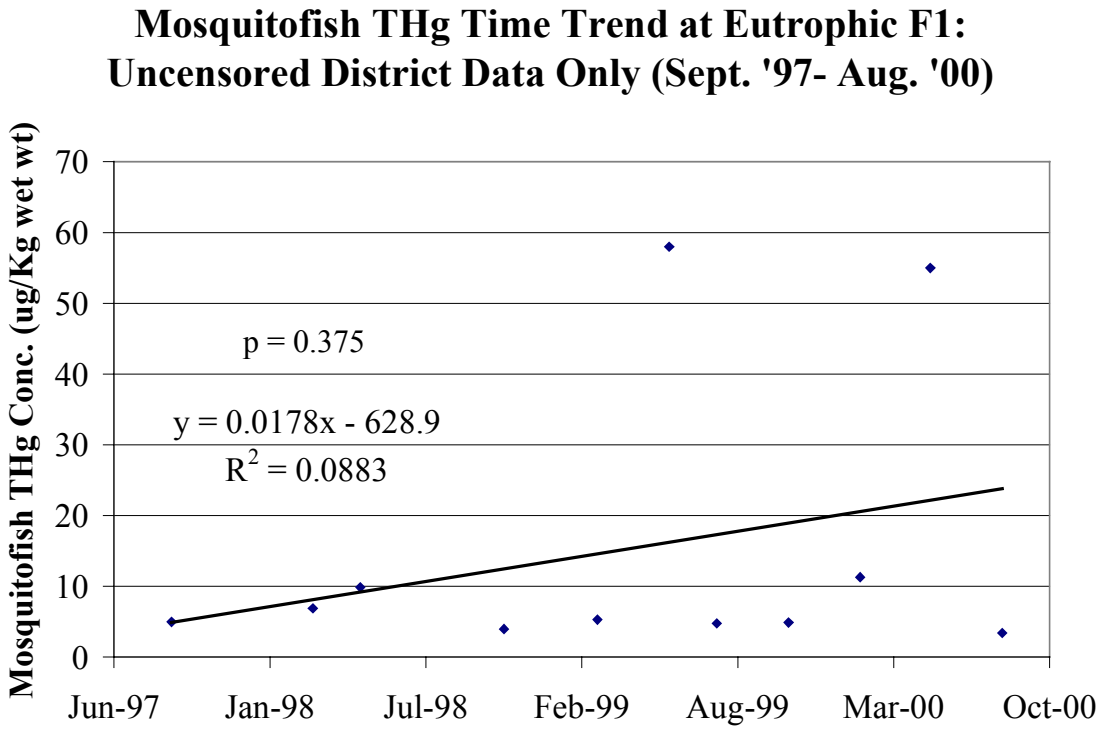

Figure 7. Time trend analysis for uncensored District data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 (District: September 1997-August 2000)

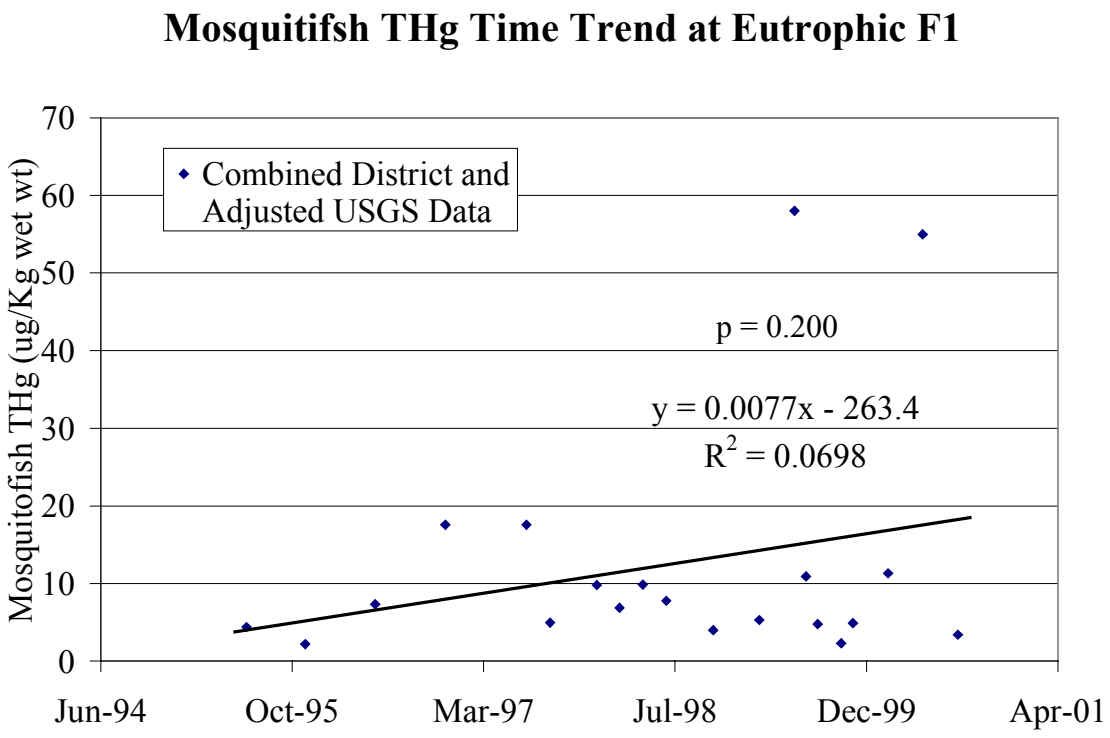

Figure 8. Time trend analysis for uncensored District data sets combined with uncensored, adjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 (USGS: July 1995 to July 1998; District: September 1997-August 2000)

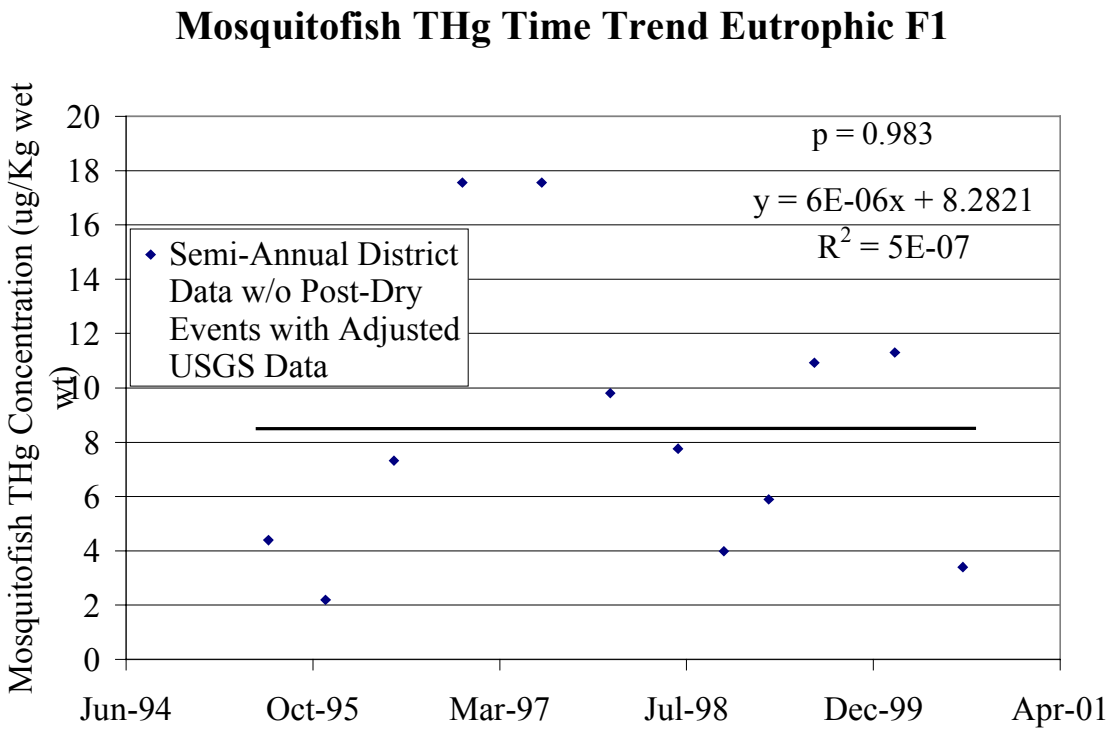

Figure 9. Time trend analysis for censored District data sets (to approximate USGS semi-annual sampling schedule) combined with uncensored, unadjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 (USGS: July 1995 to July 1998; District: September 1997-August 2000)

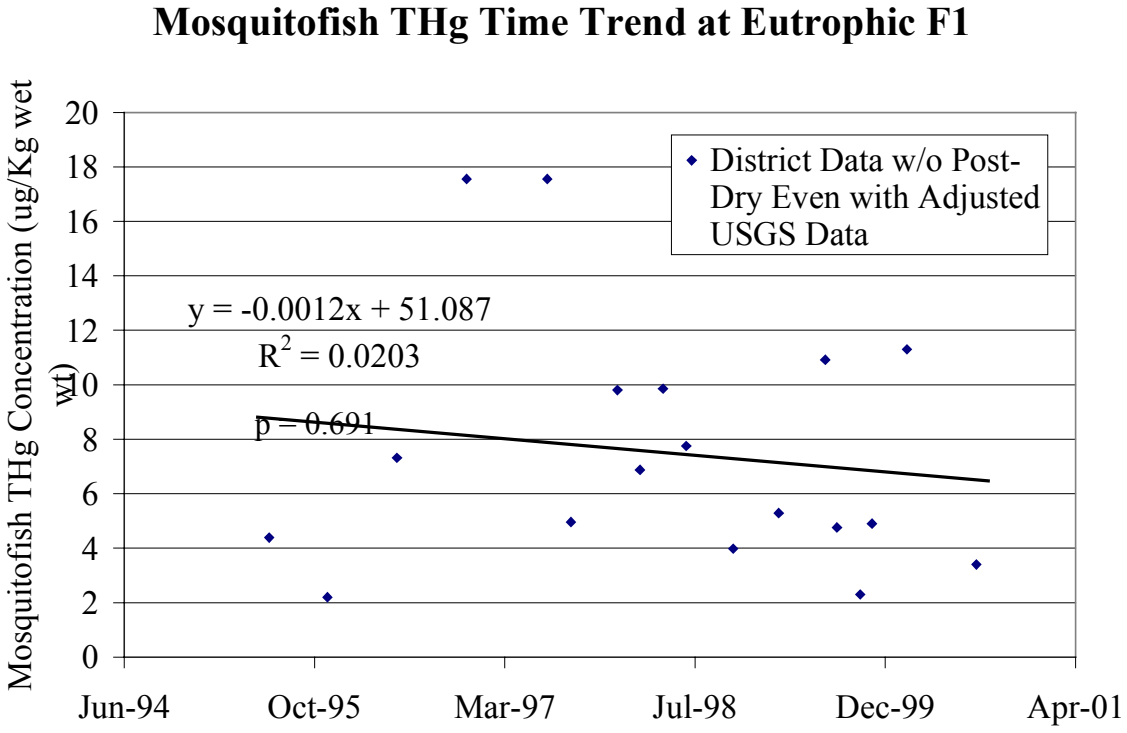

Figure 10. Time trend analysis for censored District data sets combined with uncensored, adjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 (USGS: July 1995 to July 1998; District: September 1997-August 2000)

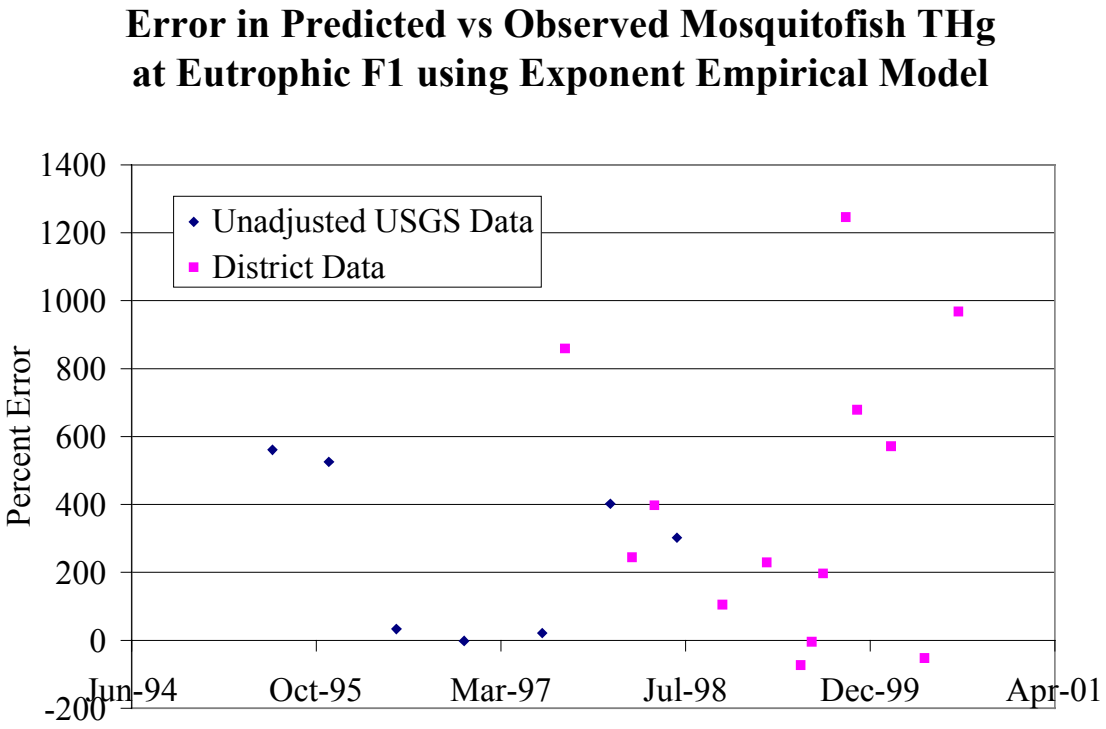

Figure 11. Error analysis of predicted (regression model: Exponent, 1998) vs observed mosquitofish THg concentrations at WCA-2A-F1 (uncensored District data sets combined with uncensored, unadjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 USGS: July 1995 to July 1998; District: September 1997-August 2000)

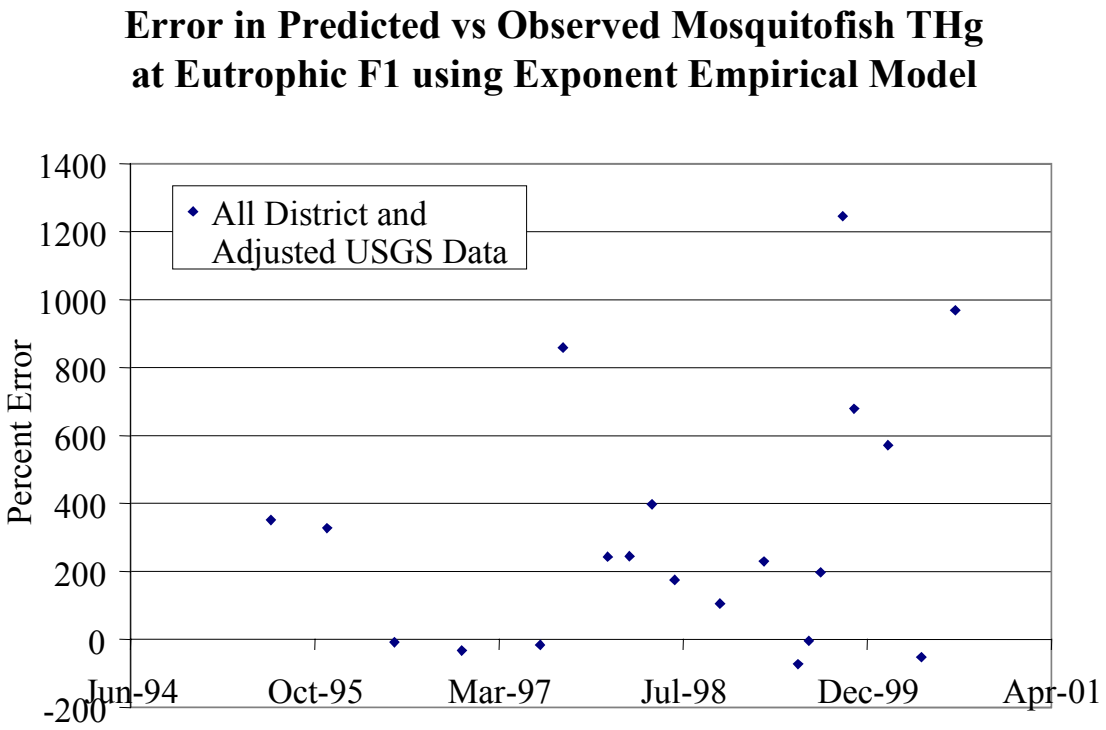

Figure 12. Error analysis of predicted (regression model: Exponent, 1998) vs observed mosquitofish THg concentrations at WCA-2A-F1 (uncensored District data sets combined with uncensored, adjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 USGS: July 1995 to July 1998; District: September 1997-August 2000)

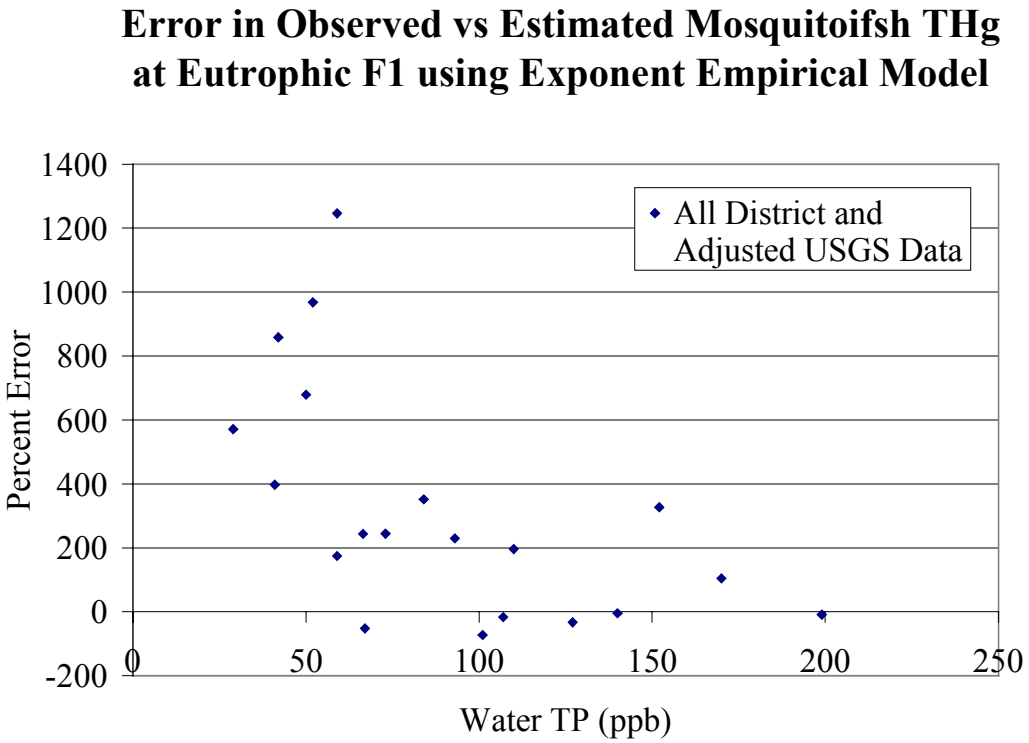

Figure 13. Error analysis of predicted (regression model: Exponent, 1998) vs observed mosquitofish THg concentrations at WCA-2A-F1 (uncensored District data sets combined with uncensored, adjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 USGS: July 1995 to July 1998; District: September 1997-August 2000)

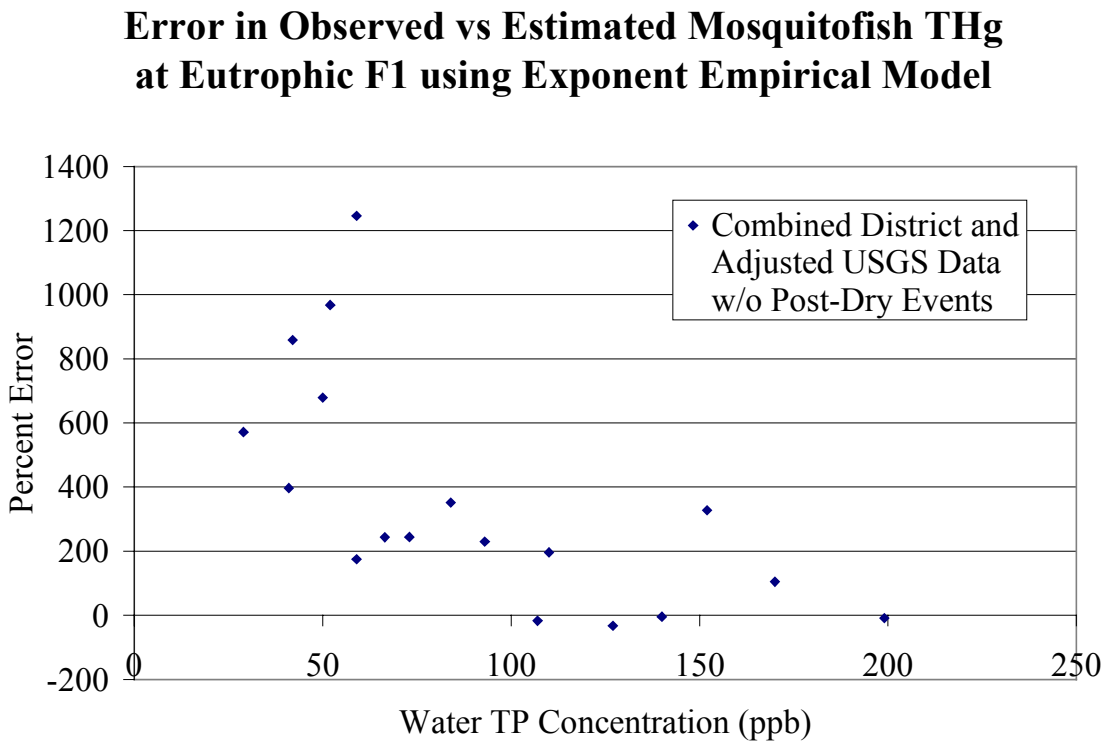

Figure 14. Error analysis of predicted (regression model: Exponent, 1998) vs observed mosquitofish THg concentrations at WCA-2A-F1 (District data set without anomalous events combined with uncensored, adjusted USGS data sets of mosquitofish THg concentrations collected along the WCA-2A "F" Transect at site F1 USGS: July 1995 to July 1998; District: September 1997-August 2000)

While the use of monitoring data alone to infer cause-effect via regression analysis is inappropriate, the use of monitoring data to refute a hypothesis by counterexample is fully appropriate. Without censorship of the data, there is no detectable, strong correlation between mosquitofish THg and surface water, pore water, and soil TP in the most impacted areas of the Everglades where the effect of biodilutiuon should be most manifest. The District disagrees with commenter regarding the cause of the absence of this relationship, however. The District has concluded that there is no significant inverse relationship between surface water TP and mosquitofish THg along the "F" Transect, not because of intra- or inter-annual variability, but because phosphorus-mediated biodilution is increasing, not decreasing, down the nutrient gradient, due to the light limitation effect. At the same time, sulfur-mediated methylmercury production is increasing down the nutrient gradient due to the pore water sulfide effect. The absence of a detectable increase in mosquitofish THg with a substantial decrease in water column TP at F1 between 1995 and 2000 provides additional support for this conclusion.]

