Appendix 1-2: Comments on the *2004 Everglades Consolidated Report* from Outside Persons and Organizations

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

With the exception of reformatting some information for better readability, the Chapter 1 appendices were not edited or spellchecked by the ECR production staff. They appear as posted on the District's WebBoard.

United States Department of the Interior

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September 19, 2003

Dr. Jeffrey L. Jordan, Professor and Panel Chair
2004 Everglades Consolidated Report Peer Review Panel
Dept. of Agricultural and Applied Economics
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Dear Dr. Jordan:

Please find enclosed comments on the draft *2004 Everglades Consolidated Report*. These comments were provided by staff of the Everglades Program Team (U.S. Department of Interior), the Arthur R. Marshall Loxahatchee National Wildlife Refuge, Everglades National Park, and the Ecological Services Branch of the U.S. Fish and Wildlife Service (Vero Beach, FL). These comments are technical in nature and do not necessarily represent official policy of the Department of Interior or its agencies.

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

We are pleased that all review comments to the draft *2003 Everglades Consolidated Report* were published in the final version. As we have commented before, it would be extremely useful for the review panel and others (including chapter authors) if the draft version of future *ECR*s were distributed with line numbers on each page to facilitate reference of review comments.

Chapter 1

1) p. 1-7, last paragraph: This paragraph would be an appropriate place to discuss the impacts of loss of flow in the Everglades, particularly in the ridge and slough habitat. A good reference is the Science Coordination Team's flow paper (Science Coordination Team. 2003. The role of flow in the Everglades ridge and slough landscape. Report to the South Florida Ecosystem Restoration Task Force Working Group), which was externally pee-reviewed, and submitted to the Working Group and the Task Force.

2) p. 1-10: Mention of the Everglades lawsuit and the relevance of the Settlement Agreement would be useful under this heading.

3) p. 1-12, 1st para, line 6: should be A.R.M. Loxahatchee National Wildlife Refuge.

Chapter 2A

1) General: This chapter provides a valuable assessment of the water quality data collected during the year throughout the EPA.

2) General: Again this year, there is concern that this chapter gives too much credibility to the Florida Class III Numerical Criteria. From the perspective of resource and ecosystem protection, it is inadequate to focus exclusively on standards that are known to be inappropriate or not protective. Some of the Class III Criteria are clearly inappropriate, and worse, not protective or sufficiently protective of our unique Everglades ecosystem. The minimum conductivity criterion of $\leq$1275 µmho/cm not to be exceeded is far above historic values for most of the ecosystem, and is completely unprotective of the refuge interior. As discussed in the chapter, the alkalinity criterion of $\geq$20 mg/L is also inappropriate for the naturally soft water dominated refuge. Prior to construction of drainage canals and agricultural land use changes, much of the rest of the Everglades was probably also soft water with low alkalinity. It is troubling that a major standards development effort is directed toward developing an SSAC (site specific alternative criterion) for DO that will lower standards, while little effort is being directed toward developing more stringent criteria for inadequately protective standards.

3) General: Again this year there is little consideration of anti-degradation of the refuge and park as outstanding Florida Waters (OFWs). Simply applying numerical criteria is not protective of OFWs. Recommend that the DEP develop estimates of the appropriate background concentrations for use in assessment of compliance with anti-degradation within the OFWs. Background concentrations of at least chloride, TDS, hardness, alkalinity, conductivity, calcium, sulfate, and total nitrogen should be estimated and used in future reports.

4) General: The report emphasizes results only for the current water year. Results are difficult to interpret without historical perspective. Detection of change is a major management objective. The report should present cumulative analyses of historical data using consistent data-reduction and data display methods. Statistical trend analyses should be performed for key indicators. This applies to water quality, hydrology, inflow and outflow nutrient loads treatment technology, and biological data.

5) General: Where is the discussion on compliance with consent decree levels, limits, and load-reduction requirements?

6) p. 2A-1, para 2, ln 8: Should read something like: "…resulted in a reduction of excursions…" not of stations. It conveys the idea that the number of stations has been reduced.

7) p. 2A-1, para 3, ln 3: Need to specify that excursions were below the standard; same for pH.

8) p. 2A-2, para 1, ln 3: add "low" pH.

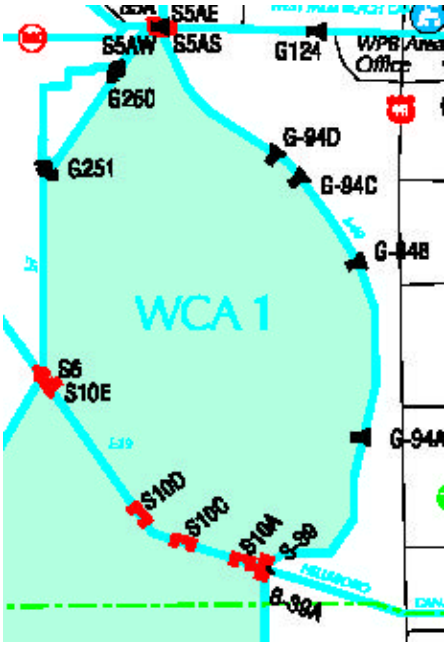
9) p. 2A-2, para 2, ln 5: Seepage and evaporation have occurred in the past, have they not? So, why is there a problem now? Were previous samples taken during the dry season?

10) p. 2A-2, para 4, ln 1: need to add pesticides considered of "potential concern".

11) p. 2A-2, para 5, ln 5: strike out "…are intended to use…" or rephrase sentence.

12) p. 2A-3, Everglades Protection Area Water Quality Sampling Stations: The text discusses the diversion of the S-6 pump to STA-2. At the S-6 pump there is a gate that allows some discharge to the refuge. Although this gate is not anticipated to be used except in extraordinary circumstances, it is appropriate to include it in the list of inflow stations and report no-flow (if that is the case) for the water year being reported.

13) p. 2A-6: Fig. 2A-2 also shows the G94B, but fails to show the location of the G-94A and G-94C structures (see figure below extracted from the SFWMD structures map). Although routine water quality sampling does occur at these structures, they are outflow points and should be noted on the map.



14) p. 2A-9: Fig. 2A-5 shows an inset of stations downstream of the C-111 inflows in Taylor Slough. It would assist the reader to have the location of the inset region identified on the smaller scale map by a rectangle.

15) p. 2A-11, Excursion Analysis: As commented last year, when uncertainty is higher we need to be more (not less) conservative and protective of the ecosystem and human health. This is particularly true for OFWs. The statistical approaches used in this chapter troubling because it violates this principal. It's OK to understand the desire for consistency with other evaluations including the Florida Impaired Waters 303d designations and understand that it may reduce the required effort and increase efficiency. However, no justification is developed in the chapter that these methods are appropriate for the purposes of this report.

The example provided on page 2A-11 clearly illustrates the problem with the excursion analysis approach applied in this report. It is stated that:

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

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

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

The change of evaluation criterion (p. 2A-12) from 5% to 10% exceedance frequency needed to define a "variable of concern" is troubling because it weakens protection of the aquatic resources. The argument that this is recommended EPA guidance ignores the fact that Florida defined the numerical criteria based on an assumption that a 5% exceedance rate would be applied. More stringent numerical criteria should first be defined that anticipate a doubling of exceedance frequency. Only after this numerical criteria adjustment should the 10% exceedance rate be applied in identification of concerns.

The use of a 20% exceedance rate for variable with fewer than 28 samples further reduces protection of the resource. As discussed above, the added uncertainty of small sample size increases, rather than reduces, the need to identify a potential problem or impact.

16) p. 2A-13, para 1, ln 3: "*potential concerns"* should be those occurrences between 10% and some percentage > 10, for example 15%; situations where the # of samples is less than 28 should be subjected to other type of analysis.

17) p. 2A-13, para 2, ln 5: according to these grades, "D" means unsatisfactory, but does it mean passing? According to text only A and B are passing. Please explain.

18) p. 2A-13, para 3, ln 6 and p. 2A-14, para 1, ln 3: potential concerns in these paragraphs have a different meaning than that given in p. 2A-13, para 1, ln 3.

19) p. 2A-15, 2nd para: Reference is made to "Refuge rim canal station S-5AD." The site is also mapped in Fig. 2A-2. DBHYDRO describes this station as "DOWNSTREAM S5A NORTHERN MOST POINT OF WCA1 SOUTH OF SR80 PUMP STATION." This site's period-of-record extends to dates prior to the construction of the STA distribution works and the G-300 and G-301 gates. Is this site upstream of the G-300 and G-301 structures? If so, it is located in the STA distribution works and is not located within the refuge rim canal.

20) p. 2A-21: What is the sensitivity of the DO SSAC? For example, station F4 in WCA-2A is listed in Appendix 2A-3 (p. 2) as a "Pass", while published results (McCormick and Laing, 2003. Effects of increased phosphorus loading on dissolved oxygen in a subtropical wetland, the Florida Everglades. Wetlands Ecology and Management. 11: 199–216) from the same data set concluded that this site is impaired. The SSAC appears to be at least moderately insensitive to P-induced declines in DO.

21) p. 2A-21: What are the plans for using the SSAC? If it is used to define the zone of impact, will this shrink or enlarge the area where moderating provisions will apply?

22) p. 2A-23, Fig. 2A-6: Caption should read, "… within the A.R.M. Loxahatchee National Wildlife Refuge."

23) p. 2A-23, Fig. 2A-6: Shouldn't water in canals have a more consistent alkalinity concentration (for example) ~200 mg/L everywhere? This is not clear in Fig. 2A-6 (computer-generated contour lines seem a bit off). Have rim canal sites been used to provide a boundary for this map?

24) p. 2A-25, 1st para: This paragraph should state that specific conductance (conductivity) is (or is not) temperature compensated. You should further specify whether field or laboratory measurements were used.

25) p. 2A-25: Recent studies south of the S-10 structures (Krest and Harvey, 2003. Using natural distributions of short-lived radium isotopes to quantify groundwater discharge and recharge. Limnology and Oceanography, 48(1), 290-298) support the hypothesis that the high conductivity observed during dry periods results from groundwater discharge in this area.

26) p. 2A-26/28: The discussion of unionized ammonia suggests that the elevated values are caused by nutrient accumulation and cycling within the rim canal. While these may be immediate factors, excessive external nutrient inputs and resulting organic production and elevated pH in the refuge & rim canal are the ultimate causes.

27) p. 2A-30, Fig. 2A-10: Is there a problem with vertical axes for the total dissolved NH_3? The un-ionized NH_3 axes vary from 0 to 0.18 mg/L in both figures. The total dissolved NH_3 goes from 0 to 4 mg/L (lower fig) and to 3 mg/L (upper fig). It seems that they should have the same scale unless the pH is very different in both stations.

28) p. 2A-31: Please discuss possible reasons why sulfate levels within the interior of the refuge were so high (@ 3-fold over historical values). Was this because of overloading of STA-1W with Lake Okeechobee water, or extensive bypass events?

29) p. 2A-34: Again this year, expanded monitoring of atrazine at STA inflow and discharge sites, as well as at sites throughout the EPA is urged. Atrazine has a wide distribution within the EPA, and controversy and uncertainty about effects remain. Atrazine is a water-soluble herbicide that selectively controls broadleaf weeds in agriculture fields. It is said to be the most widely used herbicide in the world. Atrazine is of particular economic importance to sugar farmers in South Florida. Atrazine is also widely used for other agricultural purposes and for weed control in lawns. Atrazine may also be discharged from point sources such as sugar mills (Chung et al., 1996. Fate and enhancement of atrazine biotransformation in anaerobic wetland sediment. Water Research, 30(2), 341-346). Although atrazine is relatively recalcitrant, it can be mineralized in wetlands (Chung et al. 1996) and removed by constructed wetland systems (Alvord and Kadlec, 1996. Atrazine fate and transport in the Des Plaines wetlands. Ecological Modelling, 90(1), 97-107), however, there is little evidence of atrazine mineralization in the STAs. More frequent sampling at STA inflow and outflow sites would be of value in quantifying remineralization in both STAs and EPA marshes.

    The criterion used in atrazine evaluations is based on protection of human health, as listed in the *2001 ECR*. The USEPA now has proposed guidance for setting criteria for protection of aquatic life (USEPA, 2001a. Ambient Aquatic Life Water Quality Criteria for Atrazine - Draft. *EPA-822-D-01-006*, USEPA, Washington, DC.; USEPA, 2001b. Fact Sheet: Atrazine - Aquatic Life Water Quality Criteria - U.S. EPA. *EPA-822-F-01-006*, USEPA, Washington, DC.). Detenbeck et al. (1996. Fate and effects of the herbicide atrazine in flow-through wetland mesocosms. Environmental Toxicology and Chemistry, 15(6), 937-946) 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. Research directed at establishing appropriate atrazine criteria for the EPA should be initiated.