"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."*

[This is an exploratory data analysis of the untransformed or logarithmically transformed (natural or base 10) values of mosquitofish THg and the untransformed or logarithmically transformed concentration of a chemical constituent in water, pore water, or surficial peat soil, its value in the preceding 1, 2, 3, 4, 5, or six months (Lag 0-6), its average value for the preceding 1, 2, 3, 4, 5, or 6 months (ave 0-6), or the lag (0-6) ave (0-6). The Pearson correlation coefficient was used as a measure of the degree of correlation between data pairs. Hypotheses might be derived from these results, but no hypothesis was tested in this analysis. However, when the results are consistent or inconsistent a hypothesis put forth by others (e.g., that there is an inverse relationship between the concentration of surface water TP and the concentration of THg as MeHg in mosquitofish along the WCA-2A nutrient gradient caused by a loss of biodilution), this has been noted.

The absence of a scientific hypothesis to be tested does not preclude the existence of a linear or log-linear relationship between variables. This was an exploratory data analysis, as was that carried out by Lange et al. (1993), the results of which commenter has cited elsewhere. Lange et al. found that alkalinity ($r = -0.627$; $p < 0.001$), pH ($r = -0.636$; $p < 0.001$), and chlorophyll a ($r = -0.503$; $p < 0.001$), total hardness (-0.447; $p < 0.001$), and total nitrogen ($r = -0.431$; $p < 0.001$) were better predictors of the concentration of THg in largemouth bass than water TP ($r = -0.279$; $p < 0.043$). (However, the concentration of chlorophyll a is a surrogate for algae particle concentration.) Commenter presupposes that the data are not normally or lognormally distributed.

While most environmental data sets are not normally distributed, many are lognormally distributed or can be treated as such to a good approximation.

Moreover, unlike commenter, the focus was not on phosphorus, but rather on all of the available water, pore water, and sediment quality data, and there were some moderate to strong log-linear univariate correlations with constituents other than phosphorus, so the limitation cited by commenter does not appear to be inherent to the approach or the data sets. That being the case, an alternative hypothesis to that put forth by commenter is that the water TP concentration cannot readily explain a significant portion of the variability in the mosquitofish THg concentration data collected along the WCA-2A nutrient gradient, even when the data are separated by site and season to account for intra-annual and inter-site differences, because (1) MeHg production is likely to be increasing along the WCA-2A nutrient gradient due to a decreasing pore water sulfide concentration gradient, resulting in a nearly four-fold increase in the concentration of MeHg in surficial sediment between F1 and U3; (2) the primary manifestation of biodilution (algae biomass production) is actually increasing along the WCA-2A transect due to a decreasing light-limitation gradient, and (3) an additional step in the mosquitofish food chain has been added because of the substantial improvement in water quality between F1, where DO averages less than 2 mg/L, to U3, where DO averages more than 4 mg/L. More of the site-specific observations are consistent with the District's sulfur hypothesis than commenter's phosphorus hypothesis. This one graph cannot be considered in isolation.]

*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.*

[Non-constant variance may weaken a regression, but it does not invalidate it. A consequence of heteroscedasticity is that the ordinary least squares estimates of the regression intercept and slope are not of minimum variance. Thus, the prediction of Y for a given X will have a large variance (not much confidence in the prediction). In this numerical experiment, **Figure 16** of the text of **Appendix 2B-5** displays the result of one of the hundreds of correlation analyses conducted. The intention was not to predict Hg in mosquitofish (the dependent variable) as a function of phosphorus concentration (the independent variable), as would be the case in regression analysis, but merely to explore the relationship between the two variables. For this purpose, a test of constant variance is not really necessary. Had the goal been to predict Y given X, then testing for and dealing with non-constant variance would be critical in order to obtain a regression line that is determined by data pairs (x,y) that contribute equally in determining it's position and thus the prediction of Y.]

*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 = 0) for the parameters compared in Table 7.*

[The correlation between data pairs may be high and yet not statistically significant or the correlation may be low and is statistically significant. The number of observations contributes to both situations above. The calculation of a p-value would aid in the interpretation of the Pearson Correlation matrix in **Table 7**. The quantification of the statistical significance of the values of the intercept and slope of the regression line in terms of p values have been included in the univariate analyses tables in **Attachment 1**. However, time did not permit modification of the tables in the text. Unfortunately, the SAS program used to generate the multivariate regression relationships in **Attachment 2** does not calculate a p value. SAS staff indicated that this deficiency may be rectified in future versions of the program. Time did not permit running a separate program to carry out these calculations. However, as a general rule, for data sets with a large number of observations, high r^2 values are usually associated with low p values and *vice versa*.]

"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)."*

[That was the point. The study by Pickhardt et al. omitted a measurement of the effect of P addition on MeHg production, which turned out to be an important effect in the English-Wabigoon mesocosm study. The rate of MeHg production is presumed to be proportional to the metabolic activity of sulfate-reducing bacteria and the bioavailable pool of Hg(II) accessible to them. The SRB metabolic rate is a function of temperature and is proportional to its limiting factor. In systems where all potential limiting factors are in excess, temperature controls SRB metabolism, and the bioavailable pool of Hg(II) limits the rate of MeHg production. The bioavailability of Hg(II) to bacteria appears to be mediated by pore water sulfide (Berman and Bartha, 1986; Gilmour, 1998b; Benoit et al., 1999a,b; 2001a) and DOC (Ravichadran et al., 1998; Ravichadran, 1999). The absence of an effect of phosphorus on MeHg production in microcosm (Gilmour et al., 1998a) and mesocosm (Gilmour, **Appendix 2B-2**, this report; **Appendix 2B-5**, this report; Orem et al., 2002) is most likely due to the high pore water sulfide, which is caused by the high sulfate concentrations in water, pore water, and surficial sediment and the high carbon turnover rate, which is caused by the high flux of readily decomposable organic carbon to the sediment, which, in turn, is caused by the high phosphorus concentrations in water, pore water, and sediment. Dave Krabbenhoft of the USGS has reconsidered the conclusion that phosphorus has no effect on MeHg production, noting that for the concentration of MeHg to remain constant in the surficial sediment in the face of an increasing peat accretion rate, the MeHg production rate must increase proportionally. This would be consistent with the hypothesis that, in the presence of

excess sulfate, SRB metabolism will increase in proportion to an increase in the flux of readily decomposable organic carbon to the surficial sediment (Rudd and Turner, 1983).]

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

[**Appendix 2B-5** quoted directly from **2B-2**, which was authored by Cynthia Gilmour, Ph.D., the co-principle investigator for the mesocosm study with David Krabbenhoft, Ph.D., and William Orem, Ph.D., both of the USGS. U3, with an average of 8 ppb water column TP, is moderately enriched when compared with sites in the central and southern Everglades, where concentrations average about 6.9 ppb TP. Significant changes in diatom species abundances have been documented between 7 ppb and 10 ppb. However, when compared with WCA-2A-F1 (average water TP conc. = 115 ppb TP in 1999-2002, U3 is considered unimpacted. We will include the appropriate clarification. We will add the reference to the cited paper.]

"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 CH3Hg + 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 CH3Hg + in algae results in a substantial reduction of CH3Hg + uptake by cladocerans in high nutrient, high algae conditions. Conversely, cladocerans feeding within low nutrient, low algae treatments accumulate more CH3Hg + ."

[This quote will be added. However, the conditions under which the study were carried out are not directly applicable to the Everglades and there are several deficiencies in the study design that may limit the usefulness of the results. There also appears to be a numerical error in the slopes of the *Daphnia mendotae* regression relationships for weeks 2 and 3. These are outlined in **Chapter 2B** and **Appendix 2B-5**. In what follows the study results are analyzed in greater detail.

The average algae concentrations in the tanks can be calculated from the information provided by the investigators to range from about 0.4 mg/L at the 7.4 ug/L phosphate (PO_4^{-3}) to about 1.4 mg/L at 44.4 ug/L dose, as compared to an average of about 10 mg/L at WCA-2A-F1, with an average water TP concentration of about 90 ug/L, and about 3.5 mg/L at WCA-2A-U3, with an average water TP concentration of about 8 ug/L. The dissolved organic carbon (DOC) content of the water was not reported or controlled, despite the fact that a strong inverse relationship has been observed in some lakes or laboratory studies between the concentration of MeHg on particles and

zooplankton and the concentration of DOC (Watras and Bloom, 1995; Monson and Brezonick, 1999). The potential significance of the effect of DOC is illustrated in **Figure 15**, where the fraction of MeHg on particles can be calculated per the method of McCarthy and Black (1988) using the average apparent algae/water partition coefficient calculated from data reported by Pickhardt et al. (2002) (~1.6E6 L/Kg) and the average apparent DOC/water partition coefficient of 1.5E7 L/Kg calculated from data collected at F1 provided by D. Krabbenhoft, USGS, assuming an algae/water partition coefficient of 5.25E6 L/Kg for *Selenastrum capricornutum* in stationary growth phase per Miles et al. (2001).

This is why Miles et al. (2001) controlled for DOC explicitly. (Note: the character of algal exudates and their affinity for MeHg may be very different than that of the DOC in EAA runoff, justifying the use of the KDOC value from Hintelmann et al., 1997). At the very low concentrations of algae and water fleas in the tanks, even a small increase in the DOC concentration in the experimental tanks over time could explain the decrease in the slopes of the regression relationship between Daphnia MeHg and water PO_4^{-3} between weeks two and three.

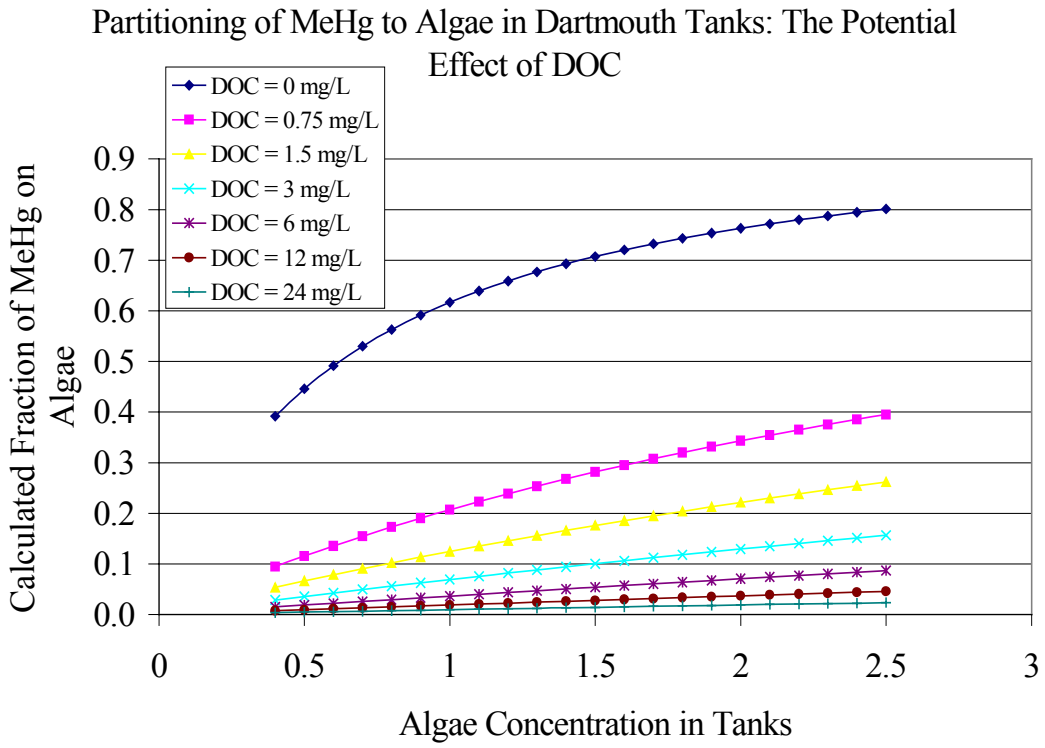

Figure 15. The potential effect of Everglades DOC on partitioning of MeHg to algae particles in the Dartmouth biodilution experiment

Twenty-four hours after adding Hg(II) and MeHg to the algae growing in the PO_4^{-3}-dosed tanks, the linear regression relationships between the concentration of Hg(II) and MeHg vs the concentration of PO_4^{-3} were statistically significant: $CH_3Hg^+ = -80.14 + 4502$; n = 11, $R^2 = 0.499$, $p < 0.016$ and Hg(II) = -917 (ug/L added PO_4^{-3}) + 45290; n = 11, $R^2 = 0.623$, $p < 0.004$. However, the slope of the regression relationship is low. This means that the magnitude of the change in the concentration of MeHg on/in algae is small relative to the magnitude of the change in the concentration of PO_4^{-3}. For example, based on the preceding regression relationship between algae MeHg and water PO_4^{-3}, the MeHg concentration on algae is predicted to increase by only about 13% as the water column TP concentration decreased from 15.6 ppb and 10 ppb between Site F5 and Site U3 along the "F" Transect. It is safe to say that such an effect is very probably not detectable in the wild, let alone of ecotoxicological significance.

The investigators also tested the hypothesis that the MeHg on/in algae would be transferred to an herbivorous zooplankter, (*Daphnia mendotae*), resulting in an increase in the concentration of MeHg in grazers relative to the algae they consumed. This phenomenon is referred to as biomagnification and has been reported in numerous lakes in the environmental literature. Ranges of zooplankton/algae BMFs from 0.4 following flooding in Lake 979 in the Experimental Lakes Region, Ontario, Canada (Paterson et al., 1998), 2.2 in Little Rock Lake in northern Wisconsin (Watras and Bloom, 1992), an average of 2.5 in 12 northern Wisconsin lakes (Back and Watras, 1995) to 8.1 in Onondaga Lake in central New York state (Becker and Bigham, 1995). Interestingly, while the authors conclude that the effect of bloom dilution of MeHg can be propagated to the grazer populations, which is supported by the scientific literature, in actuality the water fleas in week 2 contained about the same average MeHg concentrations as the algae at the same PO_4^{-3} dose. More interestingly, in week 3, the ratio of Daphnia MeHg to algae MeHg actually decrease to about 0.7. This is also reflected in the regression relationships for weeks 2 and 3, with the slope declining between weeks 2 and 3 from -643 to -265 ng g^{-1} L ug^{-1}. This suggests that under the conditions of the experiment, there was no indisputable evidence of trophic transfer of MeHg from algae to water fleas, contrary to the statement by the authors: "From these results, we infer that the concentration of CH_3Hg^+ in Daphnia across treatments was related to the concentration of CH_3Hg^+ (Fig2C) in the algal cells they ingested, which was in turn affected by algal biomass; e.g., that bloom dilution drives a diminution of metals in zooplankton."

One alternative hypothesis for the apparent absence of the propagation of the bloom dilution effect to water fleas is that the failure to allow time for the water fleas to purge their guts of ingested algae prior to analysis convolved the unabsorbed MeHg concentration in algae with the absorbed MeHg concentration in the water fleas. This flaw in the experimental design limits the usefulness of the results.

A second alternative hypothesis is that MeHg was primarily transferred directly from the water to the water fleas via the gill and body surfaces rather at roughly the same rate as from the algae via the gut membrane. This was, in fact, observed by Monson and Brezonik (1999) in their controlled exposure of *Daphnia magna* to dissolved and food-borne MeHg. In the L979 reservoir study, there

was no way for the scientists to distinguish between trophic transfer from seston to zooplankton ($0.4 + 0.85*\log MeHg_{part}$; $r^2 = 0.87$; $p < 0.001$) and water column transfer of "dissolved" MeHg to zooplankton ($2.59 + 0.99*\log (MeHg_{diss}$; $r^2 = 0.86$; $p < 0.001$) based solely on the strength of the regression relationships (Paterson et al., 1998). Moreover, in the L979 reservoir study, the residuals in the regressions were not significantly correlated with any physical factors (temperature, flushing rate), chemical factors (suspended carbon, particulate C:N, DOC, pH) or biological factors (phytoplankton biomass, 14C primary production, bacterial biomass, % Daphnia, zooplankton biomass), suggesting that the least complex, most direct, abiological process was involved in MeHg bioconcentration in zooplankton. Unfortunately, Pickhardt et al. (2002) did not report the unfiltered or filtered water column concentrations of MeHg or DOC at weeks 2 or 3 when the water fleas were sampled, so it is not possible to calculate the MeHg bioconcentration factors in algae or water fleas or to determine whether the BCFs are more strongly correlated with water column TP than the absolute concentrations of MeHg in algae or water fleas.

A third alternative hypothesis is that the concentration of water fleas, measured in number per liter, increased with increasing PO_4^{-3} dose, resulting in population dilution at the next trophic level. This would require that the concentration of water fleas be greater than that of the algae upon which they feed. In general, this would not be a sustainable food chain on a bioenergetics basis. Moreover, the average weight of a *Daphnid* in a eutrophic lake is about 50 micrograms (McIntosh et al., 2001). The concentration of *Daphnia mendotae* reported by the investigators was between 1-2 individuals per liter at 7.4 ug/L PO_4^{-3} to 2 - 9 individuals per liter at 44.4 ug/L PO_4^{-3}. If the weight of an individual *Daphnia mendotae* is on the order of 50 micrograms, then, at least, there were 0.1 mg/L of water fleas mixed in with 0.4 mg/L algae at 7.4 ug/L PO_4^{-3} and, at most, there were about 0.5 mg/L of water fleas mixed in with 1.4 mg/L algae. If water fleas are assumed to have about the same sorbing power as algae, and the MeHg on/in algae had to be redistributed among algae and water fleas, this would result in a roughly 9% and 20% decrease in MeHg on algae in the 7.4 ug/L PO_4^{-3} and 44.4 ug/L PO_4^{-3} tanks, respectively, in the absence of DOC in the tanks. With as the addition of as little as 0.75 mg/L DOC with the MeHg sorbing power of the DOC at WCA-2A-U3, the increase in biodilution due to the density of water fleas would be reduced to 3% and 9%, respectively.

A fourth alternative hypothesis is that rapid growth by the water fleas diluted the MeHg being transferred from the algae during digestion, and that this resulted in growth dilution of the MeHg transferred from the algae to the water fleas, masking the manifestation of trophic transfer. However, the correlation between water flea length and PO_4^{-3} dose was absent ($r^2 = 0.02$, $p > 0.59$.) weakening the case for this alternative hypothesis.

Whatever the cause of the absence of measurable MeHg biomagnification in the water flea element of the experiment, this should not be interpreted as compromising the validity of the results of the algae "bloom dilution" element of the experiment. However, the design of the study by Pickhardt et al. (2002) is not necessarily applicable to a shallow, subtropical wetland with dense stands of emergent and floating macrophytes that shade out algae in the most eutrophic areas (Grimshaw et al., 1997), where MeHg production occurs in algae mats (Cleckner et al., 1999), where high pore water sulfide appears to cause MeHg production to be lowest where carbon turnover by bacteria is highest (Gilmour et al., 1998a,b; 1999; Krabbenhoft et al., 2001), and where high DOC competes with organic particles for the MeHg produced. We will add this more detailed critique of Pickhardt et al. (2002) to both **Chapter 2B** and **Appendix 2B-5**.]

"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."

[The intent of this section of **Appendix 2B-5** was to summarize the recently published controlled microcosm and mesocosm studies of the sorption/uptake of MeHg by algae under controlled conditions. The summary includes but is not limited to the study by Pickhardt et al. (2002).]

"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 text will be corrected accordingly.]

"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."

[The original text of **Appendix 2B-5** will be modified to change "high P" to "conditions of P-limited exponential growth" and the word "concluded" to "hypothesized". In addition, the actual phosphate and nitrate concentrations used in the experiment will be reported instead of qualitative terns like "high" and "P-limited."]

"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."

[Quoting from the authors'abstract: "Partitioning constants determined with exponential and stationary growth phase *S. capricornutum* cells at the same conditions were not significantly different, while the partitioning constant for exponential growth phase, phosphorus-limited cells was significantly lower, suggesting that P-limitation alters the ecophysiology of *S. capricornutum* sufficiently to impact partitioning, which may then ultimately affect mercury levels in higher trophic species." The data in authors' Table 2 are consistent with this summary. Therefore, no change to the text is required beyond that agreed to above.]

"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)."

[The table will be revised per the direction of the lead author (A. Moye, UF, personal communication, 2002) to correct its errors. The abstract summary of the study results based on the corrected table is accurate as written.]

"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)."

[Commenter is not distinguishing between the types of biomass, the differences in their affinities for Hg(II) and MeHg, or the differences in the sources of Hg(II) and MeHg present in their tissues. Commenter omits important observation that MeHg concentration in periphyton biomass at highly eutrophic F1 is higher than at highly oligotrophic U3, contrary to the biodilution hypothesis. Further, when the concentrations of THg at F1 and U3 in periphyton, cattail leaves, and sawgrass leaves are multiplied by their respective observed coverages and measured biomass production rates, the mass of THg being sorbed, settled, and buried at U3 is higher than at F1. The District has hypothesized in **Chapter 7** of **ECR (2000)** and **Appendix 2B-5** of this report that the reason that this is occurring is because of shading of the periphyton mat by the dense cattail stands at F1 that are absent at U3 (Grimshaw et al., 1997). The former reference has undergone extensive peer review. Moreover, when the total mercury is broken down into Hg(II) and MeHg, cattail and sawgrass are biodiluting Hg(II) but concentrating MeHg, contrary to the biodilution hypothesis, and that the ability of cattail to bioconcentrate MeHg at F1 appears to be ten times that of sawgrass at U3. This is also contrary to the biodilution hypothesis. Most of the MeHg in fish at F1 is probably being imported in EAA runoff or produced in the periphyton mats or in the water column, because the rate of MeHg production in F1 sediments is too low to support the concentrations observed in water, sediment, and plant biomass, while most of the MeHg at oligotrophic U3 is being produced in the sediment and transferred to the food chain by predators preying on benthic invertebrates, although some of the transfer also occurs via the grazing of the periphyton mats. The MeHg production potential is higher at U3 than F1 (Gilmour et al., 1998a,b), probably because of the build-up of pore water sulfide at F1 three to four times that at U3 (Gilmour et al., 1999). Pore water sulfide is hypothesized to inhibit MeHg production by forming charged complexes or precipitates of Hg(II) that preclude its uptake by methylating bacteria (Benoit et al., 1999a,b; Jay et al., 2000; Benoit et al., 2001a). This is contrary to expectation that eutrophic areas will have higher rates of MeHg production, all other things being equal (Pak and Bartha, 1998). This, and not biodilution, is the leading hypothesis to explain the increase in MeHg bioaccumulation with downstream distance.]