30) p. 2A-34: DEP should make mention of available evidence on pesticide and other contaminant concentrations in wildlife and not just show available water-column data...after all, what's more important: contaminant hits in the water or in animal tissue?

31) p. 2A-34: It appeared odd that inflows to the refuge didn't have any hits (areas of concern) while most/all other areas did. Is this correct, even though the refuge receives poorer water of water than southern parts of the system?

32) p. 2A-35, Table 2A-8: Are there no data from Oct '02 through 30 April '03 to report here?

Chapter 2B

1) General comment: Chapter 2B is a very readable and well-organized summary of the present state of science regarding mercury. However, the chapter is very short on citations of scientific information to support the many statements and conclusions presented in the chapter. It is recognized that there is a significant amount of scientific information presented in the appendices of this chapter, but the authors should not leave it up to readers to try and find the specific portions of the appendices that may support statement in the chapter. The authors should assume that readers of the *ECR* generally are technically literate and wish to have references included in technical chapters. Many statements and hypotheses, to numerous to note, within the text of the report should be followed by citations that present supportive data.

2) p. 2B-9: Identifying the nature of the source of atmospheric mercury is a technical question, not a policy question.

3) p. 2B-18 and p. 2B-20, Fig. 2B-6 and 2B-8: Please state in figure caption whether the error bars are SD or SE.

4) p. 2B-20, Fig. 2B-8: What might be interesting to examine in Appendix 2B-4 is whether there are differences in Coefficient of Variation pre-1980s and post-1990. This may provide additional insight into the interpretation of the increase in Hg levels.

5) p. 2B-19, Fig. 2B-7: The variability in the data presented, coupled with the low number of data points, indicates that setting a $P = 0.1$ to determine regression significance (and presenting a line and an r^2) may be inappropriate. The text (on p. 2B-17) correctly presents these data as a "trend … decreasing over time."

6) p. 2B-9: Fig. 2B-2 appears to be misleading. Fig. 2B-2 implies that the rate of atmospheric deposition for the year 1810 and later was actually measured. Is this so? Were deposition rates for the 1800s actually measured? In addition, the y-axis units imply that there was a spatial context to the measurements. How was this accomplished with a corer? The reader assumes that what was measured was mercury concentration in a matrix that corresponded to a date. Perhaps a figure with concentration and dates alone would be more accurate if the rate was not actually measured. The reader can then make inferences themselves about historical Hg loading.

7) p. 2B-10, Fig. 2B-3: Is atmospheric deposition the major source of Hg input into the Everglades ecosystem?

There is a lot of text stating that atmospheric deposition of Hg is THE major input of Hg into the Everglades system. The initial such statement should be followed by a citation of a study which demonstrates atmospheric deposition as THE major Hg input into the system. This is because other phrases within the text indicate uncertainty about exactly what is the major Hg contributor:

> p. 2B-9, 1st para, 4th line: "……thereby hopefully decreasing the delivery…."
> p. 2B-11, 1st para, 1st line: "…..has been limited by predictive knowledge….."
> p. 2B-12, 2nd para, 1st sentence: "If control of local emissions of atmospheric mercury is not sufficient to manage the Everglades mercury problem….."

These phrases indicate to the reader, especially without credible citations for the initial assertion, that the earlier statements of atmospheric deposition were hypothetical and that there really is uncertainty about the major Hg sources. If they are hypothetical, state that and discuss the other potential major Hg inputs to the Everglades ecosystem.

8) p. 2B-11: Clarification of the discussion about methyl-Hg bioaccumulation is necessary. The process of increasing chemical concentration with trophic level, as discussed within the first paragraph of the section entitled "the aquatic cycle of mercury – biotransformation and bioaccumulation" on page 2B-11, is more accurately termed biomagnification. Bioaccumulation is an encompassing term comprised of two stages in chemical uptake by an organism, bioconcentration and biomagnification. Biomagnification refers to the process by which, as the chemical is transferred through trophic levels, the concentration of that chemical increases (magnifies) with each successive increase in trophic level.

   It is unlikely that methyl-Hg biomagnification begins with accumulation within microorganisms, as stated in line 6 of the 1st para of the "Aquatic Cycle of Mercury…" section on page 2B-11. Microorganisms would not have the lipid content necessary for significant methyl-Hg concentration. It is more likely that initial methyl-Hg entry into the food chain is bioconcentration within benthic and epibenthic invertebrates. Then the biomagnification process would ensue as those invertebrates were consumed by primary consumers.

9) p. 2B-12: The role of nutrients and biodilution in methyl-Hg cycling within the Everglades is a tenuous hypothesis and maybe should not be posed in this report.

   There is a hypothesis that reduced primary productivity will exacerbate "the mercury problem" (1st sentence of 1st para in section entitled "role of carbon cycling in methylmercury…" on page 2B-12). There needs to be a citation presenting supporting data for this hypothesis, if one exists.

   It appears the basis for this hypothesis is biodilution. Biodilution is a phenomenon that has been demonstrated in organisms in which there is no net chemical uptake in conjunction with organism biomass increase. Unless there is a decrease in net

methyl-Hg uptake with increased plant biomass, biodilution will not occur. Is there a citation demonstrating biodilution of methyl-Hg in plants with growth? If biodilution does not occur, rather net uptake is constant, Hg mobilization into the ecosystem may increase, given the *ECR*'s assumption that plant uptake is a significant contributor to Hg cycling within the ecosystem. In fact, the lack of presented data related to the relative contribution of plant absorbed methyl-Hg to methyl-Hg concentrations in fish and higher trophic level organisms is another weakness of the hypothesis.

Given the issues with the above hypothesis, its extension to a possible role for phosphorous in methyl-Hg cycling within the Everglades is even more tenuous. While phosphorous is a factor in primary productivity, the likely complex role primary productivity has on methyl-Hg cycling in the Everglades is insufficiently presented in the report.