"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. […in stationary and P-unlimited exponential growth but not P-limited exponential growth.] 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."

[Commenter is extrapolating static, closed system results to dynamic, open system conditions and ignoring all of the other processes that govern the concentrations of MeHg in water, sediment, organic particles, and aquatic plants and animals. The absolute concentration of MeHg in the water column is determined by the absolute rates of MeHg import, production, decomposition, and transfer to the other substantial compartments (from a mass standpoint) in contact with the water column (i.e., plant biomass; surficial sediment) together with the uptake and loss rates by algae in P-limited stationary growth phase, punctuated by occasional pulses of P-limited exponential algae growth. The magnitude of the MeHg partitioning on organic particles is dictated by the MeHg uptake and loss rates relative to the net rate of organic particle production, mediated by the concentration of DOC in the water column under the primary influence of pH, hardness, and total concentration of cations and anions (conductivity). If one presupposes that the magnitude of algae MeHg uptake alone determines the concentrations of MeHg in water, sediment, and aquatic plants and animals, then the observations of Miles et al. (2001) would be inconsistent with the observed concentration gradient in the already impacted area downstream of the District's S-10 structures in WCA-2A. However, this apparent discrepancy disappears when one accounts for all of the changes in key processes occurring along the nutrient gradient, including a four-fold increase in the concentration of MeHg in surficial soils between WCA-2A-F1 and WCA-2A-U3, suggestive of a substantial increase in the net rate of MeHg production. Rather than reject the results of a rigorously conducted study by Miles et al. (2001), it is preferable to modify one's conceptual model to accommodate all of the valid results of laboratory and field experiments, modeling, and mass balance calculations applicable to the already impacted areas of the Everglades.]

Mass Budget Analysis of Biodilution Hypothesis as Applied to the WCA-2A Nutrient Gradient

[Commenter had no comments on this section, including the observation that (1) biodilution is calculated to increase as water column TP decreases from F1 to U3, contrary to the hypothesis that a P-limited reduction in biodilution will cause an increase in MeHg concentration on periphyton biomass at U3 relative to F1, most probably due to light limitation of periphyton growth at F1; or (2) that cattail bioconcentrates MeHg at F1 more efficiently than sawgrass bioconcentrates MeHg at U3, also contrary to the biodilution hypothesis.]

"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."

[(1) The E-MCM (II) has been supplied to commenter for its review.

(2) E-MCM(I) was used in a pilot study to test the technical elements of the development of a mercury TMDL for the Everglades as the basis for regulating local air emissions sources. In that context and as part of that process, it has undergone scientific peer review. The changes in the structure of the model used for biodilution are the same as those made to MERC5 by Ambrose and Araujo (1998). Those changes and the results of their application to the evaluation of the mercury impacts of P reduction to the areas already impacted by excess P have undergone peer review (R. Ambrose, USEPA, personal communication.). Nevertheless, the changes to E-MCM(I) code to create E-MCM(II) have not been verified, nor have the results of the modified model been validated or peer-reviewed for application to the biodilution problem. That being the case, the author of Appendix 2B-5 has not drawn any conclusions regarding the significance of any particular process or influential factor or the post-restoration effects of reducing water column TP to 10 ppb in the already impacted areas solely on the basis of the results of mathematical modeling using E-MCM(II). Nor has E-MCM(II) been used as a predictive tool for regulatory application. Instead, E-MCM(II) has been used to organize, integrate, and ensure the self-consistency of the results of numerous, disparate monitoring and research studies in the Everglades.

Applying these same criteria to the biodilution hypothesis put forth by commenter, commenter's qualitative and quantitative predictions of post-restoration mercury impacts based on the biodilution hypothesis should not be used in a regulatory context until it and its predictions have undergone scientific peer review. However, in the case of E-MCM(II), the author believes that it would be a public disservice to delay the publication of relevant information in the ECR solely because it has not yet been fully published and peer-reviewed elsewhere. This is consistent with the position articulated by William Green of Hopping Green Sams and Smith on behalf of the Cooperative in a letter to the District's Garth Redfield, Ph.D., dated March 31, 2000:

"Second, the letter you sent indicates an intention by the district to exclude from the 2001 Report information not conveyed in published papers, official reports or publishable manuscripts, and data not sufficiently analyzed or quality assured. However, that approach might serve to improperly censor consideration of highly relevant data and analyses. That would conflict with the direction given the district by the Legislature to summarize 'all available data and findings' §373.4592(4)(d)5., Fla. Stat."]

"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."

[The District has often made extensive modifications to the draft chapter or appendices in response to salient peer or public review comments. The public is free to comment on the revised chapters or appendices from the preceding years in comments submitted on drafts of chapters or appendices in succeeding years. This has been done in the past, and the District has responded to those comments appropriately. These data have been available to the public for several years. The author of **Appendix 2B-5** has provided all of the surface water, pore water, soils, and mosquitofish data

to commenter during the comment period for independent analysis. The approaches to the data analyses iterated in **Attachments 1** and **2** were described in detail in the body of the report and are an extension of the analyses summarized there. The results also include the test of significance ($r = 0$) omitted from the preliminary analyses. However, the test of normality and lognormality and equal variance have not been completed as of this writing.]

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CHAPTER 3: PERFORMANCE AND OPTIMIZATION OF AGRICULTURAL BEST MANAGEMENT PRACTICES

PEER REVIEW PANEL COMMENTS

As in the past, the comments have improved Chapter 3 by identifying areas where further clarification is beneficial. Chapter 3 has been amended to address the major comments. Further explanations are provided below.

Below, repeated in bold, are the primary comments by the panel immediately followed by district responses (not bold).

An attempt should be made to explain the significant drop in phosphorus mass being discharged from the EAA. It was suggested in 2002 that a significant part of the decrease in phosphorus mass discharge may be attributable to the decline in the phosphorus fertilizer industry. Has an attempt been made to quantify the reasons for the decline?

An unidentified participant made this comment in the 2001 workshop (2002 report). As was discussed in the workshop, the District has not investigated whether there has been a decline in the fertilizer industry. This type of data is considered proprietary and is not generally available to the District. If there has been such a decline, we would also want to consider whether the decline was due to more efficient use of fertilizers brought on by BMPs, which is conversely a reasonable implication.

Does evidence exist to show whether or not phosphorus from the EAA originates from subsidence and mineralization of organic matter or from application of inorganic fertilizers?

P in the EAA comes from many sources, including subsidence, mineralization of organic matter, application of fertilizers, rainfall, etc. It is difficult to quantify all sources. Some information is not readily available, for example, data from certain business operations are considered proprietary. The transport mechanism for many of the contributors is surface water runoff. All of the BMPs are directed at minimizing discharges off site and/or minimizing or controlling the P sources that end up in discharges (e.g. control of particulate and sediment matter in discharges and fertilizer application). The rule specifically addresses the TP load leaving the EAA regardless of the sources.

Is the biogeochemical relationship between mercury and sulfur to be considered in the BMPs?

Please see Chapter 2 for a discussion on the Mercury issue.

Are hurricane effects taken into account when computing the annual baseline TP load? If so, what is the impact?

Rainfall amount, timing and distribution are taken into account when baseline TP loads are calculated to factor out their effects. In general, the impact of increased rainfall is increased runoff and its associated effects.

Have statistical analyses been performed to determine if the differences in base and BMP years are statistically significant? It is understood that these data are sums for the water year, but when the individual flows are multiplied by the 23-day average TP concentrations to obtain the mass of phosphorus removed, there probably are statistical implications that need an explanation.

EAA basin load is calculated using *measured* flow and TP concentration values. At District sampled sites, TP concentrations are based on an analysis of a 7-day composite sample. The composite is made up of many flow-weighted samples taken throughout the day for a given discharge event. This method provides more representative data for the actual load discharged than taking a single daily sample for TP analysis. The District has generally used the same or equivalent methods of sampling for both the pre- and post- BMP periods, so they should be statistically comparable; however, there has not been a statistical analysis to determine the difference between the methods or between the periods.

Are data available, other than rainfall, to assess annual percent variations in load variations? If such data are available, a brief summary would be helpful.

Rainfall is the only variable assessed and factored out in the load computations. The TP load is affected by many other variables but it is not feasible to track these separately. In determining WY load reductions, all other variables are assumed relatively constant between the pre-BMP period and the current water year with BMPs in place.

More discussion of impacts of other phosphorus contributors would be helpful in interpreting the impact of BMPs.

One major contributor of TP load in the EAA is Lake Okeechobee. Typically the TP concentrations of the lake discharges are higher when compared to the EAA discharges. Although lake "pass through" discharges are subtracted from EAA loads it is not possible with the available data to completely discount the effects of the lake concentrations on EAA canal water quality. Chapter 3 has been amended to make note of this.

Adding a sentence or two explaining sample processing and calculation procedures would be helpful to the reader.

The EAA is a complex system of canals that convey surface water flows from the EAA basin, the C-139 basin, STAs, and Lake Okeechobee to the south a majority of the time. All EAA boundary inflows and outflows are monitored for continuous flow and TP sample collection is automated

through the use of flow-proportional autosamplers. The TP autosamplers are programmed to collect composite samples on a 7-day cycle. The 7-day TP composite sample represents a weighted average and is multiplied by the daily average flow to obtain a daily TP load. The daily TP loads at all inflow and outflow sites are aggregated through a mass balance procedure to determine the net load attributable to EAA runoff. The net EAA TP load from the measured data is then compared to the base period predicted TP load (adjusted to account for the hydrologic conditions in the current water year) in order to determine the percent load reduction between the two periods.

What impact does the use of the average concentration multiplied by the intermittent flow rates have on the projected mass loading?

See responses above.

The [research] update would have been improved by presenting data from the studies. After 10 years of study, there should be many interesting results that could have been summarized in tabular or graphical format. Although much of the particulate phosphorus is in the form of biological growth, is there any indication as to how much of this growth is attached growth and transported due to turbulence or the mass that reproduces in the water body by extracting phosphorus?

Details of the research can be found in the IFAS report referenced in the Chapter.

Are future reductions in TP from the EAA to be modified, i.e., a cumulative percent reduction with some maximum reduction at which point further reduction is not expected?

No, the EFA requires only that the EAA achieve a 25% reduction when comparing each WY to the base period. There is not a requirement for a cumulative reduction or a maximum reduction. For information purposes only, the Chapter includes quantification of the TP prevented from discharging to the EPA (through implementation of BMPs) over and above that required by rule.

Conclusions

1. The BMP program has been successful in reducing the TP mass and concentrations reaching the Everglades.

2. To improve on the present program, it appears that phosphorus budgets are needed along with reduction of particulate phosphorus from the EAA.

3. Better statistical analyses of the TP reduction data are needed.

See Chapter 8A for a general load balance based on surface water runoff. Currently, data is not available to prepare an accurate phosphorus budget.

The mathematical model for rainfall adjustment includes a statistical analysis. The Chapter includes the confidence interval associated with the data. See Table 3-2.

THE FOLLOWING ARE RESPONSES TO US DOI COMMENTS DATED SEPTEMBER 24, 2002:

1. Although the EAA basin has consistently been in compliance with the 25% reduction requirement, permittees must continue to comply with the terms and conditions of their permits. They are also required to participate through the EAA EPD in research on BMPs. The UF/IFAS routinely holds well-attended workshops to educate the permittees on the latest findings on BMP efficacy. During routine inspections by the District to verify BMP implementation, staff and farm operators exchange dialogue on voluntary improvements that can be made relative to BMP implementation. Therefore, regardless of the status of the Agricultural Privilege Tax, BMP implementation is continually improving. As for the BMP points table – it is not an all encompassing list of BMPs. It is a list of the most common practices implemented in the EAA. BMPs do not have to be on the points table to be acceptable for implementation. BMP plans are routinely modified by permittees to best fit their current operation. Be reminded that the point system does not represent BMP relative effectiveness and that effectivenes is not solely dependent on the BMP itself. Its effectiveness, depends a great detail on how and where it is implemented.

2.& 3. With regard to conductivity, Chapter 3 specifically states that there is currently insufficient data to draw conclusions. Chapter 3 references cite the IFAS 10th Annual Report that is the basis for the research update portion of this chapter.

4. We must disagree with the comment that continued research, education and outreach are a waste of resources. Education and outreach is critical to proper and effective implementation of BMPs. The section of the chapter addressing how different variables affect water quality and the effectiveness of BMPs should be reviewed. It is because of these variables and the unique response to BMPs by individual farms that sharing information and making adjustments is a key aspect to the BMP success. The spatial diagrams of the farm data was included in the appendices this year as it was last.

Chapter 4: Responses to Peer Review Comments

This summary highlights significant changes made to Chapters 4A, 4B, and 4C in response to comments made by the peer review committee and other interested parties.

CHAPTER 4A

- An overall comparison of the STAs' performance was added to the chapter as Table 4A-1.

- A detailed description of the anomalous mercury event in STA-2 is presented in Chapter 2B and Appendix 4A-7.

- STA-specific discussion was added to explain the few periods when monthly outflow concentrations exceeded inflow concentrations.

- STA-specific measures for the control of exotic and undesirable plant species is addressed in the chapter.

- Figure 4A-7 was revised to be consistent with the similar figure in Chapter 2. Additional revisions will be made for next year's report to fully address the Department of Interior's suggestions.

- STA performance tables were updated based on revised data for Water Year 2002.

- The Corps is developing a vegetation management plan for STA-1 East that will address vegetation community type (e.g., emergent, SAV and/or PSTA). The District is finalizing the vegetation management plan for STA-3/4 and this will be summarized in next year's *ECR*.

CHAPTER 4B

- Scaling issues relative to the STA-1W test cells were addressed briefly in this chapter and the reader was directed to a more detailed discussion of this issue in the *2002 ECR*.

- The District has initiated a program to monitor sediment and vegetation within all the STAs currently in operation. In addition, instrumentation to monitor flow and water quality conditions within the interior of each STA is now coming on-line. The objective is to correlate differences in STA treatment performance with changes in vegetation and sediments. However, only limited data are available to date from these sampling efforts. We have deferred a detailed analysis of STA performance until there are sufficient data for a comparison across all STAs.

- Legends were added and the color contrast between bars enhanced to make Figure 4B-7 easier to interpret.

- The description of the pulsed HLR experiment conducted in the STA-1W test cells was rewritten to make it clearer.

- The District conducted two different sets of tracer studies in the test cells, one set in April 2001 and the other in July 2001. The description of these studies was reworded to make it clear that we are talking about two separate efforts.

- We deleted the reference to tracer curves with a N > 4 as tending toward plug flow conditions.

- The duration of all experiments conducted in the STA-1W test cells has been added to Table 4B-1. The length of the pulsed/depth experiments (177 to 188 days) was increased over earlier experiments (98 to 105 days) in an attempt to address concerns raised about HRTs. There were "stabilization" periods, ranging from 37 to 142 days, between the end of the HLR experiments and start of the pulsed/depth experiments in all but one of the test cells.

- Where appropriate, sections have been added to the chapter to establish the linkage between STA Optimization research and monitoring efforts and STA management.

- The results of statistical tests of significance performed on results from the STA-1W test cell experiments have been added.

- The discussion of the hydraulic tracer studies conducted in the STA-1W test cells has been simplified.

CHAPTER 4C

- Table 4C-3 has been added to summarize the Supplemental Technology Standard of Comparison results for Chemical Treatment with Solid Separation (CTSS), Submerged Aquatic Vegetation (SAV), and Periphyton Stormwater Treatment Area (PSTA) technologies.

- Hydraulic residence times have been included along with the hydraulic loading rates to our discussion sections.

- The dye tracer analysis discussion has been modified to focus on the mean and variance of the tracer data as opposed to fitting a tank-in-series model to the data.

- The discussions regarding the PSTA and SAV models have been clarified.

- The discussion and conclusion of the toxicological results for CTSS, SAV, and PSTA have been greatly modified to reflect past discussion with the Environmental Protection Agency, Department of Interior (DOI), and the Florida Department of Environmental Protection (Department) regarding the uncertainty associated with these results.

- When possible, the discussion section for all technologies was modified to include management implications.

We were unable to address several comments regarding compatibility of the STA outflow with the downstream receiving water because the appropriate data have not been collected. Currently, the District is involved in discussions with the Department and the DOI to design experiments to address this concern.

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

Grover Payne, Kenneth Weaver, and Temperince Bennett

Responses to Peer Review Panel Comments

Comment 1: *The conclusions regarding the biological responses to water concentrations of total P are confounded by the gradient of soil P concentrations, however, which makes it impossible to attribute effects solely to surface water concentrations.*

Response: The authors do not agree with the comment. The Department is legally required to establish a long-tem P criterion that is protective of the natural flora and fauna. Since the P is a nutrient and not acutely toxic, the long-term chronic effects of P-enrichment (including effects resulting from long-term accumulation in the sediment) must be considered in establishing a criterion. Therefore, it does not matter if the biological effects observed are a direct effect of the elevated P in the surface water or a secondary effect of the P accumulated in the sediment from the water column, all of the effects are ultimately tied to the elevated P loading to the system occurring through the surface water and must be considered in developing the water quality criterion.

Within the remaining portions of the Everglades, there is no evidence of a historical sediment P gradient within any of the water conservation areas. There may have been localized areas of enrichment adjacent to rookeries or other natural perturbations. However, since the gradient transect sites were located to avoid these areas they are unlikely to have influenced the results of the study.

The existing sediment gradients within each area of the EPA have formed in response to being loaded by elevated P levels from surface water inflows. This is reflected in: (1) the strong correlation between surface water and sediment P concentrations; and (2) the negative correlation of both surface water and sediment P concentrations with distance from the inflow structures. Elevated soil P concentrations are due to a large percentage of the phosphorus accumulating in the sediment through greater production and subsequent higher peat accretion rates, direct adsorption of the phosphorus into the sediment, and precipitation. Thus, elevated sediment phosphorus concentrations are essentially a component of the imbalance, which can be used to integrate surface water phosphorus conditions over time. Sediment phosphorus concentrations have commonly been utilized by researchers to demonstrate areas where phosphorus enrichment has occurred. Results presented at the 2001 Peer-Review Workshop suggest that the area of enriched sediment is continuing to slowly expand as the result of continued loading from the enriched water overlying the sediment.

Since the sediment P gradients occurring in conjunction with the surface water gradients in the water conservation areas are a direct result of the long-term enrichment of the water column, it is not necessary to differentiate the effects caused directly from the enrichment of the water column from those more strongly related to enrichment of the sediment in the derivation of a long-term water quality criterion.

Comment 2: *The Class III narrative nutrient criterion states that "in no case shall nutrient concentrations of a body of water be altered so as to cause an imbalance in natural populations of aquatic flora or fauna." In determining an imbalance, it might be appropriate to consider the nature of the harm to the system. For example, degrees of imbalance or harm occur over a continuum (e.g., a negative effect that is very temporary, a negative effect but over time the system will recover, and a negative effect that is permanent), and some imbalances occur naturally. In establishing a P criterion, should consideration not also be given to the type of imbalance that is occurring under phosphorus enrichment at different levels and in different parts of the Everglades system?*

Response: We agree with your comment. The Department did consider the nature of the system changes in determining where imbalances occurred and did not consider temporary of insignificant changes as an imbalance. Instead of relying solely on any one indicator of imbalance, the Department used a "weight of evidence" approach consisting of multiple significant changes to the structure and function of biological communities at multiple trophic levels. It was the total of these effects that was used to define when an imbalance had occurred.

As presented in previous reports, the Department's analysis of the data collected from the gradient transects indicates that many of the significant biological changes observed across trophic levels occur at the same location along the gradient at the same levels of P-enrichment, even though some communities respond more quickly than others. Because of the complex interrelationships among biological communities and trophic levels, phosphorus enrichment results in major shifts in the taxonomic composition and function of the biological communities across several trophic levels. Although these changes occur at different rates, they will all eventually occur at the same level of P-enrichment if given sufficient time.

It is also important to note that during the Department's analyses, highly sensitive and rapidly changing biological indicators that respond to very low levels of enrichment were examined, including alkaline phosphatase activity, P concentrations in periphyton tissue, metabolism rates, and microbial activity, but these indicators were not directly utilized in the derivation of the criterion. The responses of these highly sensitive biological indicators to P-enrichment have been summarized in previous versions of the chapter with a more detailed discussion being provided in the Department's technical support documents for the P criterion.

Comment 3: *It is unclear why a mixing zone in the Everglades wetlands is not considered acceptable. Is there a scientific reason for this? Such mixing zones are common in the monitoring of effects in aquatic systems.*

Response: A mixing zone is created when a discharge is being mixed with the receiving waters and a certain period of mixing (dilution and dispersion) is required before the water meets the criterion. The Everglades is a relatively unique case in which the permitted discharges comprise most or all of the hydraulic inflows to the system except direct rainfall. Therefore, the normal dilution and dispersion processes functioning in typical mixing zones is largely absent in this case. In the Everglades, the P concentrations in the surface water decreases with distance

from the inflows as a result of biological and geochemical sorptive processes. However, as was described during the Peer-Review Workshop, there are other moderating provisions that may be available and appropriate to accommodate the recovery of the impacted areas.

Comment 4: *The Review Panel believes that the body of scientific evidence supports the view that meeting the 10 µg/L P level will be protective of the natural flora and fauna of the Everglades. However scientific evidence does not support the view that this is the only level of P what would be protective of the Everglades biota, nor is there scientific evidence that the 10 µg/L standard is appropriate throughout the entire EPA. The Everglades is now, and was in the past, a heterogeneous wetland ecosystem, with spatially and temporally variable water flows, levels, and chemistry. The application of a uniform P criterion across the entire Everglades may not promote patterns of variability that are naturally characteristic of this system.*

Response: The Department and District agree that the Everglades is a heterogeneous ecosystem. It must also be understood that there is overwhelming evidence that the remaining portions of the Everglades (the Everglades Protection Area) developed and exist today, under highly oligotrophic conditions. Evidence indicates that historically, most of the biological heterogeneity resulted from variations in hydrology, not differences in P levels.