Chapter 2C

1) General: A systematic analysis of TP and TN trends at individual long-term monitoring sites in each area would add a lot to this chapter (i.e., Seasonal Kendall test or the like). Detecting changes is a major management concern. This should be made a routine component of the ERC, which has focused on each year separately and provides little historical perspective.

2) p. 2C-3, Methods: The use of ½ of the MDL is frequently used for analysis of datasets that contain observations that are less than detection (in statistics this is termed "censored" data). For datasets with most samples considerably above the MDL, this approach adds very little error. For sites with total phosphorus means near the MDL, considerable estimate error may result from the assumption of ½ MDL. Park interior site EP, for example, has a geometric mean (Appendix 2C-2) of 2.4 µg/L and arithmetic average of 2.5 µg/L. Other approaches have been intensively researched and are available (Ahn 1988; Berthouex and D. Robert 1993; Gilliom et al. 1984; Malcolm and P 1994; She 1997; Travis and Miriam L 1990). One simple approach is to calculate the geometric mean using a value of zero, ½ MDL, the MDL in three alternative calculations. The variation of the statistical results among the three assumptions quantifies the uncertainty caused by the censoring of the observations. In the case of the geometric mean, however, the zero assumption cannot be used because the geometric mean of any set of number containing a zero is zero. In this case, only the ½ MDL and full MDL assumptions can be evaluated and used to estimate uncertainty caused by the assumption.

   Further difficulty with data analysis will result from improved chemical procedures lowering the MDL over time. These changes could anomalously result in an observed temporal trend that was related only to laboratory improvements.

Ahn, H. (1988). Estimating the mean and variance of censored phosphorus concentrations in Florida rainfall. Journal of the American Water Resources Association, 34(3), 583-593.

Berthouex, P. M., and D. Robert, G. A. N. (1993). A Model of Measurement Precision At Low Concentrations. Water Environment Research, 65, 759-763.

Gilliom, R. J., Hirsch, R. M., and Gilroy, E. J. (1984). Effect of censoring trace-level water-quality data on trend-detection. Environmental Science and Technology, 18(7), 530-535.
Malcolm, J. R. C., and P, H. W. (1994). Conflicting Perspectives About Detection Limits and About Censoring of Environmental Data. Water Resources Bulletin, AWRA, 30(6), 1063-1079.
She, N. (1997). Analyzing censored water quality data using a non-parametric approach. Journal of the American Water Resources Association, 33(3), 615-624.
Travis, C. C., and Miriam L, L. (1990). Estimating the Mean of Data Sets With Nondetectable Values. Environmental Science and Technology, 24(7), 961-962.

3) p. 2C-5, 3rd para under Status: Phrase "are unrestricted by experimental design" is misleading. Most of the experiments were not designed to determine the actual phosphorus concentration at which imbalance occurs, but to determine cause-and-effect relationships. "Unrestricted by experimental design…" does not make much sense and should be deleted. Instead, discuss the strengths and weaknesses related to collecting information from the field (i.e., transect studies) versus collecting data from experiments in the field or laboratory (i.e., dosing studies). The relative strengths and weaknesses were what lead Lean et al. (1992. Everglades Nutrient Threshold Research Plan. Research and Monitoring Subcommittees of the Everglades Technical Oversight Committee) to select a suite of approaches, recognizing that no one design is perfect. Collecting transect data assures the greatest level of realism, but the lowest level of control. And, it is not possible to ascertain cause-and-effect relationships from these types of data. Experiments, even in the field, offer lower levels of realism but greater levels of control. Experimental design strives to keep all factors constant except for the one that is the subject of study. This design supports the development of cause-and-effect relationships.

4) p. 2C-5, last sentence: How does this finding from FDEP's analyses compare with results from experimental studies? Did short-term periods of TP concentrations above 10 ppb have any effect on biology in District's dosing studies, or in FIU's flume studies?

5) p. 2C-6, 1st sentence: FDEP certainly is to be commended for conducting extensive and high quality analyses in support of rulemaking. However, credit also should be given in this sentence to all of the other entities that developed the data that FDEP could analyze.

6) p. 2C-6, after 2nd sentence: For fairness and balance, it would be appropriate at this point to describe the moderating provisions. It is true that the ERC-approved criterion is 10 ppb, but it is also true (and quite significant) that numeric compliance with this criterion does not have to be met for discharges into impacted areas. These moderating provisions are authorized until 2016.

7) p. 2C-6, 1st para, next-to-last sentence: If the default criterion comes into play after December 31, 2003, how will it be measured?

8) p. 2C-6, middle of 2nd para: Mention that moderating provisions are authorized through 2016.

9) p. 2C-6: Why is there no mention of the amendments to the EFA? These amendments are as significant as the rule, and should be a part of the text.

10) p. 2C-8, 3rd para, 1st sentence: This sentence indicates that some of the WY2003 TP concentrations were "slightly above" the WY2002 concentrations. On the contrary, Table 2C-1 shows that the geometric means for the inflows to the refuge went from 33.4 to 48.4 ppb, and that the median concentration went from 27 to 46 ppb during that one-year period. The text on p. 2C-8 focuses on a very small improvement in the refuge marsh interior geometric means of 0.3 ppb over the same one-year period, yet no discussion is presented about these much more significant increases in inflow concentrations. Also, there were smaller TP concentration increases in the inflow to WCA-3.

11) p. 2C-8, last sentences of 3rd and 4th paras, 5th para, 3rd-from-last sentence: These sentences ascribe changes in TP concentrations to temporary natural phenomena, changes in water management practices, changes in water level, or general improvements in marsh nutrient conditions. In discussions of exceedances in the refuge, the TOC clearly decided that there was strong disagreement over causation, and that existing data provided limited ability to determine cause-and-effect relationships. The TOC also decided unanimously on a list of recommendations to make to the Consent Decree Principals that the TOC believed would result in fewer refuge exceedances in the future. Part of this list dealt with increasing the amount and quality of data needed to determine cause-and-effect relationships should exceedances occur again in the future. In light of the disagreement regarding cause-and-effect relationships, and the desire to improve the future scientific foundation, it is recommended that the chapter authors not suggest only one side of possible explanations of TP concentration changes.

12) p. 2C-8, 4th para: The paragraph states that TP ranged from less than 4.0 to 49.7 µg/L. Does this refer to the geometric means reported in Appendix 2C-2, or to the individual values? It would be helpful if the site name could be added in parentheses following each number.