While some of the biological communities (such as tree islands) can tolerate higher P levels, they all generally originated under oligotrophic conditions and exist today as "islands" surrounded by the highly oligotrophic marsh. There is absolutely no evidence that these areas require higher P levels to survive. It would be inappropriate and unnecessary for a different P criterion to be developed for each habitat type, since the proposed criterion was derived to be protective of the most sensitive biological communities (the oligotrophic portions of the system), as is typically done for water quality criteria. By analogy, just because some lake bottom populations can tolerate lower levels of DO, a new criterion is not developed for lake bottoms. Instead, compliance with the DO criterion is typically not determined using bottom samples. It is evaluated using an average throughout the water column or mid-depth samples, which is more practical.

In addition, it must be recognized that the measurement methodology proposed by the Department also recognizes and accommodates the heterogeneous nature of the ecosystem. In the Everglades, the most sensitive communities are generally found in the open water/slough habitats. Therefore, the gradient transects monitoring sites were located in this type of habitat. Additionally, the monitoring sites under the proposed measurement methodology will also be located in similar habitats. For example, the proposed methodology specifically excludes P data collected in the vicinity of tree islands because it is recognized that those areas are likely to have naturally higher P concentrations. It is important to understand that the Department does not propose that the criterion be applied at the "end-of-pipe" or at all places in the marsh. By deriving the criterion based on the sensitive habitat and performing the criterion monitoring in the same habitat, in no way is the heterogeneous nature of the ecosystem being compromised. Additionally, if the habitat changes over time in response to changes in hydrology or other natural phenomena, the Department has proposed mechanisms to allow either the influenced data to be omitted from the criterion evaluations or the affected site to be relocated as necessary.
It has been suggested by some commenters that the Department should recognize that a natural P gradient existed in the Everglades historically as a result of overflow from Lake Okeechobee. The Department understands that there is some evidence that a biological community existed just south of the Lake (prior to development of this area for agricultural purposes) with moderately enriched P levels in the water column. However, the Department disagrees that the existence of

such a system south of the Lake that did not extend into the Everglades Protection Area has any relevance to the development of a P criterion for the remaining historically oligotrophic portions of the Everglades. No evidence has been presented that the moderately enriched forested (i.e., Pond Apple) community that existed south of the Lake could be recreated in the portions of the Everglades Protection Area currently impacted by P-enrichment. In fact, differences in soil type, hydrology and other factors make that unlikely. Prior to the discharge of P enriched water to the Everglades Protection Area due to agricultural run-off the biological communities in that area were naturally oligotrophic. The naturally oligotrophic conditions dictate the numerical P criterion and the restoration goals under the Everglades Forever Act.

Comment 5: *FDEP has examined reanalyzed data from dosing studies performed by the Duke University Wetland Center and reported that they are consistent with the 10 µg/L criterion. The Review Panel is not convinced that the two pieces of research are consistent in supporting this criterion. There is obviously a difference of opinion, and FDEP and District scientists are encouraged to meet with DUWC scientists to review the data analyses and work to resolve differences of opinion. For example, it might be appropriate, in the data presented in Figure 5-3, to remove the % diatoms (plexiglass) from the analysis and recalculate the geometric mean change points and 95% confidence levels. It also may be necessary to recognize that because of natural heterogeneity in the Everglades ecosystem, as well as differences in experimental or sampling procedures, there may be differences in responses to levels of P loading.*

Response: As presented in the 2001 Everglades Consolidated Report Chapter, the Department has numerous concerns about the design and operation of the DUWC dosing study as well as the use of the resulting data for the purpose of P criterion development. The issues with the DUWC work identified by the Department in Payne et al, 2001 include: 1) Nearly half of TP data from the DUWC dosing study were associated with blank contamination; 2) TP measurements from dosing flumes were much more variable than at reference sites along the gradient; 3) Extent of biological change observed in the DUWC flumes was limited by the size of the flumes and the relatively short duration of the study; 4) The appropriateness of the statistical methods used to analyze the data; 5) The appropriateness of using a short-term dosing study to derive a long-term criterion; 6) The use of short-term 2-6 month geometric mean TP concentrations comprised of a few highly variable measurements does not adequately represent the P regime experienced by the biological communities; 7) The development and applications of the "one-gram" rule that has been seriously questioned in the peer-reviewed literature to support the results from the dosing study; and 8) The use of an average change point as the criterion is not adequately protective of the natural flora and fauna. The text of Chapter 5 was revised to reemphasize the importance of these issues relative to the derivation of an adequately protective P criterion.

Additionally, it should be noted that Department staff have made a continuous effort to resolve these issues with DUWC scientists (and any other interested party) with limited success. A significant difference in the conclusions drawn by the Department and those of the DUWC stems from the interpretation of the research results relative to the derivation of an adequately protective criterion. The Department is legally required to establish a criterion that "In no case shall…allow waters in the Everglades Protection Area to be altered so as to cause an imbalance in the natural populations of aquatic flora and fauna." Establishing a criterion that, according to figures provided by DUWC researchers would allow significant shifts in the structure and function of several biological communities across multiple trophic levels and does not satisfy this legal requirement. We agree with the Panel's suggestion that a review of the biological indicators and the removal of those not found to be adequately sensitive (such as % diatoms on Plexiglas)

from the DUWC analysis would help resolve the difference in the DUWC and Department conclusions.

Comment 6: *Furthermore, at the Public Workshop, additional critiques of the development of the 10 µg/L criterion were presented by the Community Watershed Fund, and these comments should be considered by the District. Additionally, an alternative to the 10 µg/L P standard was presented by the sugar industry. This material has not been peer-reviewed or presented in the open scientific literature, and thus the Panel cannot comment on it. The continued debate around the 10 µg/L recommendation of the FDEP, that of DUWC at 15.6 µg/L, and that of the sugar industry of 16 µg/L should somehow be resolved. All of these recommendations are supported by research. Some practical agreement must be reached to this issue and the sooner the better. It has been publicly stated that if a firm number is adopted, it should be applied in the context of long-term management and cost realities and by using averages over an appropriate and rolling time period, allowing for levels to exceed established limits in a given timeframe.*

Response: As indicated above, the Department has made and continues to make a concerted effort to resolve issues relative to the development of a P criterion with DUWC scientists and any other interested party. However, the Department is required to establish a P criterion that is scientifically valid and legally defensible.

As stated in the chapter, it is intended that the criterion be applied to maintain the portions of the system currently minimally impacted by P-enrichment while promoting the "net-improvement" of the currently impacted areas. Additionally, the proposed measurement methodology applies the criterion based on long-term geometric means and allows for natural variability within the system. Further, as described in Chapter 8, the District has drafted a Basin Specific Feasibility Study report that examines the economic costs as well as other costs associated with various methods of meeting the proposed criterion in each affected basin. Additionally, the Department has acknowledged that some form of moderating provision may be necessary while the effectiveness of the economical and technologically feasible treatment systems continue to be developed.

Comment 7: *The Summary, in the second paragraph, mentions a three-pronged research agenda consisting of field transect monitoring, field dosing experiments, and laboratory experiments. No further information about laboratory experiments, or data from them, appear to be used in the development of the numeric P criterion, or in supporting the 10 µg/L. Were data from such experiments used in this effort?*

Response: The District and Department utilized the data from the laboratory experiments to help understand the mechanisms involved in biological shifts observed as a result of P-enrichment. While these studies provided much information about how and why the biological changes occur, they were not designed or intended to be used to derive a numeric criterion. A limited amount of the data from the laboratory and greenhouse studies has been presented in previous versions of the Everglades Consolidated Report. The text in Chapter 5 was revised to clarify how the data from each type of study was utilized by the Department during the development of the proposed P criterion.

Comment 8: *On page 5-24, the section labeled Adaptive Management does not describe an adaptive management approach. It only describes a review 10 years after the adoption of the P criterion. Adaptive management would use information gained from the first years of*

application of the P criterion to inform the management process and make adjustments as needed. It is not clear if or how this will be carried out.

Response: The Adaptive Management section is intended to indicate that the numeric P criterion and measurement methodology will be reviewed periodically and revised as deemed appropriate. The P criterion as well as all other water quality criteria approved by the Department undergo an evaluation as part of the triennial reviewed process. Additionally, more thorough evaluations of the criterion and measurement methodology will be conducted at longer intervals.

Comment 9: *What methods, statistical or otherwise, will be used to 'optimize' the current monitoring network to provide 'adequate' spatial coverage? How is 'optimize' defined in this context (i.e. what is the objective function for the optimization process)? How will 'adequate' spatial coverage be defined and quantified?*

Response: In this context, 'optimize' includes the above statistical analysis to assure that the number and location of monitoring sites are adequate to characterize all portions of the EPA along with the determination that the number of monitoring sites is not excessive (well above the number needed to characterize an area). Based on the natural spatial and temporal variation observed historically, the number of monitoring sites needed to adequately characterize each portion of the EPA with a specified degree of certainty can be estimated using common statistical methods.

As specified in the chapter, monitoring stations shall be dispersed throughout areas that are both impacted and unimpacted by phosphorus enrichment. Since the intent of the criterion is to protect the phosphorus sensitive biological communities while maintaining the natural heterogeneity of the system, the monitoring sites for the P criterion will be located in the open water/slough habitats where they primarily exist. The location of the monitoring sites in these areas is further supported by the fact that the open water/slough habitats represent a critical component of the ecosystem and comprise a large portion of the EPA. The exact location of the monitoring sites will be verified based on a ground truthing exercise to assure that the habitat and biological communities at each location are appropriate.

Comment 10: *The sampling frequency discussion on page 5-23 is confusing. What does 'six monthly samples' mean? Is it expected that as much as six months each year it will not be possible to obtain valid samples? How does this apparent low sample size mesh with the goal of at least 20 samples required for standard compliance computations described in Chapter 2A? Cross referencing P measurement with methods used for other water quality constituents avoids confusion if the methods are different, and they appear to be in this case.*

Response: During some years, the marsh dries out and it is physically impossible to obtain valid samples during these periods. Additionally, with a monitoring program of this magnitude it is unavoidable that some number of samples will be "lost" due to QA/QC problems or will be deemed invalid for some other reason. Since the P criterion is based on an annual geometric mean, it was necessary to evaluate the minimum number monthly samples necessary to adequately characterize the P regime during any particular year. Based on the evaluation of historical data, the Department concluded that a minimum of six monthly samples was necessary for this purpose. The purpose of this was to avoid the case in which an annual geometric mean was based on one or two measurements that do not adequately characterize the P regime experienced during that year.

The respective sample sizes were selected to manage statistical uncertainty for the statistics utilized. The excursion analysis methodologies presented in Chapter 2A and the TP measurement methodology address different underlying premises, have different purposes, and were designed to manage uncertainty in accordance with the statistics utilized. The underlying premise of Chapter 2A is that a parameter is of management "concern" (potentially impairing use) if its true exceedance probability exceeds 10 percent. In contrast, the P criterion measurement methodology was formulated based on the premise that an imbalance of flora and fauna will occur in the marsh if the true long-term (5-year) average (geometric mean) TP concentration exceeds 10 µg/L. The true value (probability or average) cannot be measured or known by either test; rather the value must be estimated from a set of samples, which introduces statistical uncertainty. Uncertainty can be evaluated as the probability of statistical errors. Water-quality managers must be concerned with both Type I (false positive) and Type II (false negative) errors and should design statistical tests to manage these error rates at acceptable levels (10-15%). Because the two methodologies are evaluating different statistical measures (means versus probabilities) the rates of these errors are effected differently by sample size. For the binomial approach utilized in the final draft of Chapter 2A, sample sizes of at least 28 balance average errors rates at acceptable levels. In the case of the P-criterion measurement methodology, Monte-Carlo simulations based on sample variance observed at WCA-2A reference sites and first impacted sites (E4 and F4) indicated that a sample size of at least 4 to 6 samples was required to adequately manage both type I and II error rates. To be conservative and minimize uncertainty a minimum sample size of 6 was selected to annually measure the P criterion.

In the future, it is not envisioned that the binomial approach used in Chapter 2A will be applied to the P criterion measurement methodology. Unlike most other numeric water quality criteria, which are expressed as single-sample maximums (mostly aquatic life-based), the proposed P criterion is based on an annual geometric mean and incorporates a measurement methodology that accounts for the natural variability. As stated above the P criterion is predicated on the idea that long-term (5-year) geometric mean TP concentrations exceeding 10 µg/L will result in an imbalance of Everglades flora and fauna. Applying an additional 10% excursion frequency onto the measurement methodology would effectively allow 10% of the marsh to exceed the P-criterion. This clearly would not be protective of flora and fauna across the entire EPA. It is likely that future editions of Chapter 2A will present a TP report card including a figure similar to Figure 2A-8 depicting stations "passing" or "failing" the measurement methodology.

Comment 11: *How is "statistically significant departure" determined? Is the current year's data tested against all previous data, or only the previous year's? Has the role of different numbers of samples in testing two populations been considered?*

Response: During the Department's analyses, statistically significant departures from the normal unaltered structure and function of the ecosystem were determined by comparing the structure and function of various biological communities across several trophic levels at the least impacted sites (i.e., sites farthest away from the source of enrichment) to sites as you move up the P gradient closer to the source using a combination of cluster and change point analyses. Since sites along the P gradient have been impacted for 30 to 40 years (prior to any biological measures) it was impossible to examine the temporal changes. Instead the changes were examined spatially along the gradient. Therefore, the different number of samples collected from year to year was not an issue.

Comment 12: *Are the only reasons that outlier data may be deemed 'not valid' listed at the top of page 5-24? Or is the analyst reserving the right to identify other reasons to declare data to be invalid? If the latter is the case, what stops stakeholders, displeased with the findings, from requesting other data be declared 'not valid' for reasons they deem sufficient? How will such differences of opinion be resolved in producing widely accepted agreement on P standard compliance in the EPA?*

Response: The list of reasons to deem outlier data 'not valid' is not exhaustive. As required by existing law (Section 403.021(11), F.S.), the Department will not consider deviations from the water quality criterion to be violations where the deviation would occur absent man-made discharges to or alterations of the water body. Instead, the Department will make the determination on what data are 'not valid' and document the reason any data are omitted from the evaluation. During the course of development of the P criterion rule, the Department expects to solicit further public comment on what data should be deemed not valid. Where appropriate, the list of factors identified in Chapter 5 may be expanded and adopted as part of the final rule. Additionally, it is important to understand that the reasons to deem data 'not valid' only apply to data identified as statistical outliers as specified in the referenced rule. Therefore, only a very small portion of the monitoring data will ever be in question.

Comment 13: *The proposed measurement methodology is declared objective and scientifically reliable on page 5-24. Has the methodology been peer reviewed? Or is the judgment based on the example presented in Table 5-5?*

Response: The example presented in Table 5-5 was meant to provide an example application of the proposed measurement methodology as well as an indication of the accuracy of the methodology in delineating impacted from impacted sites. The judgment above was based on numerous evaluations of the methodology from throughout the EPA performed by the Department as well as comments from well-known peer-reviewers outside the agency with expertise in wetland ecology, development of monitoring programs, and statistics. The peer-reviewers include Bob Kadlec, Bill Walker, Jan Stevenson, and Jim Karr.

Comment 14: *On page 5-23, it is noted that a 'District Technical Publication' will be prepared that describes the monitoring network and Standard Operating Procedures to be used to obtain samples form the network. Hopefully, questions like those above will be addressed when more effort is devoted to precisely defining a total phosphorus standard compliance monitoring program, from sample collection through data analysis and interpretation. Once drafted, before it is formally used to produce a standard compliance assessment, will the publication be peer reviewed?*

Response: It is anticipated that the 'District Technical Publication' will fully address the development of the monitoring network, sampling requirements, and data handling not addressed in the rule or other existing documents. The document will be open to comments from interested parties and peer reviewers during its development and prior to finalization.

Comment 15: *The overall water quality goal of requiring a numeric P level of 10 µg/L on a system wide basis for the Everglades region has been discussed since the public review process was initiated. The Review Panel's understanding is that the FDEP supports adoption of this criterion on a landscape scale (end of pipe with no mixing zone) rather than supporting the opinion that a mixing zone is logical. A system-wide single criterion also presents problems in*

not recognizing the variability found in all natural systems. The Panel continues to express concern as to the logic, given time and cost restrictions, of trying to restore a very complex and large area to a given point, as driven by total P, and then trying to maintain it at that point for all of South Florida. Natural systems continue to evolve and are not static. While to some this seems to be a very effective manner in which to judge the success of the restoration effort, it may result in inordinate costs to the District and a number of related environmental risks and management difficulties. It also provides only a snapshot of a given area at a point in time and says little about the long-term management of the region. Furthermore, the ability at an operational scale to treat water to the proposed criterion has not been demonstrated.

Response: The Panel appears to misunderstand the Department's position. The Department has never suggested that the criterion be applied to "the end-of-pipe" or at every point in the system. The proposed criterion was developed to protect the more P-sensitive biological communities generally found within the open water slough habitats. Likewise, the criterion will be applied to monitoring sites representative of these sensitive habitats. This approach will allow for the characteristic heterogeneity while protecting the sensitive portions of the marsh. This approach is further supported by the fact that the open water/slough communities have been shown to be critical to maintenance of the normal structure and function of the ecosystem as well as comprising a large portion of the landscape.

As discussed previously, it must be understood that the remaining portions of the Everglades developed and existed, with the exception of localized areas adjacent to rookeries and other natural perturbations, under highly oligotrophic conditions. While it is true that the natural system will continue to evolve, it is also true that without the input of excessive amounts of P, this evolution would have occurred under highly oligotrophic conditions at a very slow pace. Allowing excessive levels of P in the remaining Everglades will accelerate the eutrophication of the system and will not protect the existing natural populations of flora and fauna. Historically, most of the natural changes in the system appear to have resulted from changes in hydrology not changes in P levels. The proposed criterion is not intended to "force" the system to remain static. Instead, it will allow the natural evolution to occur at a more natural pace.

As discussed above, the panel also misunderstood the Department's comments on mixing zones. The Department staff stated that in the context of the Everglades Protection Area where most or all of the hydraulic inputs, other than direct rainfall, are through the permitted discharges, the regulatory definition would not apply. However, it was noted that other moderating provisions or relief mechanisms may be available.

Comment 16: *It is still not clear the public understands that the Restudy Bill authorized the District to construct pilot projects to help determine the feasibility of technologies included in the comprehensive plan (2001 Everglades Consolidated Report, page 10-1, summary, paragraph 2). The current report should clearly state that testing these technologies does not imply that they are proven. The District runs the risk of bad press and misunderstanding on the part of the public, environmental risks, and wasting money if assumptions are made based on either incomplete science or unachievable goals.*

Response: The above comment appears to be focused on the content of another chapter (possibly Chapter 7) since the pilot projects authorized by the Restudy Bill are not relevant to the development of the P criterion.

Comment 17: *Even though the Review Panel understands the position of the National Park Service, presented in the Public Workshop, that "The demonstration that the 10 µg/L criterion is supported by a variety of approaches is particularly effective," some members of the Panel still consider the numeric P criterion a general goal rather than a legally binding definition of success, and recommend using a range of numeric values based on a number of environmental and managerial criteria, along a transect north of the ENP. Further, they note the need to recall that a given technology may be locally feasible, but not viable within a different context or larger region. Some effort should be made to explain this context if only in a footnote.*

Response: The Panel's comment is not fully understood by the authors and appears to be directed more to address the comments made by the National Park Service. The comment also appears to merge the proposed numeric P criterion with Everglades restoration goals under CERP. The P criterion can be used in CERP as a performance target just as many other water quality criteria are used as performance targets for the CERP projects. Additionally, once adopted, the numeric P criterion will be as equally binding as any other water quality criterion adopted by the Department.

Given the legal requirements for this criterion and the nature of the remaining Everglades ecosystem, the use of "a range of numeric values based on a number of environmental and managerial criteria, along a transect north of the ENP" would not be legally possible or technically sound since this range of values would not afford equal protection to all parts of the system (assuming the authors understand the recommendation).

Comment 18: *The NPS raises a very interesting point concerning the validity of Figures 5-4 through 5-7. If the network was not designed to measure compliance in the existing marsh network, then continuing to collect data at these locations without expanding the spatial coverage may not be adequate for validating compliance in the long-term.*

Response: The monitoring sites identified in Figures 5-4 through 5-7 only represent sites existing as part of the District's current ambient monitoring programs. As stated in Chapter 5, the Department only intended these sites to serve as a starting point in the development of a more appropriate network to be used for P criterion monitoring. Furthermore, the Department also acknowledged the probability that sites would likely need to be added, deleted, or moved to develop a network adequate for this purpose.

Responses to Department of Interior Comments

Comment 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.*

Response: The authors agree with the comment. While the Department's analyses indicate that the significant biological changes along the WCA-2A gradient occur over a relatively narrow range of P concentrations, the use of the EPA recommended 75th percentile does help verify that the proposed 10 µg/L criterion is appropriate.

Comment 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.*

Response: The authors disagree with the comments that the incorporation of the pass in any year when the GM is below 10 ppb, regardless of the 5-year history weakens the test. Based on evaluations of the measurement methodology performed by the Department, including the example provided in Table 5-5, the inclusion of the single year criterion does not cause the first impacted sites up the gradient from the reference sites "to bounce in and out of compliance from one year to the next as a result of natural variability".

Additionally, the incorporation of the single year criterion does not minimize the importance of the 5-year average. Some biological communities (such as periphyton and macroinvertebrates) respond to P-enrichment over time-periods of 1 year or less while others (such as macrophytes) may respond over periods greater than a year. Additionally, the 5-year time frame is sufficiently long to account for natural variability.

If the single year 10 µg/L criterion was deleted as recommended, it would be possible for a single year with a high P level to result in a station failing the test for five years even though the P levels during four of the five years were below 10 µg/L. This would result in an elevated Type I error rate. Since the proposed test fits the historical data well and balances the Type I and Type II error rates, the recommended change does not appear warranted.

Comment 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.*

Response: The text in the draft chapter available on the District's web-site is correct. The error apparently occurred during downloading or editing. The text will be rechecked prior to finalizing the chapter.

Comment 4:*. The 1-year limit of 15 ppb is described as the "the upper 95th 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 90th 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 with the precedent established elsewhere in the Everglades and would reduce the 1-year limit from 15.1 to 13.8 ppb.

Response: During the derivation of the upper annual limit, the Department calculated the limit using multiple methods and percentiles. Based on an evaluation using historical data, the use of the 95th percentile more fully captured the magnitude of natural variation within the reference sites without significantly altering the balance between Type 1 and Type II error rates. Additionally, the use of the 95th percentile is also consistent with the use of the 95% confidence interval in the derivation of the numeric criterion. Since the use of the upper limit based on the 95th percentile fit the historical data better, it was selected over simply being consistent with the calculation of limits for other purposes. Additionally, the Department's analyses also indicate that the failure rate for the measurement methodology is primarily driven by the requirement that the 5-year average annual geometric mean be maintained at 10 µg/L or below. Therefore, the slight 1.3 µg/L decrease in the upper limit has little effect on the overall results of the measurement methodology.