13) p. 2C-10, Table 1: The statistical summary comparing TP values for different intervals (1978-2001, 2002, 2003) may be misleading because of changes in the sampling program designs. If the intent is to show trends, the table should be trimmed to reflect the same set of sites for each interval. ENP Marsh monitoring started in 1985 (not 1978).

14) p. 2C-12, Fig. 2C-2: It would also be useful to split the 10-50 ppb interval into two intervals (10-20 and 20-50 ppb). Or use 10-15 and 15-50 as in Fig. 2C-1. This provides better indication of sites in transition.

15) p. 2C-12, Fig. 2C-2: It is interesting to note that the "Rim" canal stations in the refuge are more frequently above 50 µg/L than the "Inflow" sites. This suggests that

phosphorus is being internally loaded from canal sediments at times when inflow loading is reduced.

16) p. 2C-13: It would be of interest to include a summary of nitrogen concentrations similar to the phosphorous statistics presented in Appendix 2C-2.

Chapter 3

1) p. 3-9: If permit-level data are able to use for determining Everglades Agricultural Privilege Tax, then these data should be presented in the *ECR* despite the comments that these data are not applicable here (lots of data are presented as ancillary data).

2) p. 3-15: The Three-Year Average Total Phosphorus Load % Reduction does not match up with the three year's worth of data presented in this table (5^{th} row). Please check to make sure there isn't a rounding error.

3) p. 3-16, Table 3-5. Define LOK SWIM.

4) p. 3-21, Table 3-6 : Is it correct that the actual WY2003 TP concentration for the C-139 is 23% higher than "base"? If so, please add a discussion here.

5) p. 3-27, Fig. 3-11 and p. 3-28, Fig. 3-12: If the data are available to define several farms as > 10 lbs/acre, then it is possible to present (graphically) the location of potential "hot spots" to the reader.

Chapter 4A

1) General: This is a very important chapter, summarizing important information from the year of continued STA experience.

2) General: Information on the individual STAs seems inconsistent. Some present average hydraulic loading (cm/day), and some do not. Flow weighted mean concentration tables (e.g., Table 4A-4) do not consistently present the same constituents. Why, for example, are chloride and sodium not reported in Table 4A-10 but are reported in Table 4A-13? Does this represent a difference in constituents monitored at these inflows and outfalls?

3) Statistical summaries of STA data – appendices – general: The summaries for the current year are informative. Presentation of the cumulative record and discussion of trends in performance would be much more useful.

4) Those STAs that experienced overloading in WY2003 should be briefly discussed in the "Summary" as it is a notable component of the text of this chapter.

5) General: The discussion of water quality at sites downstream of STA discharges is limited to dissolved oxygen and mercury. This should be amplified to include nutrients and other relevant water quality parameters.

6) General: The report indicates that inflows to STA-1W, STA-2, and STA-5 exceeded design capacities. What measures are being taken to bring the loads down to the design levels? There is limited discussion of this in STA-1W (lake releases), but this would not address excessive runoff.

7) General: Information on bypass loads for each STA should be presented. Reasons and remedies for bypass should be discussed. Table 4A-1 should be expanded to include a row showing bypass loads and a row showing total load to WCA (bypass + STA outflow). Bypass loads to Table 4A-1 and to the input/output figures for each STA (4A-4, 4A-11, 4A-19, 4A-30).

8) p. 4A-1: Provide information in the Summary section about the STA inflows that exceeded design capacities.

9) p. 4A-2, Table 4A-1: Present with information on design specs, so the reader knows what this translates to (overloading, within design parameters, etc.).

10) p. 4A- 2, Table 4A-1: It would assist the reader in comparing STAs if values were also provided as area specific (per square meter) values. Area information is provided, and the reader can easily perform these calculations. However, it would make the comparison more straightforward to provide this for the reader.

11) p. 4A-3 Table 4A-2: Table 4A-2 includes information on STA-1W bypass flows and loads. This information is a valuable addition to the *ECR*, and this addition by the authors is noted and appreciated.

12) p. 4A-4: List the amount of water going to STA-1E from Acme.

13) p. 4A-4: When is the G-311 structure planned on being on-line?

14) p. 4A-46, Fig. 4A-26 shows that a large portion of the phosphorus load from STA-5 is being retained in Rotenberger. There is no discussion of the impacts of these loads, despite the extensive monitoring of water quality, soils, and vegetation being performed downstream of the STA-5 discharge.

15) p. 4A-13, Fig. 4A-7: The caption of Fig. 4A-7 includes a parenthetical statement "Note: the Refuge also includes the Snail Farm and Strazzulla properties." The refuge, in fact, also includes additional property including impoundments, a headquarters area, and cypress swamp. Perhaps the statement could be revised to read "Note: the refuge includes some land outside the WCA-1 boundary." The label should be "A.R.M. Loxahatchee National Wildlife Refuge (WCA-1)".

16) p. 4A-12: The x, y and z transects are miles from the STA-1W discharge, and are nearly totally irrelevant to a discussion of DO in the STA-1W discharge. Those transects are relevant to penetration of water and contaminants into the refuge in that vicinity.

17) p. 4A-17, 1st para: There is no direct (1:1) linkage between STA-1W discharge and the x, y, z transects to make this DO conclusion.

18) See Comment #1 in Chapter 4B below.

Chapter 4B

1) General: There is a general lack of connection between Chapters 4A, 4B and 8A. Chapter 4A discusses STA performance, but with no interpretation of whether design expectations are being met. FDEP has in the past suggested doing such an interpretation based on a design model. The flows and loads for the year would be run through the design model to see if expectations are being met. At least two models are available: the STA design model and its successor, DMSTA. As a consequence, there is no way to tell if the STAs are below, at, or above design performance.

   Chapter 4B discusses results from a loosely connected set of experiments and platforms. However, the report contains no quantitative interpretation of those results in terms of STA performance projections.

   As a consequence, based on information in this *ECR*, the activities described in 4B have exerted no influence on decisions on STA optimization. Chapter 8A presents the outline of the Long Term Plan, but neither the LTP nor Chapter 8A describe any usage of results from Chapter 4B.

   One of the most important things that needs to be communicated to the reader is that a decision has been taken to configure the STAs as a sequence of emergent vegetation followed by SAV alone or possibly followed by SAV followed by PSTA. It would seem useful to include this in the Executive Summary and Introduction.

2) General: It would be valuable to the reader to have maps of the STAs, including their structures. This chapter, as all the others, should function as a stand-alone document.

3) General: There are places within Chapter 4B where some places are high in detail, whereas others are lacking in detail (or supporting documentation), such as p. 4B-14, 3rd para.

4) General: This is a very important chapter, summarizing new studies and findings from research, modeling, and operational experience over the water year. Because of its importance, efforts to expand the detail of reporting and analysis within this chapter are well justified.

5) General: A large part of the information presented here has been placed in appendices. It would help the reader if these appendices were listed and summarized in the introduction section. The authors should then attempt to place the chapter information and appended information into a context of optimization objectives, data needs, and process research needs.

6) General: Throughout the report (for example see the last paragraph on p. 4B-5) references to a year or years (such as 2002) should explicitly state calendar year or water year.

7) p. 4B-1, 2nd para (see also p. 4B-13): The lack of correlation of outlet TP with inlet TP loading is an artifact of univariate analysis of bivariate (or multivariate) behavior. Outlet TP depends on at least the two primary variables of inlet TP concentration and hydraulic loading. The lack of correlation with the product of these two is not only not surprising, it is a predicted result from existing models. For a visualization of this effect, see Appendix 4B-11 (Kadlec and Walker, 2003. Draft Technology Review of Periphyton Stormwater Treatment) in this Report. It is recommended that these single variable statistical analyses be dropped from the report, because they are badly misleading.

8) p. 4B-1, 3rd para: Poor performance of some of the STAs likely resulted from factors other than vegetation management. Time sequences indicate poor performance prior to vegetation management in some instances. It seems fairly obvious that huge overloads had a great deal to do with poor performance in STA-1W.

9) p. 4B-3, STA Optimization Monitoring: As stated in this chapter, past *ECR*s have provided annual water and total phosphorus budgets for the STA treatment cells. This was a valuable part of the report that provided insight not only into STA performance, but also helps to evaluate data quality and future data needs. It was therefore disappointing to find that this analysis was not included in the draft *2004 ECR*. In previous comments it has been suggested that mass balances should be extended to other constituents. At a minimum this should include chloride and total nitrogen. For discharges to the refuge, it would also be of value to see such an analysis for calcium and alkalinity. It is recommended that these balances be incorporated in next year's *ECR*, and that previous mass balances for all previous years be included in appendices of that report.

10) p. 4B-3: The spatial scale of sampling at a 16 hectare size is more of a uniform sampling distribution than a "grid" sampling, which implies a spatial scale with direct linkages among grid cells.

11) p. 4B-6, Submerged Aquatic Vegetation: Where, how, replicates? Coontail is incorrectly implied to be a rooted member of the SAV community .

12) p. 4B-7: "Despite differences in the vegetation community…". What were they?

13) p. 4B-8, Treatment performance: What is the timing of the "correspondence" between the timing of herbicide application and spikes in weekly TP outflow?

14) p. 4B-9: In WY 2003, it was learned that TP removal performance of *Hydrilla* was poorer than that of native, rooted SAV. Discuss here.

15) p. 4B-11, Table 4B-5: n = ?

16) p. 4B-13, Fig. 4B-2: It appears that the regressions have better fits if the STA 5 data were removed. Please discuss.

17) p. 4B-16, 4B-18: The scales on these graphs obfuscate any interpretation of relative performance.

18) p. 4B-17, South Test Cells: What were the species of SAV?

19) p. 4B-19: Provide more detail as to the parallels between Lake Panasoffkee and the Everglades? E.g., Is Lake P- characterized by peat sediments?

20) p. 4B-19/20: "… the long-term stability of this storage compartment was difficult to assess because most of the P was stored in the upper 10 cm of sediment…" Didn't you just assess the long-term stability by the subsequent comment that, the sediment is still, "subject to diagenesis and release back into the water column"?

21) p. 4B-20, Florida Lake and River Study: Not enough detail present for the reader to know the parallels to the Everglades. Sand or peat based sediments? Differences among SAV species? Is there really an average 30-year period of record data on actual SAV community composition/distribution data among the sites examined?

22) p. 4B-26. 2nd para: "This experiment failed to demonstrate that PACL …. were effective at eliminating P flux from the sediment to the water column." This statement implies that PACL might be effective, and that the design was the problem. Is this actually the case, or was the design sufficient to change opinion as to the potential for this theory to be true?

Chapter 5

1) General: By providing information on rainfall, flows, ET for the WCA's and ENP, this chapter covers a topic of great importance to the health and restoration of the Everglades. In future years, the scope of this chapter should be expanded and the text expanded.

2) p. 5-1, last para: Does rain not count as an "inflow"?

3) p. 5-3, Fig. 1: Names in figure are displaced.

4) p. 5-5, the sentence "Droughts are characterized by a significant decline in annual rainfall.": It would be preferred to use a more quantitative definition. Frederick and Ogden (Frederick and Ogden, 2001. Pulsed breeding of long-legged wading birds and the importance of infrequent severe drought conditions in the Florida Everglades. Wetlands, 21(4), 484-491), for example, defined drought years as those where "stages <1 standard deviation below the mean." The Palmer Drought Severity Index may be less well suited to defining drought. The citation, "Abtew and Huebner, 2002," should be replaced by "Abtew et al. 2002" because there are three authors on this report.

5) p. 5-8, Fig. 5-6 and others: It is unclear which site or sites are being averaged in Fig. 5-6 and other figures. Site names should be listed in the figure caption or in the body of the text. Also, what is the source for the "station elevation" shown in the figures?

6) p. 5-8, Fig. 5-6 and others: It might be nice to plot the last couple of years' worth of data on these graphs to give the reader a better perspective of what's going on, especially in light of using 3-year averages for other sections of this chapter.

7) p. 5-11: Was there inflow from the S-6 diversion gate entering into the refuge? If not, this should be stated.

8) p. 5-11: It would be informative to present average period-of-record flows along with current year values.

9) p. 5-15, Conclusion: Rainfall can be better quantified by return frequency from the historic period of record.

10) Appendix 5-1: Outflows in Table 2 of Appendix 5-1 do not include G-94A and G-94B. Even if there was no flow for the year, this should be noted.