Comment 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.*

Response: The monitoring sites identified in Figures 5-4 through 5-7 only represent sites existing as part of the District's current Environmental Monitoring and Assessment (EMA) program. As stated in the Chapter, the Department only intended these sites to serve as a starting point in the development of a more appropriate network to be used for P criterion monitoring. Furthermore, the Department also acknowledged the probability that sites would likely need to be added, deleted, or moved to develop a network adequate for this purpose. The associated text in the chapter is intentionally vague to allow for appropriate changes in the location and number of sites during the development of a monitoring network.

Also, in addition to the monitoring network specified in the measurement methodology, there will be additional monitoring sites in the vicinity of permitted discharges that will allow a more detailed evaluation of the area potentially influenced by the discharge. The location of these sites will be developed specifically for each discharge and will be specified in the permit for that facility.

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

Response: The monitoring sites in the Figures reflect existing ambient monitoring sites. The additional sites referenced in the comment were established to evaluate permit compliance. Since permit compliance and ambient monitoring are distinctly different issues, these sites are not considered appropriate for the monitoring of ambient conditions and have not been used for that

purpose elsewhere. However, those sites (or a similar group of sites) will likely continue to be monitored for permit compliance issues.

Responses to USEPA Comments

Comment 1: *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.*

Response: The range of values specified in the text was taken from the range of annual geometric means for individual sites while the median was taken from the annual geometric means for the group of five reference sites. This will be clarified and revised in the chapter as appropriate.

Comment 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 . The same statement is found in "USEPA, 2002. Water Quality Criteria, Nutrients, Frequently Asked Questions, Question 5." See . The 75th percentile concept, but not this exact quote, is also found in various national nutrient criterion guidance documents.*

Response: The USEPA citation will be revised accordingly.

Comment 3: *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?*

Response: 1) The maps in Chapter 2A include monitoring stations in canals and at water control structures that may not be appropriate for the P criterion monitoring. Additionally, the Chapter 2A maps include sites from monitoring programs not incorporated into the Chapter 5 maps. The Chapter 5 maps will be updated to include these sites where appropriate.

2) While it is true that the existing sites were not established for P criterion monitoring, they do represent a suitable starting point from which a more appropriate monitoring network can be developed to satisfy the intended purpose.

3) The Department also acknowledged the probability that sites would likely need to be added, deleted, or moved to develop a network adequate for criterion monitoring.

4) It is anticipated that during the development of the criterion monitoring network that an effort be made to utilize as many existing monitoring sites as possible, regardless of source. This not only minimizes the cost associated with performing the monitoring, it also allows for the maximum use of the data collected at any location.

Responses to Community Watershed Fund (Don Kent) Comments

Comment 1: *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.*

Response: See response to Peer-Review Panel Comment #1.

Comment 2: *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.*

Response: The authors disagree with the comment. The use of the "reference site approach" is a nationally recommended and accepted method for establishing water quality criterion, especially nutrients that are generally not directly toxic. While the Department's reference site analysis does indicate that flora and fauna are not imbalanced at long-term surface water concentrations less than 10 µg/L, the analysis also indicates significant imbalances have occurred in the structure and function of the natural biological communities at long-term concentrations of 14 µg/L. To assure adequate protection for the natural biological communities, the criterion was derived using the upper 95th confidence interval of the concentrations at the designated reference sites, which is near 10 µg/L depending on the exact method and period of record used in the calculation. Additionally, the proposed 10 µg/L criterion is also supported by the fact that one of the designated reference sites in WCA-2A, Station F5, that has a higher average TP concentration than the other reference sites (i.e., slightly below 10 µg/L), shows definite signs that the more sensitive biological indicators are starting to change and may become imbalanced over time. However, using the "weight of evidence" approach the available information was not sufficient to designate it as imbalanced. Additionally, the 10 µg/L criterion was further supported by the use of the 75th percentile method recommended by the USEPA for derivation of nutrient criterion. Therefore, it is incorrect to suggest that the Departments analyses "tells us only that flora and fauna are not imbalanced at these surface water concentrations".

Comment 3: *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.*

Response: While it may be possible under certain circumstances to obtain accurate samples at water depths less than 10-cm, it is also accepted by most researchers performing sampling in the marsh that the likelihood that the samples will be contaminated by suspended sediment and floc increases dramatically at depths less than 10 cm. Therefore, at the initiation of the P criterion research in the Everglades, the Everglades Technical Advisory Panel recommended that samples not be collected in the marsh at depths less than 10 cm to minimize the risk of obtaining contaminated samples. The District followed this recommendation during their research program. Therefore, data are not generally available for periods when the water depth was below 10 cm. Other than during periods when the water depths were less than 10 cm, the District's monitoring was not restricted based on water depth. Therefore, the percentage of samples collected at various water depths above 10 cm will likely correspond relatively closely to the percentage of the time the marsh water levels were at those levels during the study period. Since the same 10 cm minimum sampling depth used during the development of the criterion is incorporated into the proposed measurement methodology, it is likely that the bias introduced by the use of contaminated samples collected at very shallow water depths far outweighs any bias introduced by using accurate data collected using the recommended sampling protocol for both criterion development and the measurement methodology.

Comment 4: *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.*

Response: The use of the existing phosphorus gradients in WCA-2A and WCA-1 provides the ideal situation to establish the P criterion. The existing P gradients were formed as a result of being dosed with water comparable to post-STA water (especially with regard to speciation of the P present) for approximately 40 years. Not only do the existing gradients represent a full scale (unrestricted) system dosed with post-STA like water, they also allow a greater appreciation of the importance of the long-term chronic effects of P-enrichment (such as sediment enrichment) than does a controlled experiment conducted over a relatively short period. Therefore, the existing P gradients are ideally suited for use in deriving the appropriate P criterion. We also agree with the comment that the design of the experimental dosing studies conducted in the Everglades would be improved by dosing with post-STA water instead of the much more reactive orthophosphate that was used.

Comment 5: *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.*

Response: This comment is inconsistent with the information provided by Smith and McCormick (1999). The paper by Smith and McCormick was evaluating the relationships between P levels in the inflows to WCA-2 and the resulting P concentrations at various points in the marsh. Since the inflows to WCA-2A enter an area already highly impacted by P-enrichment with highly P-enriched sediment, it was concluded from their evaluation that there would be a significant lag time (~10 years) required before the effects from changes in management (i.e., reductions in P levels in the inflows through BMPs etc.) could be reliably detected in the march. Given all of the possible biological and geochemical sorptive processes that can capture the added

P, it is understandable that when P enters WCA-2 at a point it takes some period of time before a significant response at a distance downstream of the inflow. Smith and McCormick further indicate in the case of attempting to observe a decrease in marsh P concentrations at a distance downstream of the WCA-2 inflows in response to a reduction in inflow concentrations, this lag period is further extended due to the process of "internal loading" or efflux from the enriched sediments. Further, it is this internal loading in combination with changes in hydrology and the natural variability of the system that results in the suggestion that a period up to 10 years is required to establish a relationship between changes in inflow P levels and concentrations and in the downstream marsh. Therefore, the conclusions from the Smith and McCormick (1999) paper have little bearing on the natural variability of a site or area within the marsh and the use of these conclusions as justification for the commenter's statement 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" is totally erroneous. Furthermore, to assure the natural variability was taken into account adequately, the Department evaluated the natural variability at selected sites in WCA-2 for periods extending more than 20 years. It should be noted that variability induced by man-made changes in the inflow levels as studied in the Smith and McCormick (1999) paper does not constitute "natural" variability.

Comment 6: *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.*

Response: The Department's recommendation to omit data from years with less than six measurements from the P criterion measurement methodology evaluations was not made without full consideration and justification supported by statistical analyses. As discussed in previous responses, during the development of the measurement methodology, the Department has acknowledged the fact during some years the marsh dries out and it is physically impossible to obtain valid samples during these periods. Additionally, with a monitoring program of this magnitude it is unavoidable that some number of samples will be "lost" due to QA/QC problems or will be deemed invalid for some other reason. Since the P criterion is based on an annual geometric mean, it was necessary to evaluate the minimum number monthly samples necessary to adequately characterize the P regime during any particular year. Based on a statistical evaluation of the historical data, the Department concluded that a minimum of six monthly samples was necessary for this purpose. The purpose of this was to avoid the case in which an annual geometric mean was based on one or two measurements that do not adequately characterize the P regime experienced during that year regardless of the reason.

The commenter has not applied the measurement methodology in a manner consistent with that proposed by the Department. The comment also fails to acknowledge that when the entire measurement methodology is applied, the claimed excursions at the WCA-2A were totally due to the inclusion of the annual geometric mean for 1985, which is based on two individual

measurements following a dry-down of the marsh. The two measurements in 1985 were 35 and 15 µg/L resulting in a geometric mean of 22.9 µg/L. The annual geometric mean based on these two highly variable measurement collected in 1985 is not deemed representative of the conditions during the entire year and its use allows the single high measurement (i.e., 35 µg/L) to control the criterion evaluation for the following five years. Additionally, the claimed excursions in WCA-1 are based on applying only a portion of the measurement methodology. When the full measurement methodology is applied correctly, there are no excursions among any of the reference sites in either WCA-1 or WCA-2A.

The commenter also erroneously indicates that the Department "attributes these excursions to the lack of sampling platforms, differences in sampling methodology, and abnormal events…". Since when the proposed measurement methodology is applied as intended there are no excursions at the reference sites, it is not necessary to attribute them to anything. The chapter does correctly suggest that the slight variation in overall P levels between WCA-2A and WCA-1 reference sites may result from the lack of sampling platforms in WCA-1 and differences in sampling methodology between the two areas.

Comment 7: *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, a 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).*

Response: The commenter's conclusions are unsupported. During a communication with the commenter, it was acknowledged that the above conclusions were based on a very cursory evaluation of the data and probably inaccurate assumptions. In addition, the comment fails to specify what confidence level was utilized in making the above estimates.

The Department has conducted extensive statistical evaluations of the data collected in WCA-2A and WCA-1 during the development of the proposed criterion and measurement methodology to assess the natural spatial and temporal variability at sites minimally impacted by P-enrichment. The results of these evaluations as well as test applications of the proposed criterion and measurement methodology to historical monitoring data available from throughout the EPA indicate that the phosphorus conditions at sites in the marsh are being accurately characterized by the Department's proposed methodology.

Comment 8: *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 to 1999). 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.

Response: The authors agree with the general overview of the current status of the treatment technologies provided in the comment. However, we do not agree with the implication that the P criterion takes into account the current ability to achieve adequate treatment using available technologies. This suggestion is flawed both technically and legally. The Department is legally bound to adopt a P criterion that is protective of the natural flora and fauna. EPA regulations specifically prohibit the consideration of technological feasibility or cost of compliance in setting of water quality criteria. Those factors can, however, be considered by the state in approving a moderating provision or relief mechanism or in establishing the designated use of the water body. In this case, the designated use (Class III waters) has already been established. The Department does agree that technological feasibility and cost should be considered in determining whether a moderating provision is appropriate. Analysis of the scientific information available indicates that long-term TP concentrations above 10 µg/L do not adequately protect the natural flora and fauna regardless of the ability to treat the inflows to 10 µg/L or less. If the criterion were to be established based on the current ability to achieve it, there would be no reason to continue to improve the treatment effectiveness to more protective levels. Additionally, this faulty logic could potentially put the currently unimpacted portions of the marsh at risk of becoming imbalanced.

In summary, when the criterion becomes effective, if the treatment technologies have not advanced sufficiently to achieve the criterion, there are regulatory relief mechanisms available that can and probably will be utilized to allow for this circumstance without resorting to the adoption of a nonprotective criterion that does not satisfy the statutory requirements.

Comment 9: *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.*

Response: The Department has acknowledged that the recovery of the currently impacted areas will occur over an extended period of time. Since the criterion must be applied to the entire EPA, it is also acknowledged that the criterion monitoring sites in the impacted area will appropriately fail the measurement methodology test until recovery has occurred. There are regulatory mechanisms available to acknowledge the fact that the impacted areas will recover over some period without slowing the recovery through the establishment of a higher criterion.

A REPLY BY THE CHAPTER 6 (2003 ECR) AUTHORS TO REVIEW PANEL COMMENTS

November 4, 2002

(Authors responsible for replies are shown **in bold**)

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

BY E. JOE MIDDLEBROOKS

The District personnel are to be commended for collecting and analyzing such an enormous quantity of data collected over such a huge expanse.

1. Most sections of the Chapter do not contain statements as to why the data are being collected. A brief statement at the beginning of the Summary and the Introduction outlining the objectives of the data collection and how the data are to be used would be useful to the reader. As the sections read, one could assume that these are random experiments. An expanded statement similar to the material on pages 6-66 and 6-67 would fill the need.

REPLY **Sklar----**: The following statements have been added to the Summary:

Hydrologic Trends: These data are collected by the Everglades National Park (ENP) and the Environmental Monitoring Division of the South Florida Water Management District (SFWMD). They serve the departments of Regulation, Operations, Planning, Restoration, and Research. Research uses these data as baselines for comparisons across time, habitats, and biogeographic regions. Most importantly, the water depth, rain, and discharge data presented in this chapter of the ECR gives the research scientists the hydrologic variables needed to develop experimental designs and test various stressor-response hypotheses. The data shown here demonstrate the spatial and temporal variance for regions that continue to be fragmented and hydrologically altered.

Ecological Trends: These data were collected to understand how Everglades flora and fauna respond to water resource management. The crayfish study by the Regulation Department evaluates how the extent and intensity of the dry-downs in isolated and short-hydroperiod wetlands can harm this vital prey item. Wading bird data, critical to long-term restoration goals and short-term operations at individual structures, are collected by numerous university and state agencies and compiled by the SFWMD (Gawlik, 2001) to evaluate how regional hydroperiods and depths are effecting distributions, abundance, and nesting success. Tree island and mangrove data are also collected to establish long-term restoration goals and short-term operational guidelines (i.e., Adaptive Protocols). As mandated by the 404 Permit for STA Operations and Discharges, data are presented in this chapter on downstream ecological impacts. Work on ridge and slough habitats, their flooding tolerances, physiological requirements and sediment decomposition, has been added to our research agenda because CERP needs to know how to

prevent further encroachment of sawgrass and cattail ridges into slough habitats, restore historical micotopography, and evaluate the need for canal backfilling.

Remote Sensing and Modeling: Remotely sensed data are used to create vegetation maps, track habitat changes, locate gradients, identify impacts, and establish baselines. These data are used by all synoptic and modeling elements of the SFWMD research program for the testing of hydrologic hypotheses and the development of CERP performance measures. New and updated models are presented in this chapter. These combine the remote sensing data with the ecological trends data and the hydrologic data to estimate total system response (the ELM) or individual species response (the Florida Bay seagrass model and the Habitat Suitability Indices) to planned hydrologic alterations.

2. It is obvious that the natural impacts of the hydrology of the Everglades are pronounced and unpredictable from season to season; however, the study of the hydrology and its impact on the EPA physical structure and wildlife is essential. Has an effort been made to determine the impact of controlling the water depth at more or less a constant as proposed by the Natural Systems Model? With the wide natural fluctuations that have occurred in the past, it would appear that "constant" depth would produce an entirely different ecosystem. It is recognized that modeling of such a variable system is extremely difficult, if not impossible, but if restoration is to be accomplished, a study of this important variable must continue.

REPLY **Sklar**: The NSM does not propose constant water depth. Please tell us where in this chapter you were lead to suggest this and we will correct it. However, as you have noted a large-scale, long-term hydrologic experiment “to control the water depth” is needed. It is called LILA (Loxahatchee Impoundments Landscape Assessment) and it will begin in 2003 and will be described in the 2004 ECR.

3. The study of tree islands is interesting and further illustrates the complexity of such an immense ecosystem. It appears that the tree islands are in a constant state of flux because of storms, ground water, flow patterns and unknown variables, but at the same time further complicates the task of interpreting the influence of hydrology on the EPA. Has thought been given to this constantly changing environment in the modeling processes?

REPLY **Sklar**: The basic design of a tree island model was presented in the 2002 ECR. The details of this model can be found in Wu et al (in press). The model incorporates foods, droughts, fires and hurricanes to predict tree island loss and recovery.

Wu Y., K. Rutchey, W. Guan, L. Vilchek and F.H. Sklar. (in press) Spatial simulations of tree islands for Everglades restoration. In: Sklar and van der Valk. (eds.) Tree Islands of the Everglades. Kluwer Academic. Dordrecht.

4. What impact will the influx of high nutrient concentration groundwater have on the treated surface waters from the STAs or chemical treatment? Will the benefit of the removal of TP to a concentration of 10 micrograms per liter be negated by this influx of nutrients? Are the origins of these nutrients known? What is the magnitude of the groundwater entering the Everglades?

REPLY **Sklar**: It is rare to find groundwater head differentials that produce seepage into the Everglades. In general, the Everglades is the shallow aquifer recharge region for the Lower East Coast and groundwater tends to flow out of the Everglades. There can be localized upwelling regions such as, under tree islands or where peat layers are very thin. It is thought that these are areas where groundwater nutrients may influence local vegetation structure and growth. However, there are no data to support this and there have not been any studies to evaluate STA surface water quality interactions with groundwater quality and movements.

5. Modeling the Everglades and various components has to be one of the most complex activities undertaken in the modeling field. The need for such models cannot be over emphasized when considering the immense financial resources that will be dedicated to the restoration of the Everglades. Errors in judgment will result in huge wastes of financial, scientific and engineering resources.

REPLY **Sklar**: The NSM was used to establish historic water levels. The SFWMM and the ELM (and maybe others) will be used to evaluate plans to create historic water levels. So, here we have hydrologic restoration targets based upon how well models mimic models. We are aware of the seriousness of this problem. That is why our multidisciplinary research projects are so vitally important. They improve the models and provide an understanding of the ecosystem mechanisms that need to be "revived".

Specific Suggestions and Editorial Comments

SUMMARY

6. A general statement of why the various studies are being conducted would be helpful.

Sklar---- See answer to #1

HYDROLOGICAL TRENDS

7. On page 6-2, first paragraph, first line, the sentence beginning with "The worst site." It would be better to select a word other than "worst." For example, "The greatest water level fluctuations occurred in WCA-2A where water levels went down and back up five separate times during the year."

Sklar---- This sentence has been fixed.

ECOLOGICAL TRENDS

8. Why is the number of birds varying so widely?

GAWLIK—Pg 6-2, para. 5 - Added as last sentence "This high inter-annual variability in nest numbers is typical of South Florida wading birds and illustrates the close connection between wading birds and the widely fluctuating hydrologic patterns that characterize the Everglades."

Tree Islands

9. Second sentence, what appendix?

SKLAR--An appendix to Chapter 6 was provided at the web site.

REMOTE SENSING AND MODELING TRENDS

10. Modeling: (2). The use of the word "caused" probably should be "predicted."

SKLAR--Agreed

INTRODUCTION

11. As mentioned above, a statement at the beginning indicating the objectives of the experiments and how the data are to be used would be helpful.

SKLAR--See answer to #1

HYDROLOGICAL TRENDS

12. Table 6-1, 6-2, 6-3 AND 6-4 would be improved by showing the range of weekly values for the long-term and 2002 water-years along with the average values.

SKLAR--Ranges have been included.

13. Does a historical plot of the water levels exist, and if so, are the fluctuations similar or vastly different from modern data?

SKLAR--NSM predicts pre-drainage water level fluctuations and the SFWMM estimates the fluctuations for the last 30 years. Fluctuations are not as extreme in the NSM as they are in the SFWMM. The system appears to be more “buffered” in the pre-drainage system. If time allows, I will incorporate these data into this ECR.

EVERGLADES NATIONAL PARK

14. Either here or preferably earlier in the chapter a brief description of the NSM would be helpful. Not much, just the premise for the model.

SKLAR-- There is a footnote on page 6-5 for the NSM that includes a short description.

Salinity Patterns in Florida Bay

15. It is likely that the R2 for the relationship between Annual Mean Salin.ity and Annual Southeastern Everglades Rainfall would be much smaller if the individual values were regressed. See the example in Table 1.

SKLAR-- We agree that R2 values will decline if individual values are used. However, these data are spatially cumulative and aggregated to remove lag effects and spatial variances that will mask the general relationship between rain and salinity. If the ECR was a scientific publication a more detailed statistical model would be presented. The ECR audience is better served by these simplified relationships.

ECOLOGICAL TRENDS

FAUNA

Wading Birds

16. Was there an increase in the phosphorus and nitrogen concentrations in the water during or after the nesting and hatch? Birds excrete large quantities of nutrients and could have a pronounced effect on the nutrient concentrations in areas where they congregate. Has an effort been made to determine the contribution of N and P to the EPA?

GAWLIK & CROZIER-- Our response follows. No additional text was inserted into the report.

No water quality data associated with bird concentrations are being collected. It is well known that phosphorous and nitrogen concentrations do accumulate in soils and plants at areas where birds congregate (Powell et al. 1991, Oliver and Schoenberg 1989). Bird contributions of N and P to the Everglades have been examined (Frederick and Powell 1994) and were found to be locally important near colony sites. The contribution of P by birds was only a small fraction of the annual deposition of P in the overall Everglades marshes.

17. Why is a shift in the timing of wood stork nests to earlier in the breeding season desirable?

GAWLIK Pg. 6-22, para 2 - Insert before last sentence "The timing of nesting is important because Wood Storks require 90-120 days to complete their nesting cycle. If the young do not leave the nest before the onset of the rainy season, their chances of finding suitable foraging habitat is reduced."

Crayfish

18. Third paragraph. It is doubtful that the high R2s are valid for the data shown in Figure 6-7. If all data points are regressed, the R2s will be much less. See Table 1 where the differences in regressing means and individual data points are illustrated. When means are used in the regression, you will obtain the same equation as that obtained with the individual values, but far different coefficients of determination (R2).

HUFFMAN---You are correct. As a result, we have modified Figure 6-7.

19. Table 6-6. Are there statistical differences between the means? Data appear too erratic to draw conclusions.

HUFFMAN— I did not find statistically significant differences between the mean burrow depths over time. I did find significant differences among burrows created in different substrates. The overall number of measurable burrows created per zone/sampling event was due to chance, the overall depths of these new burrows were consistently deeper over time, just as the groundwater level also was deeper below ground over time.

The reason the data appears erratic is due to: The number listed in each cell under **zones** represent the number of new, active, measurable burrows per sampling event, not burrow depths. Each zone lay along a natural topographic gradient, and had an optimum period for burrow formation-when that particular zone first became dry. After this initial period of intense burrow building, new burrows were usually not created; therefore there were no further new burrow counts/measurements in the future sampling events. This is why "N/A" is listed for some zones in some of the sampling events. Likewise, zones that dried out later in the study had no representation in the earlier sampling events.

Herpetofauna of Tree Islands

Snakes

20. Last paragraph, last sentence, not a complete sentence.

SKLAR-- New Sentence: However, little is known about any feral breeding populations of these exotic snakes.

SOILS AND SEDIMENT

Sedimentation and Erosion Trends

21. Ninth paragraph, first sentence, "occurred" should read "occurring."

SKLAR-- DONE

Peat Microtopography and Spatial Pattern in the Ridge and Slough Landscape

22. Third paragraph, last sentence, "Figs. 1 and 2", probably should read Figures 6-10 and 6-11.

DONE

23. What difference does it make to the EPA if ridges and sloughs are formed and along comes a hurricane or storm and everything is disrupted?

MCVOY-- To date, no evidence has ever been found of storm-induced destruction of the ridge and slough pattern. Remote sensing and soil core evidence suggests that--under natural conditions--the pattern remains constant over centuries. A more likely hurricane scenario is that rapidly raised water levels cause exceptional flows, but in the normal flow direction, scouring sloughs and thus reinforcing, rather than diminishing the original pattern

Decomposition

24. Most reaction rates are influenced by increases in concentration.

PENTON --While in the strict sense of enzyme kinetics relating to substrate concentration, Michaelis-Menton dynamics certainly apply. However, in this study and the general studies of enzymatic decomposition processes, the quantity of enzymes produced by the microbial community as a result of substrate induction is the source of investigation. Some examples of environmental affects on enzyme reaction rates and enzyme production in the environment are as follows:

Decomposition of complex plant matter results in the production of aromatic hydrocarbons known as polyphenols. These compounds have been shown to inhibit enzyme activity through non-competitive mechanisms (Freeman et al., 1995).

The presence of divalent cations have been shown to relieve inhibition caused by polyphenols. Among the most important are Ca and Mg (Wetzel, 1991) Our recent studies show that this appears to occur in the Everglades.

The quality of the substrate also affects the decomposition dynamics of organic matter. The presence of high concentrations of lignin has been shown to retard decomposition processes. This is evident by the recalcitrant nature of lignin and the increase in lignin concentration with soil depth as compared to cellulose and other polysaccharides (Melillo et al., 1989)

The availability of N and P also affects enzyme production. In cases where P and N are available, less energy is directed by the microbes to N and P acquisition through the production of extracellular enzymes. Therefore, more energy can be used for the production of enzymes required for C acquisition (Sinsabaugh et al., 1993)

Therefore, the statement of most reactions rates are influenced by increases in concentration is correct. Reaction rates can be influenced by substrate concentration in the laboratory. We have investigated this prior to experimentation by altering fluorogenic substrate concentrations. In this case, the alteration of substrate concentrations led to differing reaction rates (Penton and Newman, in prep) All our enzyme assays are carried out at saturating conditions.

Freeman, C., Liska, G., Ostle, N.J., Jones, S.E., and M.A. Lock, The Use of Fluorogenic Substrates for Measuring Enzyme Activity in Peatlands. Plant and Soil, 1995(175): p. 147-152.

Melillo, Jerry M., Aber, J.D., Linkins, A.E., Ricca, A., Fry, B.,Nadelhoffer, K.J. 1989. Carbon and nitrogen dynamics along the decay continuum: Plant litter to soil organic matter. Plant and soil, 115:189-198.

Sinsabaugh, R.L., Antibus, R.K., and A.E. Linkins, McClaugherty, C.A., Rayburn, L., Repert, D., and T. Weiland, Wood Decomposition: Nitrogen and Phosphorus Dynamics in Relation To Extracellular Enyzme Activity. Ecology, 1993. 74(5): p. 1586-1593.

Penton, C.R., and S. Newman. Determining Optimum Substrate Concentrations and Incubation Times for Fluorogenic Assays. In prep.

VEGETATION

Ridge and Slough

25. Common names and photographs of the plants would be helpful to the non-scientific reader.

MIAO --- Common name for Elecoachris cellulosa and Rhyncospora are spikerush and beakush, respectively.

26. Figure 6-17, a better description of what constitutes a subfigure would be helpful.

MIAO-- Caption has been modified.

Vegetation community changes in Rotenberger

27. How do you know that a 31-year average stage will lead to a "natural" or "original" condition? What evidence indicates that the NSM truly represents historical conditions?

Sklar—The NSM can never be calibrated or validated. Therefore it is impossible to say that NSM is the “true” representation of the historical Everglades. See pages 2-45 to 2-49 of the 1999 Everglades Interim Report for a description of NSM and a partial answer to this question.

28. Second paragraph, fifth line from top of page 6-45, correct punctuation is "wetland species; however, in the past …..".

Sklar--Done

29. Second paragraph, adding a few words such as "indicating a return to the original state." at the end of the last sentence, if appropriate, would be helpful.

Sklar--Done

30. Table 6-7. What are the values in the parentheses? Are the (No. of leaves) measured over some measured area or what?

LEEDS & CORONADO – It is No. of leaves per square meter.

Tree Island Ecology

Tree Growth

31. Figure 6-20. A definition of "Leaves," "Reproductive," "Woody," and "Miscellaneous" would be helpful. Probably know what leaves are, but others need description.

SKLAR--DONE

32. Table 6-8 is missing.

CORONADO--Table has been inserted

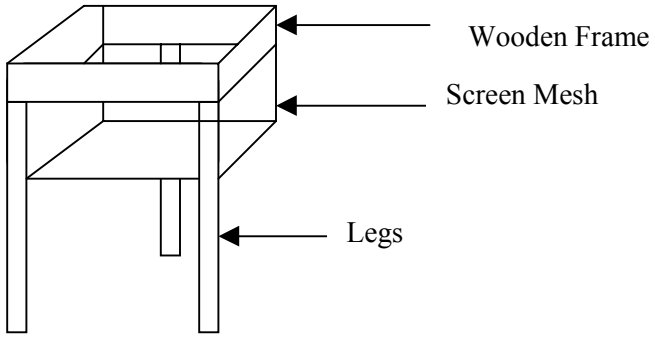
Tree Island Litterfall

33. How do you compensate for transport of leaves, etc. caused by water movement and rainfall? In other words, what types of controls are available to ensure that effects of wind, water transport, etc. are considered when collecting litterfall?

KORVELA&CORONADO-- Litterfall is collected in a .25 m^2 trap standing at approximately 1.25 meters above ground surface. Litter traps are 50-cm X 50cm wood frames with screen mesh affixed to the underside. The screen mesh has a drupe (wall) of 20 - 25 cm from the top of the trap. Three wooden legs support the trap at a height of 1.25 cm.

Effects from water movement, rainfall and wind are minimized due to the construction design of the trap. Water movement and rainfall will transport litterfall out of the traps only under extreme high water events that result in water levels going above the top of the litter traps. The effects from wind are also minimized due to the 20-cm drupe in the screen mesh, except under Hurricane force winds. This drupe prevents litterfall from being blown out of the traps under normal weather conditions.

Sample periods having either high water or high wind event, such as a hurricane, are eliminated from the data set. . In the case of hurricane Irene the top of the litter traps went under water, resulting in the loss of litterfall. Additionally, high wind and downed trees resulted in the toppling of many litter traps. The data collected during this timeframe was eliminated from the database.



MANGROVES

34. Is there any indication of the importance of microtopography relative to the other factors? How is oxygen taken up by the plants, oxygen to soils and then to usage by plants?

CORONADO-- Landward zonation of mangrove species has been related to the microtopographic characteristics of the coastal zone where mangrove forests dominate the landscape. Microtopography also affects the magnitude of the influence of tides on soil environmental parameters such as, soil salinity, redox potential (Eh), sulfide and nutrients. All Mangrove species have physical adaptations to cope with anoxic soils that characterize most mangrove ecosystems. For instance, R. mangle has prop roots (aerial roots) that in turn have small pores (lenticels) whose main function is to exchange oxygen between the atmosphere and the mangrove root system. Other mangrove species, A. germinans and L. racemosa, have developed another structures (pneumatophores) that contain also lenticels whose function is similar to that of the aerial roots. In short, oxygen and CO2 are exchanged through these structures.

35. Fourth paragraph, last sentence. Sentence should be specific about which of the sites differed.

CORONADO-- This spatial variability in litterfall production among sites was significantly lower at the dwarf site ($p<0.001$) relative to the other three sites whose litterfall production was statistically similar ($p>0.05$).

REMOTE SENSING AND MODELING TRENDS

REMOTE SENSING

Everglades Tree Island Canopy Measurements Using Lidar and

Hyperspectral Data

36. More discussion of the preliminary results would be interesting assuming that more information is available.

RUTCHEY--There really isn't any more to report as we are in the very early stages of Phase 1 of this study and basically have only collected the raw data.

IKONOS Satellite Vegetation Mapping Evaluation

37. Although the results are preliminary, a discussion of what the various colors and configurations represent would be useful.

RUTCHEY-- Two sentences were added to describe the colors in the image. The paragraph is now:

Figure 6-25 represents one of the first IKONOS satellite data sets that the South Florida Water Management has obtained. The image is a false color-infrared composite of WCA-1 and has four bands of information with a spectral resolution of four meters. Redder areas represent regions where there is more dense vegetation. Large black areas on the western side of the image are fire scares. Five separate images make up this composite with four collected on 03/21/02 and one on 03/10/02. Spectral characteristics were matched between the images using band ratio techniques to produce this relatively seamless scene. Cloud cover and shadow effects account for approximately 8.5 percent of the area coverage. Work is currently being conducted to see if this data will be able to be used for classifying the exotic Lygodium sp. and also cattail coverage.

FROM: JOANNA BURGER JEITNER@BIOLOGY.RUTGERS.EDU

Date: Tuesday, September 17, 2002 10:54 AM

COMMENTS ON CHAPTER 6

The shift in emphasis of the SFWMD from phosphorus cycling and a phosphorus threshold is timely and scientifically justified in light of the importance of understanding fully the hydrological cycle and needs of the Everglades. This chapter fairly examines the state of these issues and future needs.

Questions and comments follow:

38. Page 6-1. Was there a lag time in any aspect of the drought? And have there been observable lag times in the past that makes this event different?

SKLAR Reply: Due to the fragmentation of the Everglades and the often spotty nature of the wet season rainfall, there can be lag effects within and between regions. However, if rainfall is relatively homogeneous and structures remain open, the Everglades will very quickly return to "normal" hydropatterns.

39. Page 6-2. It might be useful to state upfront why increased flow to Florida Bay may be negative, and to what components (vegetation, fish)?

RUDNICK Reply. The paragraph on page 6-2 is a summary paragraph and already states that the concern is increased N loading and algal blooms. Expanding this to describe the consequences of blooms to ecosystem structure and function seems beyond the summary. I recommend leaving this paragraph about as is (perhaps only with minor edits if it improves clarity).

40. Page 6-2. On what basis were the species selected for examination?

SKLAR—All species were selected because previous studies have suggested that they are key indicators of ecosystem food web structure or they are important elements of ecosystem function. However, the wording of paragraph 2 should be changed to:

This year, we briefly report on the annual wading bird surveys and on those studies that have significantly added to our understanding of Everglades structure and function. These include herpetofauna descriptions on tree islands, tree growth rates on tree islands, crayfish relationships with groundwater, and ridge and slough evaluations of microtopography, decomposition and vegetation.

41. Page 6-2. Can the wading bird numbers be put in context with those of the rest of Florida or the southeast. In other words, were there more nesting birds in the total region, or just a shifting into the Everglades? In some years birds may not breed, so there could be an increase in the percent of birds nesting without an increase in total populations.

GAWLIK Reply: pg. 6-21, para 3 - Delete the first sentence. Start a new paragraph at the beginning of the second sentence. Insert the text below as the new first paragraph for the Wading Bird section.

"Wading birds play a prominent role in CERP, and their conservation is of special interest to the public. In the Everglades wading birds are used as indicators of wetland ecosystem conditions. The location of nests, timing of nesting, and total numbers of nests are indices of local ecosystem conditions and do not reflect regional population trends. There is increasing evidence from genetics studies that the geographic population boundary for species of wading birds that occur in the Everglades is the entire southeastern United States. Thus, changes in the wading bird indices for the Everglades do not translate directly to changes in overall population trends and vice versa."

42. Page 6-17. Are there historical data to indicate what salinity was in Florida Bay before this total disruptions to the Everglades? A decade may not be long enough.

RUDNICK Reply. A single decade is not sufficient to describe a baseline of salinity characteristics (spatial and temporal variations, central tendencies, etc). However, the vast majority of water quality and ecological information on Florida Bay has been collected during the past decade and thus the relationship between freshwater flow, salinity, and ecological status must largely be inferred from changes during this time.

For the purpose of defining restoration targets (especially as part of CERP's Florida Bay and Florida Keys Feasibility Study) and understanding long-term ecological change in Florida Bay, historic salinity information is (ideally including pre-drainage information) needed. Some historic data and estimates from salinity proxies do exist. Salinity measurements made prior to the major ecological changes in the bay, which were apparent by 1987, were quite patchy in time and space. Mike Robblee, of USGS-BRD, has compiled salinity data from the literature (including gray literature) and these data are available. However, much variance in the data exists because they were not collected uniformly as part of a monitoring program, but rather in an inconsistent manner (spatially, temporally, methodologically). The Robblee data set does not include any information collected prior Everglades canal construction.

Pre-drainage salinity information, however, has been provided by palaeoecological studies of Florida Bay (funded by ÜSGS, SFWMD, and NOAA). These studies have included analyses of sediment cores and growth rings of coral skeleton material. Historical salinity has been estimated by both biological (species composition of molluscs, foraminifera, ostracods) and chemical (del 18-O, Mg/Ca, DOM fluorescence) proxies. Much of this work had been published (see Bull. Amer. Paleontology Number 361, Nov. 2001 and also USGS website: http://sflwww.er.usgs.gov/projects/eh_fbswc/).

It should be noted that this historic salinity information, and an assessment of the relationship of salinity and ecological change, will be part of the Florida Bay and Florida Keys Feasibility Study. Results from this Study could be presented in future Consolidated Reports.

43. Page 6 -20. The section on Florida Bay and the CROGEE is particularly clear and insightful (as well as documented). Are there published papers on the secondary effects on coral reefs - might be useful to add.

RUDNICK Reply: There is a great deal of uncertainty regarding the impacts of changing Everglades hydrologic conditions and nutrient loading on the Florida Keys' coral reefs. These reefs, like coral reefs worldwide, have been in a state of rapid decline in recent decades. Cause and effect relationships are not well understood, but hypotheses and some inferences (albeit weak) regarding potential secondary effects of Everglades restoration have been presented. Two references for these hypotheses are in the Porter and Porter volume. One, by Larry Brand, is already cited in the Report. The other is: Lapointe, B.E., W.R. Matzie, and P.J. Barile. 2002. Biotic phase-shifts in Florida Bay and fore reef communities and the Florida Keys: linkages with historical freshwater flows and nitrogen loading from Everglades runoff. P. 629-648 in: The Everglades, Florida Bay, and Coral Reefs of the Florida Keys; J.W. Porter and K.G. Porter, eds. CRC Press, Boca Raton, FL.

44. Page 6 -21-22. The shift in nesting locations of the wading birds in the Everglades is not negative; it may well reflect the more traditional pattern. Colonial species switch locations as conditions dictate. What is essential is that there be a range of conditions so that all species find appropriate nesting sites each year. Only if there were a dramatic shift in reproductive success associated with the shift itself is there cause for alarm. Where did the base low/high come from, and how was it determined?

GAWLIK Reply: We agree that from the bird's perspective, nesting location changes may not be negative unless it impacts reproductive output. However, our perspective is from that of the ecosystem. The presence or absence of birds in a location tells us something about that area.

We recommend adding the text below to the end of the last paragraph on page 6-22.

"The movement of birds away from the coastal Everglades indicates that conditions there are not suitable and the ecosystem is not functioning in the way in which it did historically (Ogden 1994)."

Ogden citation to add to Lit Cited:

Ogden, J. C. 1994. A comparison of wading bird nesting colony dynamics (1931-1946 and 1974-1989) as an indication of ecosystem conditions in the southern Everglades. Pp. 533-570 in S. Davis and J. C. Ogden eds, Everglades: the ecosystem and its restoration. St. Lucie Press, Delray Beach, Florida.

Add the text below to the end of the title for Table 6-5.

"Base low/high represents the lowest and highest three-year running averages for the base years 1986-1995. This period represents the first set of consecutive years when wading bird nesting surveys were conducted systematically for all of the Water Conservation Areas and Everglades National Park."

45. Page 6-23. Is there a relationship between burrow depth for crayfish and wading bird foraging or success?

GAWLIK AND HUFFMAN Reply: The crayfish studied by Huffman dominate the short-hydroperiod wetlands. The dominant Everglades crayfish in long hydroperiod marshes is *Procambarus fallax.* This species rarely if ever burrows. It simply hides in the vegetation and under periphyton. As far as the general relationship between crayfish burrows and wading birds, the closest thing that we am aware of is the information Keith Bildstein published on fiddler crab burrow depth and white ibis foraging in salt marshes. To the best of our knowledge, there is no comparable information on the depth of crayfish burrows and wading bird foraging.

46. Page 6-26-28. Very useful information. However, it is not clear from the descriptions what the key questions are that should be examined in the future. Perhaps three or four key indicator species should be selected for in-depth study to determine the effect of changes in tree islands or hydrological regime would have on them (and by extension, other similar species).

SKLAR & GARRET—The purpose of this section was to archive the biodiversity of herpetofauna. You are right -- the best way to take this to the next level of understanding is to select just a few species to study. However, data is scarce and we are still in the process of deciding how to select and measure a few key species.

Make last sentence to Para. 1, pg 6-26: This review highlights potential dependencies of the fauna on tree islands. Future studies will need to document how spatial distributions and abundance of key indicator species are impacted by hydroperiods and water depths.

47. Page 6-46. Is decomposition being examined as well?

CORONADO-- So far, decomposition is not being examined but we are planning to initiate some decomposition experiments to have a better understanding of the litterfall dynamics on tree islands.

FROM: SHARITZ@SREL.EDU

Date: Wednesday, September 18, 2002 03:22 AM

Chapter 6 Review - Hydrologic Needs - Effects of Hydrology on the EPA

Rebecca Sharitz

48. This chapter presents a large amount of information, organized into categories of hydrologic trends, ecological trends, and remote sensing and modeling trends. The first two sections contain results of different monitoring/sampling or experimental studies, but these are not very well integrated. Often they appear to be a collection of somewhat unrelated studies. It would be helpful to provide some overview or rationale that connects and justifies these efforts.

Sklar--All the research is integrated by one common theme: Optimization of hydrologic regimes to 1) prevent further degradation of the Everglades and 2) create a hydrologic environment for long-term sustainability of ecosystem function and structure. It can appear not to be integrated due to various states of completion, differences in geography, and bureaucratic origins (i.e., Governing Board, operations, regulation, or legal requests for information). However, ever effort will be made to highlight related studies where appropriate in the Summary and Introduction for Chapter 6.

49. Similarly, some of the information that is given is much more developed and analyzed; some is rather preliminary. Also, there is quite a lot of difference among various subsections in how clearly they are written and how free they are of editorial errors.

Sklar--This chapter has 20 authors. Every effort is being made to smooth out the different writing styles.

50. On page 6-2, in the second paragraph, the discussion of CROGEE's review somewhat misrepresents the intent of CROGEE (by using the phrase "harmful to Florida Bay" without further explanation) in giving caution that increased freshwater flows to Florida Bay may increase nutrient loading (especially N) and stimulate algal blooms. See page 6-20 for a better description of the cautions expressed by CROGEE.

Sklar & Rudnick--New sentence: .. CROGEE cautioned that historical restoration of Everglades and increased freshwater flow to Florida Bay, as currently planned by CERP, may not be beneficial to Florida Bay.

51. In the section of herpetofauna of tree islands on lizards, is any effort being made to track the green iguana? Does it seem likely that it might become a problem in the future?

Garrett-- To my knowledge there is no great concern of BREEDING populations of green iguana within the interior of the Everglades. While many escaped/released iguanas have been

spotted within the Everglades (specifically in ENP), they have not established populations yet. The reason may be that juvenile green iguanas are not be able to successfully survive the cooler (temperatures below 60 degrees) winters of the subtropics.

52. In the discussion of frogs and toads, how are alligator holes an important environment for anurans, especially if they typically support fish that are larval anuran predators?

Garrett-- Alligators holes are important refugia for both tadpoles and adult anurans during the seasonal drydown in the Everglades. They are still preyed upon during this time. To make sure that this is understood, sentence should read (and would be better placed as the last line in the first paragraph under the section Frogs and Toads): Alligator holes, associated with tree islands and solution holes, can be important environments for many anurans as refugia during the dry season in the Everglades.

53. Figure 6-9 is unclear. What does the shaded area in the lower part indicate, and what is the line right above the shaded area?

Sklar--Added sentence to figure caption: The depth of the storm surge during the hurricane was estimated from marl deposits on tree trunks along a transect. If one assumes that these high water marks were relatively flat (the zero depth line), then one can estimate the relative ridge topography (the shaded area).

54. In the section on sedimentation, page 6-32, paragraph 2, how was the depth of the storm serge measured along the transect?

Sklar--See #53 above

55. -Figure 6-10 and 6-11 legends, n=3: Does this mean that 3 measures of peat surface elevation and bedrock elevation were made at each 50m point along the transects? If so, how far apart were these measurements taken?

McVoy-Yes, each station was measured in triplicate with 1-2.0 m spacing. Text has been modified.

56. In the section on decomposition, page 6-37, paragraph 2, the results reported in thelast 3 sentences appear to be the reverse of those shown in Figures 6-12 and 6-13. Which is correct?

Penton--Due to a typo, the meaning of the En index became reversed. Corrected text: When En ratios are greater than 1, nitrogen mineralization is occurring. When the ratios are less than 1, nitrogen immobilization is occurring.

57. On page 6-38, a stem elongation response to flooding has been shown in otheremergent wetland species, such as Panicum hemitomum and Leersia hexandra, and so is not uncommon.

On page 6-41 in paragraph 1, insert R. tracyi in line 4 after biomass, for clarity. Was there no P effect on E. cellusloa biomass?

Miao-- On page 6-41 in paragraph 1, should insert "of both species" after biomass, and then add "when the water treatments were pooled".