Chapter 6

1) General: This chapter is an important component of the *ECR* and contains much information that is relevant to restoration, particularly CERP. However, as in last year's report, this chapter is a loosely organized compendium of sections that are written in different styles and that contain different levels of detail. The chapter would benefit greatly from an editing job (There are differences in format and content. The chapter would be easier to follow if all sections were of similar structure and content.), and from a synthesis section that suggests what the management implications are of the results presented. The management implications are very important to elucidate, and the authors should present possible implications wherever possible. The summary would be more useful if it was a synthesis of the needs, objectives, and results in a synthesized form, rather than a cut and past of the individual section summaries. Also, citations are needed in some sections to support statements that are made.

2) General: The first paragraph of the summary states that the "Programs of study were based on the short-term and long-term needs of the South Florida Water Management District operations, regulations, permitting, environmental monitoring, Everglades Forever Act mandates, and the Comprehensive Everglades Restoration Plan (CERP)" but there is not a consistent description of the need or tie-back to these needs in the individual sections.

3) General: Each section should have a clearly stated objective and relationship to District needs.

4) General: The authors do not take full advantage of the opportunity to really show how their work can be applied.

5) General: Many table and figure captions could be improved.

6) p. 6-1, 2nd para,1st sentence: Change "Florida Conservation Commission" to Florida Fish :and Wildlife Conservation Commission.

7) p. 6-1, 2nd para: USCOE and University of Florida also participate in the wading bird monitoring efforts.

8) p. 6-1, 2nd para, last line: ...ground surveillance system used to document ~~nighttime~~ *faunal* activity...

9) p. 6-2, 2nd full para (starts "In 2002, permanent vegetation plots…"): Are the vegetation studies in "WCA-3" being conducted both in WCA-3A and WCA-3B? WCA-3B has been held as an example of an area that has been degraded by lack of experiencing flow-though conditions, to the point that the ridge and slough pattern has been very much degraded. If some of the tree islands are in WCA-3A and some in WCA-3B, might the differences in water management between the two confound the results? Also, are not the data on species diversity and richness from Heisler et. al. 2002? If so, this publication should be cited; if not, the other citation should be provided to clarify which study is the basis for these statements.

10) p. 6-2, 2nd full para: In this paragraph and other parts of the text, tree islands are referred to by a combination of letters and numbers (e.g., 3BS2 – presumably the second of an unknown number of tree island in southern WCA-3B? - in this paragraph). A map showing where these tree islands are located would be enormously helpful for anyone reviewing this and subsequent portions of the text.

11) p. 6-2, last para: In this paragraph and others throughout the document, Old World climbing fern (common name) is referred to simply as "Lygodium" (genus name, but not in italics) This stands in contrast to the treatment of other plant species, for which a common name is provided and a Latin genus and species epithet are given. Suggest using the common name given in this comment, followed by the genus/species (*Lygodium microphyllum*) in italics, unless it is possible that the text is also referring

to another species of invasive climbing fern, *L. japonica*, which is more common in northern Florida.

12) p. 6-2, last sentence: Strike "a significant amount of cloud cover". Replace with "imagery interference from cloud cover".

13) p. 6-3, 1st para: Define "heuristic".

14) p. 6-3, 2nd para under Introduction: It seems that, given the discussion of the role of flow later in the chapter, that flow should be included as part of the text on source, timing, duration, and depth.

15) p. 6-3, Introduction, 2nd para, last two lines: What does this mean?

16) p. 6-3, 2nd para under Introduction: Correct reference would be to the amended EFA, 2003.

17) p. 6-3, 3rd para, line 16: Define "The direction taken in the past...".

18) p. 6-3, last para, starting with, "This chapter should be viewed": would fit well in the summary as the first part of a paragraph that is followed by the major points for each section.

19) p. 6-4, Monitoring, modeling, and assessment: This section seems out of place and doesn't really add much. Perhaps it could be used to explain better how this information supports District efforts.

20) p. 6-4, Wildlife Ecology: Not sure what the point of the first paragraph is.

21) p. 6-4, last line: van der Valk, not Valk.

22) p. 6-5, Wading Bird Monitoring: Discuss levels of uncertainty associated with bird count estimates.

23) p. 6-5, Wildlife Ecology: Wading bird monitoring section can do a better job of discussing how wading bird data are used to make operational decisions.

24) p. 6-6, Table 6-1: What is the base condition?

25) p. 6-6, Food Web Studies, Past and Present: It is not clear why this information is important for water managers.

26) p. 6-6: The section on food webs is a good one, and is particularly important to a greater understanding of what factors control Everglades plants and animals, with large implications for CERP and for water quality restoration issues.

27) p. 6-6, 2nd para: Explain better the use of stable isotopes. It does not tell the reader why and how stable isotope analysis is a good tool in food web studies. If this discussion is going to be kept, the section needs to make it clearer how selection for one isotope occurs in food web processes.

28) p. 6-7, 1st para: It would be helpful to have a reference to which studies of mercury biomagification are being cited.

29) p. 6-7, 2nd full para (starts "A major uncertainty…"): It would be helpful to include a brief statement as to why ostracods, etc., "are not good studies for the Everglades." Similarly, it would be helpful if the concept in the next sentence were to be expanded to explain why the food webs need to be studied at the local level, particularly if this document is to be used to support an effort to do so. While specialists may understand the reason, not all reviewers may do so in the context of the Everglades micro-landscape scale.

30) p. 6-7, last sentence of 2nd para: This statement seems to conflict with statements made on next page (see 3rd sentence, 2nd para next page).

31) p. 6-7, 3rd para, sentence beginning with "The diets, distribution, and abundance…": Not sure what this means. Why are these not "good studies" for the Everglades?

32) p. 6-7, last para, 1st sentence: Should be "fishes", not "fish". The use of "fish" refers to one species, while "fishes" refers to multiple species.

33) p. 6-8, 1st para: More citations needed.

34) p. 6-8, first para: It is stated that "the sailfin mollies and flagfish are considered herbivores, whereas the least killifish and eastern mosquitofish are considered omnivores…" Is it worth providing a reference as to who considers these to be one or the other, or is this common knowledge to the point that this should simply be stated as fact?

35) p. 6-8, 1st para: Sentence citing Browder et al. incomplete. Preferential consumption of diatoms and green algae by what? It is not clear if last two sentences are referring to Browder et al. If not, need additional citations.