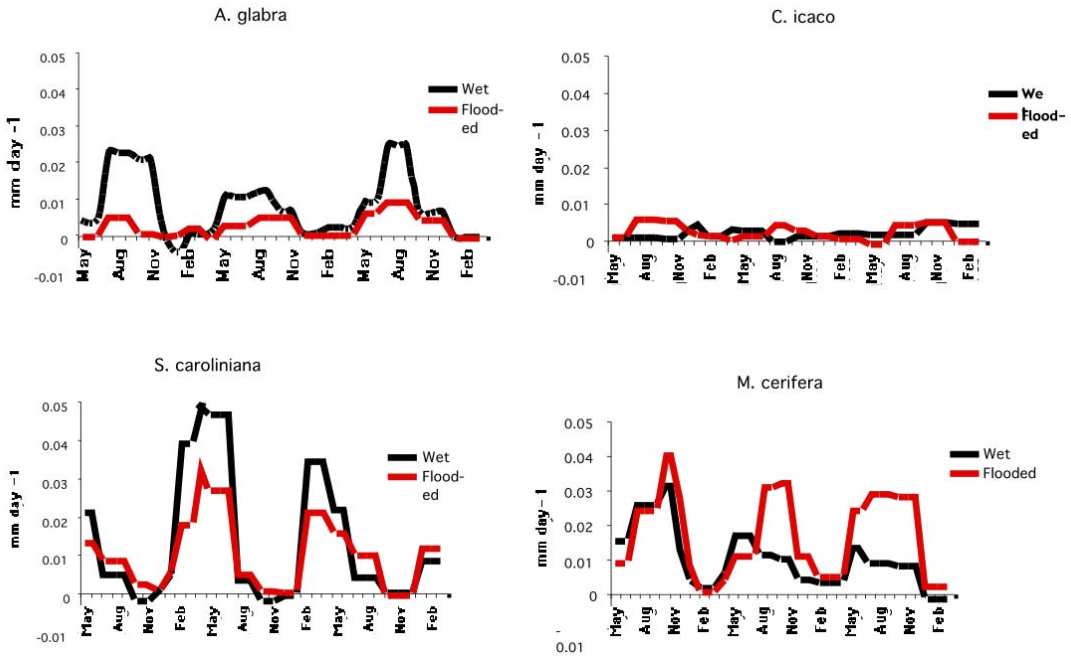
There was P effect on E. cellulosa biomass, but none on R. tracyi.

58. In Figure 6-17, what are the units on the Y-axis?

Miao--Answer: % (Figure has been fixed)

59. In the section on tree island ecology on page 6-46, Figure 6-16 does not show tree growth of the four species mentioned in paragraph 3. On the next page, Table 6.8, which is cited for growth data also appears to be missing, and such data do not appear to be given anywhere. Similarly, on page 6-48, Figure 6-22 is incorrectly cited.

Sklar--Something happened with the formatting of this chapter. We will make the corrections. The text should be referring to Figure 6-19 not Figure 6-16. The missing figure and table are shown below:



Species	Water Depth	Hydro Period
Annona glabra	*-0.12*	*-0.04*
Chrysoblan usicaco	*0.33 **	*0.28 **
Myrica cerifera	*0.41 **	*0.40 **
Salix caroliniana	*-0.57 * **	*-0.53 * **

Table 6-8 - Tree growth rate (mm/day) co relation with Water Depth (cm) and Hydroperiod (number of days a tree islands has been flooded)

* Significant at alpha = 0.05
** Significant at alpha = 0.001

60. In Figure 6-22, what are the units on the Y-axis?

Sklar--Reply: The caption has been modified: g m^{-2} yr^{-1}

61. What is the image on page 6-54? If this is included, it needs to have a legend.

Sklar--Reply: A formatting problem occurred. This image belongs with Figure 6-23

Editorial (the chapter needs to be examined carefully to correct numerous minor errors. These are only a few.)

6-2, pa 2, l 8 - write out CROGEE

6-3, pa 2 - write out genus names

6-4, pa 6 - give genus of T.

6-14,15 - what is supposed to be on these pages?

6-26, pa 5, l 5 - insert high after remain

6-28, pa 6, l 3-6 - something is missing here

6-29-30, Figure 6-8 and 6-9 - should be moved to later position in text closer to where they are cited.

6-30, pa 2 and 4 - Cahoon refs don't appear in the Lit Cit.

6-33, pa 3, final sentence - the figure numbers need to be corrected, or omit the sentence

6-41, pa 2, lines 1 and 6 - reverse Figure 6-1 top to bottom; likewise in 6-43, Figure legend, change top and bottom in line 1

6-44, pa 2 - clarify that the past five years was prior to July 2001, if indeed that was the case

6-45 - Table 6-7: Technically, isn't Plant density" really "Shoot density" since these are rhizomatous plants? **YES**

6-46, pa 2 - Is the Messina and Conner book the best reference here? Perhaps better to cite a specific chapter in the book that deals with flooding?

6-47, pa 3 - both Figures 6-19 and 6-20 show these data.

Review of Chapter 6: Hydrologic needs-effects of hydrology on the Everglades Protection Area (Ping Hsieh, Florida A&M University)

62. Research on the hydrology of Everglades must be encouraged because many biological and biogeochemical processes are hydrologically driven or mediated. The task, as demonstrated by Chapter 6, is extremely complicated that requires great deals of skills and experience to do the job. I appreciate the effort of the workers, who put together this complicated subject matter, even though "this report does not, as this time, quantify the hydrologic needs of the Everglades". It will be beneficial to readers, if specific objectives of each experiment or investigation could be stated clearly in the report. A brief review of hydrological inter-connection among elements of wetland ecology such as: vegetation, biogeochemistry, erosion, sedimentation, salinity, microbiology and nutrient cycling would be very helpful to readers as well. A consistency in unit (metric or English) throughout the chapter is more desirable. Followings are my specific commons and questions:

Reply (SKLAR)— Clarification of the objectives and the relevance to restoration of each project has been added to the Chapter 6 Summary and the Chapter 6 text.

63. Pg 6-1 Are inflows controlled or spontaneous? What is the relationship between rainfall and inflow rate in each area?

Reply Sklar— All structure inflows are controlled. Inflows into the WCA's are designed to facilitate flood control. Inflows into the Park are estimated from predicted rainfall combined with actual watershed runoff rates. This rainfall-driven algorithm was developed by the Hydrologic Systems Modeling Division at the SFWMD.

64. Pg 6-2 Why increase N loading stimulates algal bloom? Does it mean that P is not limiting?

Reply Rudnick— Nitrogen is not the sole concern regarding Florida Bay water quality and ecological restoration. The primary concern regarding the ecological state of Florida Bay has generally been on salinity. Concerns regarding water quality, however, are connected to potential hydrologic modifications that are intended to lead to restoration. Some investigators have hypothesized that many of the ecological changes observed in Florida Bay since the 1980s have been caused by nutrient enrichment and not changing salinity. These investigators follow that increasing freshwater flow will increase nutrient inputs and exacerbate existing problems. Nitrogen has become the focus of more concern than phosphorus in recent years, based on two major findings. First, measurements of nutrient loading to the Bay from the Everglades (since 1996) have demonstrated that P loading is extremely low, while N contributions appear to be a significant portion of the Bay's nutrient budget. Second, bioassays have demonstrated that while phytoplankton production in eastern Florida Bay is phosphorus limited, phytoplankton in central and western Florida Bay are more typically N limited. Given that algal blooms in the bay, which began in the early 1990s, occur only in the central and western Bay, the possibility of increased N loading with restoration has become a major concern.

Pg 6-2 Line 25, It is "methods for decomposition measurements" not "decomposition techniques".

HYDROLOGICAL TRENDS

65. Why water retention (or residence) time is not included in the discussion of this chapter? It is a very important hydrological consideration of any wetland system.

Reply Sklar— This is an excellent suggestion. An analysis of residence times can be an important performance measure for CERP and may play a critical role in ridge and slough development. Due to time constraints, it can not be included in this year's report.

66. Is the water depth of NSM served as a control in the experiments? Why?

Reply Sklar—It is not clear what experiments are being cited in this question. In general, NSM supplies an estimated of the hydrologic extremes for different areas of the Everglades. These extremes may then be used to constrain hydrologic experiments. The concept of a control in an experiment that evaluates a gradient of "dosages" my not be appropriate.

SALINITY PATTERNS IN FLORIDA BAY

67. According to the top portion of Fig. 6-6, it is hard to believe that freshwater flow of Taylor Slough and C-111 wetlands had no relationship to the salinity of Eastern Florida Bay. There may be a little phase shift (delay), otherwise freshwater flows sync well with the salinity.

Reply Rudnick-- Figure 6-6 shows the approximate synchrony of seasonal increases in freshwater flow and decreases in salinity in eastern and central Florida Bay. However, simple linear regression analysis of salinity and flow data show that this relationship is weak. Analysis of phase shifts was done, using a monthly time step (which was the sampling interval). When this shift was not taken into account, no significant relationship between flow and salinity was found (eastern and central bay $R2 \leq 0.01$). A significant, but weak relationship was observed when time lags were incorporated. As stated in the report text, the best fit was observed with a three month lag, but R2 values were still only 0.25 for the eastern bay and 0.20 for the central bay. This weak relationship is likely a consequence of the complex circulation patterns of Florida Bay and the relatively low rates of freshwater discharge (i.e. that salinity is more dependent on local rainfall, evaporation, and circulation than freshwater flow). It also reflects the weakness of such a simple statistical approach. Recognizing this weakness, CERP's Florida Bay and Florida Bay Feasibility Study plans to develop both mass balance and hydrodynamic models for Florida Bay. Coupled with upland hydrologic models, this is expected to greatly improve our understanding of the effects of changing freshwater stages and flows of Florida Bay salinity

ECOLOGICAL TRENDS

68. Why No. of birds increased from 1994 to 2002? Is it hydrology or else?

Reply Gawlik-- "This high inter-annual variability in nest numbers is typical of South Florida wading birds and illustrates the close connection between wading birds and the widely fluctuating hydrologic patterns that characterize the Everglades."

CRAYFISH

69. I do not understand Fig. 6-7. What does a sample event represent?

Reply Huffman— Table 6-6 list the sample events. However, Figure 6-7 has been changed. The groundwater levels are now shown on the x-axis.

SOILS AND SEDIMENTS

Fig. 6-8 and Fig. 6-9 should be under the sub-heading of **Soils and Sediments** not under **Herpetofanna of Tree Islands**.

Reply: An attempt will be made to correct this formatting error.

70. What does the lower part of Fig. 6-9 represent?

Reply Sklar— Added sentence to figure caption: The depth of the storm surge during the hurricane was estimated from marl deposits on tree trunks along a transect. If one assumes that these high water marks were relatively flat (the zero depth line), then one can estimate the relative ridge topography (the shaded area).

71. 6-30, Soil organic matter formation is from both aboveground and belowground productions not just belowground production.

Reply Sklar— New sentence: "As a result, belowground production of mangrove peat is considered the primary soil building mechanism."

72. 6-31, Paragraph 2, "After burning, samples were analyzed for ash and organic matter content." What do you mean by that?

Reply Coronado-- Samples were burned to obtain ash free organic matter content.

73. 6-31, Para 5, I could not see from Fig. 6-8 that negative elevation change had occurred. It appeared that elevation might have been increased at dry environment and constant at marsh and Flooded environments. Also, the statement of the last sentence is contradictory to the results of statistical analysis.

Reply Coronado-- Text has been modified to describe what figure 6-8 indicates. However, even though elevation change has been constant at marsh and flooded environments, shallow subsidence, which is highest at the flooded environment, is the factor that would make these environments compromise their persistence in the event of an accelerated increase in sea level rise. The last statement has been modified to match what the statistical results are indicating us.

74. 6-32, The results suggest that the dry, marsh and flooded environments are generally keeping pace with the relative sea-level rise by sedimentation and OM building (within the margin of errors). I do not see how hurricanes can help to explain the accretion of the wetland. Hurricanes may deposit and washout sediments. Did you see the white marl layers in soil columns all over the place as the feldspar marker layer did in the experiments?

Reply Sklar—You may be right that hurricanes can be erosional. However, studies in Louisiana indicate limited places of erosion and extensive regions of deposition. Our only interaction with this type of high energy event has been with Hurricane Irene and we found no evidence of erosion and only very limited deposition across the Buttonwood Ridge (see Figure 6-9). Most of the coastal wetland appeared unimpacted.

Peat Microtopography….

75. It seems to me that hydrodynamics (water flow) and vegetation may interact to create the pattern (parallel to the stream) of Fig. 6-10. Did you measure flow rates of the experimental areas? The patternlessness of Fig. 11 might be caused by the slower flow rate.

Reply Sklar-- We have only recently documented that there is a difference in the microtopography of ridges and sloughs but it is not at all clear how they are developed and sustained.

DECOMPOSITION

76. Decomposition : (a) What is a En ratio? What is EICQ? (b) How do En ratio and EICO relate to net decomposition? (c) The title of Fig. 6-13 may be in error. Those are not the P enriched sites. The interpretation of this section is quite confusion or not clear.

Reply Penton—Text describes these two indices. Due to a typo, the meaning of the En index became reversed. Corrected text: “When En ratios are greater than 1, nitrogen mineralization is occurring. When the ratios are less than 1, nitrogen immobilization is occurring”.

VEGETATION

77. The statement of the last sentence in 6-39 is contradictory to what the data said (P has effect on at least one species).

Reply Miao --- Eleocharis cellulosa (spikerush) and Rhynchospora tracyi (beakrush) are two dominant wet prairie species in the Everglades, and thus their responses to hydrology and nutrient availability have important implications to Everglades vegetation restoration, specifically Decompartmentalization, seepage management, and operations of water preserve and conservation areas. Changes in root and shoot growth, indicative of health, of the two species in response to water levels and phosphorus availability were examined in growth chambers designed specifically for root growth analysis. The two species responded to P and flooded conditions differently in terms of their above- and below-ground morphology, growth, and biomass. The results indicate a better adaptation of spikerush to higher water levels than beakrush. As a consequence of the different responses to water level and P availability, the relative success of spikerush and beakrush in the field may be altered with changes in hydrology. However, for a more holistic, ecosystem evaluation, information concerning the competitiveness of these two species in relation to other vegetation is needed.

78. 6-45, Is there any seed bank problem found in the changeover of obligated and facultative species?

Reply Coronado--- No, there is not a seed bank problem associated to the fluctuation of marsh plants species observed at Rotenberger. In fact, due to changes in hydrology the data suggest that some obligate marsh species (i.e Utricularia) have been successful in recolonizing a marsh environment that has been subjected to drought and fire events that occurred previously to start discharging water from STA-5 (Stormwater Treatment Area). However, more data are needed to better evaluate the effect water discharge into Rotenberger.

79. Tree Island Ecology: (a) 6-46, The figure legend of Fig. 6-19 is not right. There is no figure for tree growth data. (b) It seems to me that litterfall is a function of seasonal temperature. Is it true? (c) Can tree species indicate water regime of a wetland?

Reply Sklar— (a) There was a figure misplaced in the draft. It has been corrected. (b) Tropical tree species produce litter year-round. However, most trees will produce signficant litterfall right after fruiting and/or during late summer. (c) Yes, we know for example that willow is more tolerant of flooding than most other species and is usually found on tree islands that have been inundated from many years. However, we do not know how long a willowhead can survive continuous inundation or what the seedlings require for successful recruitment and thus, long-term sustainability.

80. Remote sensing and Modeling Trends: (a) Can aerial photographs and satellite images differentiate tree species in Fig. 6-21? (b) What are the resolutions of X, Y and Z of the LIDAR images? (c) If the resolution of Z is not good enough, how do you overcome the shortcoming?

Reply Rutchey— This is possible and it depends on the scale of the photography or resolution of the satellite imagery being used. Part of this research is to evaluate whether the hyperspectral imagery is able to differentiate between different tree species. However, one of the reasons we obtained both LIDAR and hyperspectral data is because of the potential synergistic effect of having both of these data sets utilized for solving species identification.

Final Review Panel Comments of Chapter 6: Hydrologic needs-effects of hydrology on the Everglades Protection Area (10/16/02)

81. The authors agreed to add specific objectives for each study and discuss their relevance to CERP. A general comment expressed by the peer review panel was that those studies must be more focused and address the relevance and implication to the management of Everglades. A brief review of hydrological inter-connection among elements of interest in each study such as: hydroperiod and wading birds; dissolved oxygen, hydrodynamics and decomposition; erosion, sedimentation and topography; salinity and vegetation etc. is recommended. A consistency in units (metric or English) throughout the chapter is necessary. Following are specific comments and suggestions:

Reply Sklar— We will add more specificity to each discussion of results so the audience has a greater appreciation of its relevance. However, we are alarmed by the general comment that these studies are not focused or relevant. Increased focus and relevance is of course desirable, and may be achievable. At the same time, the panel should be aware that broadly critical comments provide tempting justification for District Governing Board and/or Exec. Branch members to reduce or even eliminate research budgets. Such comments from the panel can easily be used to suggest incompetence and a program out of control.

The panel might want to combine its recommendation for increased integration of research efforts, particularly with CERP, with acknowledgement that ongoing research is applied, not basic, and that it appears to be motivated by operational and ecosystem management needs. We note that all Everglades Division research addresses either hydrologic or water quality questions, and in almost all cases does so through testable hypotheses. We also note that larger studies are externally reviewed and all are annually reviewed for relevance during the budget cycle. We question whether a document like the ECR, designed to simply update a general audience of our latest findings, is the best tool for evaluating the efficiency and complexity of our entire CERP, RECOVER, EFA, Minimum Flows and Levels, Adaptive Protocols, Regulatory, 404 Permit, and Fl Bay Feasibility research programs. I hope that in preparation of ECR 2004 this panel and the authors of this chapter are given better instructions as to the nature of the review process and the requirements for chapter content.

Hydrological Trends

82. Hydraulic retention (or residence) time is an important hydrological parameter of any wetland system. It should be monitored and included in the interpretation of DO, decomposition and many other related issues.

Reply Sklar—Good suggestion. We will consider this type of analysis for the next ECR.

Salinity patterns in Florida Bay

83. The report stated "Salinity in the eastern and central bay was not well correlated with freshwater flow from the southeast Everglades." According to Fig. 6-6, correlation between the freshwater flow and the salinity was very good. The relationship, however, probably had a phase shift (out of phase in terms of time) and that may have messed up the correlation analysis. The

flow vs. salinity relationship needs to be analyzed with phase shift in mind. The phase shift of the relationship is also an important piece of information in the study.

Reply Rudnick-- Figure 6-6 shows the approximate synchrony of seasonal increases in freshwater flow and decreases in salinity in eastern and central Florida Bay. However, simple linear regression analysis of salinity and flow data show that this relationship is weak. Analysis of phase shifts was done, using a monthly time step (which was the sampling interval). When this shift was not taken into account, no significant relationship between flow and salinity was found (eastern and central bay $R2 \leq 0.01$). A significant, but weak relationship was observed when time lags were incorporated. As stated in the report text, the best fit was observed with a three month lag, but R2 values were still only 0.25 for the eastern bay and 0.20 for the central bay. This weak relationship is likely a consequence of the complex circulation patterns of Florida Bay and the relatively low rates of freshwater discharge (i.e. that salinity is more dependent on local rainfall, evaporation, and circulation than freshwater flow). It also reflects the weakness of such a simple statistical approach. Recognizing this weakness, CERP's Florida Bay and Florida Bay Feasibility Study plans to develop both mass balance and hydrodynamic models for Florida Bay. Coupled with upland hydrologic models, this is expected to greatly improve our understanding of the effects of changing freshwater stages and flows of Florida Bay salinity

Crayfish

84. What is the ecological significance of the burrow depth of crayfish? Does it affect the food chain or something significant in the Everglades?

Reply Huffman-- This study found that crayfish dry season survival behavior (burrowing) is strongly related to seasonal declining water levels. The groundwater table is dynamic and consistently falling throughout the dry season. Likewise, the crayfish created deeper and deeper burrows as the dry season progressed. Burrow depth was used as direct measure of crayfish response to a natural environmental stressor. One of the effects of human consumptive water use is an overall lowering of the groundwater table, and altered hydroperiod. Crayfish are a documented key species, and a population's survival is strongly influenced by the presence/absence of water in wetlands. This study showed that the crayfish do not dig burrows to a predetermined depth (e.g., 1/2 meter, etc) but actively dig deeper as the groundwater falls further bwlow ground, apparently seeking some level of moisture in order to pass the dry season. Understanding this relationship allows water managers to make more informed decisions about adjusting water resources allocation for both environmental and public use demands. Loss of crayfish would affect the food chain, although to what degree is unknown. Many restoration targets (e.g., white ibis and other wading birds) depend heavily upon crayfish as a food source at certain times. There are also many species that feed exclusively on crayfish, like the crayfish frog and crayfish snake. Crayfish also structure the macrophytic environment as herbivores, and, as detritevores, move energy through the wetland food web.

Herpetofauna of Tree Islands

85. The literature review and preliminary investigation on the herpetofauna of tree islands are to be encouraged because they provide vital ecological information of Everglades in terms of effects

of altered hydrology and other impacts. It is an important to get more baseline information such as this for future reference.

Reply Garrett— We agree with your assessment.

Soils and Sediments

86. Fig. 6-8 and Fig. 6-9 should be under the sub-heading of Soils and Sediments not under Herpetofanna of Tree Islands.

Reply—This has been fixed.

87. 6-30, Soil organic matter formation is from both aboveground and below ground production, not just belowground production.

Reply Sklar— New sentence: "As a result, belowground production of mangrove peat is considered the primary soil building mechanism."

88. 6-31, Para 5, I could not see from Fig. 6-8 that negative elevation change had occurred. The statement of the last sentence is contradictory to the results of statistical analysis.

Reply Coronado— Text has been modified to describe what figure 6-8 indicates. However, even though elevation change has been constant at marsh and flooded environments, shallow subsidence, which is highest at the flooded environment, is the factor that would make these environments compromise their persistence in the event of an accelerated increase in sea level rise. The last statement has been modified to match what the statistical results are indicating us.

89. 6-32, Those results suggest that the dry, marsh and flooded environments were generally keeping pace with the relative sea-level rise by sedimentation and OM building (within the margin of errors). I do not see how hurricanes can help to explain the accretion of the wetland. Hurricanes may deposit and wash out sediments. Did you see the white marl layers in soil columns all over the place as the feldspar marker layer showed in the experiments?

Reply Sklar— You may be right that hurricanes can be erosional. However, studies in Louisiana indicate limited places of erosion and extensive regions of deposition. Our only interaction with this type of high energy event has been with Hurricane Irene and we found no evidence of erosion and only very limited deposition across the Buttonwood Ridge (see Figure 6-9). Most of the coastal wetland appeared unimpacted.

Peat Microtopography and Spatial Pattern in the Ridge and Slough Landscape

90. Hydrodynamics (water flow) may be important to the decomposition and vegetation of the sloughs. Faster water flow and submerged vegetation may increase DO, which enhances decomposition. The decomposition would lower the peat accumulation that feed back to increase

the flow. The flow, however, will increase erosion and provide a negative feed back effect. Flow, microbiology and vegetation may interact to create the observed spatial pattern (Fig. 6-10) of the slough and ridge landscape. Lack of water flow probably, on the other hand, would result in the patternlessness of Fig. 11, according to the above reasoning. Those speculations could be resolved by further studies.

Reply Sklar— These are all good observations and they are variables that need to be addressed within an integrated Ridge & Slough research program. Unfortunaetly, no such program exists at the District. The Park has recently initiated a $1.5 M ridge & slough study in Shark Valley. After we review the Park's program and the preliminary studies by Miao, Penton and McVoy at the District, we will be in a better position to draft a ridge and slough program.

Decomposition

91. One of those titles of Fig. 6-12 or 13 is in error. The interpretation of this section is quite confusing and needs to be presented in a clear manner.

Reply Penton—Text describes these two indices. Due to a typo, the meaning of the En index became reversed. Corrected text: "When En ratios are greater than 1, nitrogen mineralization is occurring. When the ratios are less than 1, nitrogen immobilization is occurring".

Vegetation

92. How do the biomass of E. celluosa and R. tracyi compare to those of sawgrass and cattail? This information may be important to predict the rate of peat accretion under different species.

Reply Miao-- Since both E. celluosa and R. tracyi compare are much smaller than sawgrass and cattail, their biomass (g/m2) are several folds lower than sawgrass and cattail. For example, the aboveground biomass of sawgrass and cattail were between 600 and 750 g/m2, whereas that of E. celluosa and R. tracyi was between 100 and 130 g/m2

93. 6-45, Was there any seed bank problem found in the changeover of obligate and facultative species?

Reply Coronado— No, there is not a seed bank problem associated to the fluctuation of marsh plants species observed at Rotenberger. In fact, due to changes in hydrology the data suggest that some obligate marsh species (i.e Utricularia) have been successful in recolonizing a marsh environment that has been subjected to drought and fire events that occurred previously to start discharging water from STA-5 (Stormwater Treatment Area). However, more data are needed to better evaluate the effect water discharge into Rotenberger.

Tree Island Ecology

94. Some discussion on the tree species-water regime association should be given.

Reply Sklar— The whole point of this study is to document the water regime association. However, data contradictions have been found between growth, litterfall and community composition. Maybe next year we can be more conclusive.

Remote Sensing and Modeling Trends

95. Can aerial photographs and satellite images differentiate tree species in Fig. 6-21? If the resolution of Z of remote sensing is not good enough (considering most elevation differences in Everglades are within 1 m), vegetation species could be a valuable indicator to the micro-topography (because the association between hydroperiod and topography) of the Everglades.

Reply Rutchey— This is possible and it depends on the scale of the photography or resolution of the satellite imagery being used. Part of this research is to evaluate whether the hyperspectral imagery is able to differentiate between different tree species. However, one of the reasons we obtained both LIDAR and hyperspectral data is because of the potential synergistic effect of having both of these data sets utilized for solving species identification.

96. Clarify objectives:

Reply Rutchey-- I think that the Lidar and Hyperspectral text covers the relevance and implication to the management of Everglades. "This work will include applying up-to-date airborne LIDAR techniques with Hyperspectral digital imagery, together with field measurements of vegetation characteristics to develop a prototype monitoring program to enhance the ongoing District tree island research program and assist in evaluating the effectiveness of the CERP." Likewise I think the NASA collaboration text also covers the relevance and implication to the management of Everglades "As such, the District's goal for the NASA collaboration is to develop a more cost-efficient operational procedure for mapping the spatial and temporal characteristics of the greater Everglades using newer methods, such as hyperspectral remote sensing techniques."

For IKONOS data you could add the following. "Old World climbing fern (Lygodium sp.) is an exotic species that is currently being established system-wide in extremely remote and undisturbed areas such as the Everglades. It has reached a critical mass in south Florida where it is expected to exponentially increase its rate of expansion. IKONOS satellite imagery is being investigated as a tool to map Old World climbing fern (Lygodium sp.) to give land managers a better way of tracking and developing site specific methods for eradication."

Chapter 7A

Responses to Richard A. Meganck's comments

1. The 2003 Draft Report contains the statement (page 7A-2) in the History section "The degradation of the South Florida ecosystem must stop. It must be restored, preserved and protected." I believe that this statement raises many questions for which we do not yet have answers. First the "degradation" statement should not be attributed to the District, as it is value laden. In addition, the term "preserved" will most certainly attract the attention of urban planners and those interested, as it is my understanding that domestic use has priority over all other uses. Finally, the term "restored" is misleading as it implies that the District can take certain management decisions and return natural systems to a prior state. This is speculative at best and certainly will lead to misunderstanding on the part of the public.

RESPONSE: The statement has been removed.

2. The critical projects identified in the report were supposedly identified to provide quick responses to immediate problems. What results have been forthcoming from these efforts.

RESPONSE: As of September 2002, none of the critical projects have been completed.

3. On page 7A-3 it is noted that "existing animal and plant populations have adapted to some degree to the altered ecosystem." Are these animal and plant populations ones that will be maintained? If so, won't these areas be further affected by CERP activities and therefore negatively impacted from the point of view of existing wildlife populations? Again, if the determination is made that further change to the adapted system is acceptable, then we should drop the pretense that we are restoring the everglades system to some predetermined historical point and are going to maintain it at that point. Rather, the District should be clear that the overall goal is to make the Everglades more natural by restoring historical hydrology patterns and volumes, but that management cannot restore and maintain any natural system in a static sense.

RESPONSE: No pretense is made in Chapter 7A that the Everglades system is going to be restored to some predetermined historical point. The goals of the CERP are clearly stated in the Summary and Highlights section as "to restore the quantity, quality, timing and distribution of water".

The existing animal and plant populations will be monitored closely to insure that the restoration effort does not cause long-term negative impacts to the populations. If such impacts are determined to be occurring, the adaptive assessment process will refine CERP projects to prevent further impacts.

4. Why are PMPs and PIRs not required for "critical projects". It is not clear to me and should be explained in the document.

RESPONSE: The critical projects were authorized separate and prior to the CERP. These projects required brief project reports, referred to as "letter reports". These letter reports can be viewed on the USACE's Jacksonville District Web site (www.saj.usace.army.mil).

5. How secure is the funding for all of the projects planned as part of the CERP? Is the District prepared to keep the public fully informed as to the consequences should funding be reduced or stopped for any reason?

RESPONSE: Funding is guaranteed for 10 years.

6. The National Academy of Sciences recently reported that the CERP might negatively impact water quality in the Florida Bay, at least from the public's point of view. The Academy called for more research into the relationships between CERP activities and the Bay. What is the District's response to reports such as this, particularly once such information has been published?

RESPONSE: RECOVER was aware of this issue prior to the National Academy of Sciences report being issued and additional research is already being developed. RECOVER takes all such reports and concerns seriously, and will take the actions necessary to resolve these issues through the adaptive assessment process.

7. Based on the comments on chapter 2A by the representatives from the U.S. Fish and Wildlife Service and the National Park Service on the compatibility of their agency mandates (referring to protocols and reporting procedures for managed alkalinity, dissolved oxygen and water quality goals/levels, etc.) with those of CERP, I am concerned as to the cumulative potential impact on the CERP particularly as it relates to the ability of District management to make decisions that will actually render the result desired. This is a very serious issue and one that I had assumed had been negotiated early in the CERP development process. The discussion did not resolve this issue. Simply bringing to the attention of the District that a discrepancy exists in indicators is not solving the problem. Rather I was hoping that the response would touch on the method for resolving these discrepancies and reporting the results in the triennial review. At a minimum the District should clearly state that applying the results of science is an iterative and incremental process.

RESPONSE: This issue is dealt with in Chapter 7B.

Everglades Program Team Review Comment's

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.

RESPONSE: All acronyms used in the chapter are included in the Acronyms and Abbreviations section of the ECR.

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

RESPONSE: The correction was made

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).

RESPONSE: The revised implementation schedule is now included as an appendix.

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?

RESPONSE: The existing animal and plant populations will be monitored closely to insure that the restoration effort does not cause long-term negative impacts to the populations. If such impacts are determined to be occurring, the adaptive assessment process will refine CERP projects to prevent further impacts.

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.

RESPONSE: The line does intentionally decrease before it increases for the reasons stated in the comment. This decrease is now noted in the text of the chapter.

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

RESPONSE: Best is too vague of a term. Feasible implies that the solution that will achieve the highest level of restoration cost-effectively.

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

RESPONSE: If this information is available at this time, Chapter 8C will contain it.

Chapter 7b Response to Comments

Everglades Program Team

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 not recommend application of the current version of a model.

Response: Text revised to state use of ELM is being considered.

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

Response: Text in figure clarified.

Peer Review Panel

Chapter 7B: RECOVER Activities

First and foremost, the relationship between RECOVER and CERP is clear. This is important for the general public as it is to the RECOVER team that they will logically turn when faced with a question or issue that requires further information. The Panel supports the long-term goal of a total ecological model to evaluate the interactions among the regional models and the upstream and downstream effects of transboundary (pilot project limits) actions.

Response: No response needed.

The RECOVER process, developing and implementing an adaptive management program for the CERP, is a critically important part of the overall CERP program, and must be based on a well-designed and well-supported program of monitoring, assessment and research. So far, most of the development efforts appear to have focused on identifying ecological indicators. The chapter gives a good general description of the RECOVER process to date, but leaves open numerous questions that are perhaps beyond the scope of this report. For example, have clear restoration goals and targets been established? Do

they recognize possible ecological trade-offs among various elements of the Everglades system?

Response: The answers to these questions are addressed in the CERP Monitoring and Assessment Plan (final draft due December 2002).

How are exogenous forces in the South Florida region (such as population growth, economic changes, land use changes, sea level rise, etc…) incorporated into the identification of indicators and the establishment of goals?

Response: These factors will be taken into account during the Initial CERP Update and the development of interim goals.

Figure 7B-1 and the explanatory text provide a very good basis for public understanding of how the CERP process will be monitored and adapted over time. However, it is always suspect to make statements that purport to fully understand the impacts of applying a number of management strategies on any system at the landscape level.

Response: Uncertainty in ecosystem restoration is fully recognized in the Monitoring and Assessment Plan, but will be made clearer in this chapter next year.

The concept of setting interim hydrologic restoration stressor-based performance measures is a good one. Obviously the District cannot wait until all of the hydrologic works are completed before beginning to determine their impact on the system. The targets identified by the Alternative Evaluation Team will logically give an indication if restoration efforts are proceeding in the proper direction. This is a valid goal and one that does not require additional definition, in my opinion, at the outset of this process. Some limited and qualified data are better than no data at all. Adapting the model and redefining measurement points and criteria, as new data become available, is the only logical way to proceed. However, transferring results from the site level to the landscape level may present difficulties as there are a number of related indicators that may differ between scales.

Response: No response needed.

The institutional implications of the last RECOVER objective "Develop a consensus…" noted in the 2002 Consolidated Report continues to critical to future management of the region and should be given priority from the outset so as to catalyze joint ownership of the program.

Response: No response needed.

The MAP is logically presented and needed. The Panel suggested a sixth section to the MAP, one identifying potential management implications of the monitoring efforts. The Adaptive Management Program detailed on page 7B-10 is a very important tool that will permit the District to keep the public informed as to tangible progress on the restoration

effort. The success of this ambitious inter-institutional effort is apparently based on developing a proactive partnership where no one agency controls decisions. As the results of pilot projects and other field trails and investments (policy or infrastructure) become clearer, adjustments affecting certain programs and agencies will be required. This, in turn, requires true institutional collaboration.

Response: The panel's recommendation that a sixth section be added to the MAP is partially explained by Appendix 7-6 (RECOVER Adaptive Assessment Protocol). This protocol is the beginning point for a CERP Guidance Memorandum on the assessment process. The guidance memorandum, when completed, can be incorporated by reference into the MAP.

The CERP Annual Report Card continues to be a useful approach to document and report progress toward recovery of elements of the ecosystem, and for informing the general public. It should not be considered adequate to communicate with decision-makers or various stakeholders, however. It will be important to recognize that some of the variation in the performance measurements may be the result of unexpected influences not related to CERP activities. Thus, it would seen critical that monitoring of the indicator elements of the report card include research to establish cause-effect relations between the performance and CERP activities vs. effects of environmental variation or other external influences.

Response: Concur. See section on the Adaptive Management Program.

The milestones noted in Table 7-1 on page 7B-13 seem logical but will need further explanation to ensure that the public understands the reason for these interim goals. For example, why is it important to extend the climatic period of record from 31 to 36 years? Also it may be of interest to note how these milestones related to the overall monitoring program.

Response: The text has been clarified as to the extension of the climatic period of record to 36 years and that the milestones refer to the anticipated target dates for completion of Initial CERP Update tasks.

Revisions to Ch 8A in Response to Peer-review Comments

- Text was added regarding the District's intent to continue research and STA Optimization efforts to address the remaining scientific engineering and scientific uncertainties.

- A more detailed description of the DMSTA model was added.

- Clarification was added to the text and tables to indicate that the DMSTA modeling work performed by the consultants was completed using the SAV_C4 calibration set.

- Text was added to indicate that there is on going scientific debate as to the most appropriate calibration data set to use. A description of the method used to address this uncertainty was added.

- Text was added to describe the peer-review process utilized throughout the completion of the basin-specific feasibility studies.

- In Table 8A-2, the simulated outflow concentrations for STA-3/4, STA-5 and STA-6 were revised to reflect results of consultant's DMSTA modeling.

- Text was added describing the various sources of phosphorus loads received by the Everglades Protection Area.

- Figure 8A-1 was added showing the phosphorus loads contributed to the Everglades Protection Area for Water Year 2002.

- Tables 8A-6 and 8A-7 were added that document phosphorus loads contributed to the Everglades Protection Area for Water Year 2002 by source and structure.

- References were added.

Review of Chapter 8B: The Everglades Stormwater Program

REVIEWER 1: MIDDLEBROOKS (JEFF JORDAN) 9/16/2002

Chapter 8B is an excellent summary of the efforts and successes of the ESP. Including summaries of ranges of data and results of monitoring for the various basins would improve the chapter. The tight time frame mentioned in Chapter 8A for implementing the EFA still exists and may make it difficult to fully utilize the information from the scientific and engineering studies.

The changes in regulations providing the District with more flexibility in dealing with stormwater outside the District will help achieve the objective of preserving the Everglades. Completion of the science and engineering and cooperation among the numerous organizations involved in this complex task must occur before imposing arbitrary standards.

The approaches to evaluating and treating stormwater are reasonable, and if the research and coordination effort are completed before an attempt is made to implement control strategies, restoration of the Everglades has an excellent chance; however, success depends upon completion of the studies and a cooperative effort.

The importance of integrating all of the activities designed to recover and protect the Everglades cannot be over emphasized. Cooperation between the various entities and the Legislature is the route to success. Section Comments

INTRODUCTION

The updated map is very useful and gives a good perspective of the problem facing the entities engaged in protecting the Everglades.

Comment: Why do the Wellington/Acme Improvement District, Feeder Canal, and L-28 basins not meet standards?

Response: Because they were not meet standards in previous year. A sentence "It should be noted that there were significant TP concentration reductions in WY2002 comparing with previous years in Feeder Canal and L-28 Basins." was added in the revision.

STATUS AND PROGRESS OF IMPLEMENTING THE ELEMENTS OF THE ESP

ESP WATER QUALITY ANALYSIS AND MONITORING PROGRAMS

Comment: Where did the Diazinon and chlorpyrifos (ethyl) originate?

Response: Sentence: "Diazinon was detected in two surface water samples at the North Springs Improvement District 1 (NSID-1) pump station, where diazinon is the most widely used pesticide by homeowners on lawns, gardens to control insects and grub worms; chlorpyrifos (ethyl) was detected in one sample at S-177, where chlorpyrifos is a pesticide used on food and feed crops, ornamentals, lawns and turf; and ethion was detected in one sediment sample at S-176, where ethion is a pesticide used for citrus grown." is added in the revision.

Comment: Second paragraph, line 3. Why not report the differences rather than using the word "relatively."

Response: word **"**significantly" replaced "relatively" for summarizing the flow. There are many structures, interested readers can find difference by comparing the Table 8B-1 of 2002 ECR and this consolidated report for each structure.

Comment: Line 5, the punctuation should read as follows: "this consolidated report; therefore, the comparison".

Response: Changed has been made according to comment.

Comment: Fourth paragraph. Restructuring the sentence as follows would make it clearer.

"The ACME1DS and G-94D culverts, operated by VOW/ACME, remain open at all times when upstream pump stations ACME1 or ACME2 are operating and discharging to the Arthur R. Marshall Loxahatchee National Wildlife Refuge (Refuge)."

Response: Change has been made according to comment.

Comment: Table 8B-1. Define the superscripts.

Response: The superscripts have been defined in the revision.

Comment: Page 8B-6, third paragraph, it may be desirable to briefly define "timed sampling."

Response: (water sampled at periodic intervals) was added to briefly define "timed sampling."

Comment: Page 8B-6, fourth paragraph, what effect will eliminating the need for these farms to pump west in the A.R.M. Loxahatchee National Wildlife Refuge?

Response: "the Lake Worth Drainage District (LWDD) is continuing in its efforts to provide sufficient capacity to allow all discharges from the remaining farms to go east" as stated in the previous sentence.

Comment: Page 8B-7, lines 2 and 3, why not provide a brief summary of the data shown in

Table 3 of Appendices 8B-1b and 8B-1e?

Response: a brief summary "which indicated flow weighted TP concentration of 26 ppb" is added in the revision.

FINANCIAL ASSESSMENTS

Public Outreach Initiatives

Comment: Is it feasible to prepare summaries of the referenced documents for inclusion in this chapter? I realize that the documents are readily available, but many readers needing a synopsis will not access the Internet.

Response:

Comment: Page 8C-11, second paragraph, Are there plans to put this document on the Internet?

Response:

PROGRAM MANAGEMENT AND IMPLEMENTATION

UPDATES OF ACTIVITIES IN ESP BASINS

Comment: North Springs Improvement District Basin (NSID), Second paragraph, first sentence, insert comma after "potentially higher." Line 4, a capital "T" is needed at the beginning of the sentence.

Response: Changes have been made according to comment.

Comment: North New River Canal Basin, Line 5, punctuation needs to read as follows: "'last 18 months; therefore, their agreement."

Response: Changes have been made according to comment.

Comment: C-11 West Basin, Third paragraph, line 10, define NGVD.

Response: (National Geodetic Vertical Datum) is added to define NGVD.

Comment: L-28 Basin and Feeder Canal Basin Because of the repetition in these two sections, perhaps it would be possible to combine the two.

Response:

REVIEWER 2: RICHARD A. MEGANCK

The goals of the non-Everglades Construction Project (ECP) permit schedules and strategies as stated in the summary and introduction sections to this chapter are clearly stated. I note with particular interest the mention that public outreach efforts have been expanded to address certain issues of interest to the general public. Figure 8B-1 presents important information on the extent of the Everglades Protected Area (EPA) as well as a wealth of information on discharge/water quality data collection points that I have always felt need to be better communicated to the general public.

The section of the report dealing with the status of progress of implementing the ESP beginning on page 8B-4 is very well written and presented in a logical manner.

I continue to believe that the potential weaknesses as well as future problems associated with not meeting the 2006 deadline for P levels should be included in public outreach efforts. I also feel that an effort to communicate the integrated nature of the actions being undertaken should be included in public education materials.

Comment 1. Is there any way to combine some of the reporting requirements noted in table 8B-2?

Response:

Comment 2. It is not clear how the activities contemplated under CERP will impact those that will be completed as part of the ESP. Is there anyway to clarify these relationships in the text of the document?

Comment: 3. Last year I asked about the result that "more than 75 percent of the data collected at the upstream VOW/ACME monitoring sites were below 95 ppb…" It is still not clear to me if this represent a downward trend since STAs were installed?

Response: It is inappropriate to conclude a downward trend because WY2002 is wet comparing with WY2001. Trend can be concluded with multiple years' data.

Comment: Even though the total water retained by the STAs is approximately 5% of the total water flowing south from the EFAs, has there been any study on either the loss of freshwater water surges and sediment composition on the ecology of the Florida Bay?

Response:

Reviewer 3: Everglades Program Team (EPT)

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CHAPTER 8B: THE EVERGLADES STORMWATER PROGRAM

Comment 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.

Response:

Comment 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).

Response: The main chapter presented only the data of "INTO" structures (discharged into ENP) S-18C, S-332, while other C-111 structures S-175, S-332D and S-174 data are presented in Appendix 8B-1a of this consolidated report.

Comment 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).

Response: The S-332B is not a Non-ECP structure therefore is not reported here. The data might need to be reported when permit is issued in the future.

Comment 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.

Response: A summary paragraph is added in this revision. "The lowest TP concentrations were observed at structures in the C-111 Basin at S18C, S174, S177, S-331, S173, S-332D, which discharge to the southeastern portion of the Park by way of the C-111 Canal and Taylor Slough. The TP data for these monitoring locations had an observed median concentration of 6 ppb for S18C, S175 and S332, with 75 percent of the samples having concentrations below 10 ppb for S18C, S175, and S332. During WY2002, the S175 and S332 structures were operated infrequently, and discharged only 6 acre-ft for S175 and none for S-332, to the Everglades

National Park. The S18C Structure discharged approximately 172,835 acre-feet to the lower C-111 Canal. It should be noted S178 had a flow-weighted mean concentration of 93 ppb, with a discharge of 4,398 acre-ft. It had the highest TP concentration in the C-111 Basin." There is no endosulfan concern according to monitoring data for WY2002 for Non-ECP pesticide monitoring sites, therefore it is not mentioned in the main text. There was no automatic sampling for the S178 during WY2002, therefore it is not mentioned here.

Comment 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.

Response: Chlorpyrifos and ethion concentrations were discussed in the revision.

There is no Atrizine and endosulfan concern based on monitoring data for WY2002 for Non-ECP pesticide monitoring sites, therefore it is not discussed here.

Comment 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.

Response:

ECR Chapter 8 E Reponses

Specific questions:

♦ On page 8E-16, it is stated that the District plans to conduct experimental herbicide applications for Old World climbing fern on tree islands in the Refuge in 2001. Was this done? Is there any information of the outcome of these applications.

Response: This was an oversight on the author's part and this paragraph has been updated.

♦ What effort is being made to coordinate invasive species management plans with the CERP, and to assess long-term changes resulting from CERP activities on their future management. The description of research needs suggests that this is not being addressed, and perhaps this should be pointed out more directly as a need.

Response: The author feels that the issue has been given adequate attention in the chapter.