36) p. 6-8, 2nd para, 3rd sentence: If Browder et al. 1991 is the only study available on this topic, the sentence is not well supported. It is agreed that the Everglades food web is poorly understood, but it is not clear that this selection actually takes place.

37) p. 6-9, Fig. 6-1C and D: Define VP-D.

38) p. 6-9, Fig. 6-1: These data do not support the assertion in the caption that amorphous detritus contributes to the majority of production. Detritus certainly seems to be more abundant in guts than other food types, but because food quality of various food items

was not determined, the statement is a stretch. A small amount of high-quality animal parts in the gut may be responsible for most of the production.

39) p. 6-9: Section on camera technique interesting, and probably will produce very useful information. However, because very few data have been collected, this topic may not warrant inclusion in the report.

40) p. 6-9, Camera Trapping: Also species lists for tree islands in the "Tree Island Book" (Sklar, F. and van der Valk, A. (Eds) 2002. Tree islands of the Greater Everglades. Kluwer Academic Publishers. Dortrecht, The Netherlands. 541pp.) and Brandt et al. (2003. Wildlife and vegetation of bayhead islands in the A.R.M. Loxahatchee National Wildlife Refuge. Southeastern Naturalist. 2(2): 179-194).

41) p. 6-9, Camera Trapping: Would be interesting to have a list of species, "captured" not just the turtles.

42) p. 6-10, 1st full para (starts "There are no known methods for trapping the large aquatic turtles…"): Given historical photographs of Tom Shirley with large quantities of snakes caught in what is now the WCAs, what challenges or opportunities might there be to expanding this investigation to include snakes that depend on tree islands?

43) p. 6-10, last para: 25 faunal species were captured on film, yet only 5 are discussed. What about the other species?

44) p. 6-11, Table 6-2: What data or analysis of these data are available at this time? If neither the data nor the analysis are ready, when might they be?

45) Pp. 6-11 through 6-14: This section provides no or few references, in comparison to other sections of this chapter. If the analysis is a work in progress, then it would benefit this chapter to refer to it as such.

46) p. 6-12, top: The difficulty of reconciling three different missions not necessarily a time scale issue. It may be simply that the three missions are incompatible.

47) p. 6-12: Section on Rotenberger restoration weaker than rest of chapter. In particular, significant conclusions are drawn that are not supported by the data presented. See additional comments below.

48) p. 6-12, Restoration of Rotenberger: What was the goal of the restoration? Change to domination by obligate wetland plants? Was the loss of facultative species "good"? p. 6-13 it is not clear if the increase in the obligate species was "good" or "bad" because of their association with high nutrients. There are two issues here that seem to need better separation. What effects were from hydrology and were they good? What were from water quality and were they good? What are the implications for long-term? Since the issue of hydropattern restoration with "dirty or clean" water it is critical that this section clearly present which effects are which and discusses the long-term implications.

49) p. 6-13, Fig. 6-3: Not needed. Two data points more easily presented in text.

50) p. 6-13, last para: It would help to have citations supporting statements about obligate plant species and their response to nutrient status.

51) p. 6-13, last para (starts with "While long-term changes…"): "Typha domingensis" should be italicized.

52) p. 6-14, top: The persistence of nutrient-tolerant wetland plants is also indicative of water column concentrations of TP greater than 10 ppb!

53) p. 6-14, Fig. 6-4: Needs error bars. Also, a listing of the species in a legend would be beneficial.

54) p. 6-14, 2nd para: The second sentence needs to be rethought. If TP concentrations are greater in the inflow, increases in volume and velocity would not result in lower TP in the marsh unless there was a secondary source of dilution water. Also, the use of the work "effluent" is inappropriate. A better phrase might be "inflow to the marsh." The next sentence is a reach, given the presented data. It certainly is possible that plant uptake has an effect, but there are no data showing that plants are taking up "most of the phosphorus." Need to see the data supporting the last sentence in the paragraph, or a citation to the report/publication. Overall, the data seem to support the possibility that the new inflows are creating a more nutrient-enriched marsh than what existed previously. This apparent result has important implications, especially regarding the "hydropattern restoration" provisions of the amended EFA and the phosphorus rule (presently under challenge).

55) p. 6-14, 3rd para: The 1st sentence is a major problem. It is not at all clear that increases in water level and hydroperiod have been beneficial to Rotenberger, even in the short term. The data presented illustrate an increase in the presence of nutrient-tolerant wetland plants, no decrease in soil TP concentrations despite lower surface water TP concentrations, and striking increases in plant tissue nutrient content. Also, there is no indication that lower TP concentrations in future inflows will facilitate the shift of current vegetation to "more desirable wetland species." The most recent WCA-2A cattail map suggests that in areas where cattail have decreased, lowering of the water level and subsequent fires may be necessary.

56) p. 6-14, 3rd para: No data presented on porewater quality (1st sentence).

57) p. 6-15, Table 6-3: Much more information is needed to be able to evaluate the data in this table. Most importantly, how many samples were taken, and what were the locations of the samples? This is basic information that should be provided in order for the reader to evaluate the conclusions.

58) p. 6-15: Section on belowground biomass has an organization very different from other sections within the chapter, and provides a good example of the lack of consistency. In fact, the organization of this section into introduction, methods, results, and discussion is probably the best format for the rest of the chapter. However, even with better organization, this topic seems to be one of the least relevant to restoration efforts. While a greater understanding of belowground biomass dynamics would contribute to greater ecosystem understanding, it seems that there must be other, more important topics to be pursued to provide the scientific foundation for restoration.

59) p. 6-15, 1st para: More information is needed as to how hydrology is directly related to the creation of organic matter.

60) p. 6-15, 2nd para, 1st sentence: The most important factor related to fine roots is that they provide a tremendous amount of surface area relative to their biomass. This huge surface area is essential for material exchange between the soil environment and the plant.

61) p. 6-15, 3rd para: Relative the previous comment, more information should be provided as to the need for, and significance of, determination of fine root biomass.

62) p. 6-15, last para: 3AS2 seems to experience no hydroperiod (0 months!), not a "short" hydroperiod.

63) Pp. 6-16 and 6-17: It appears that only three tree islands have been studied with respect to belowground biomass. Given that only three tree islands apparently have been studies (how many is not clear, which again is why a figure showing the locations and identification of all relevant tree islands would be so useful) how robust is this conclusion, given that only three tree islands apparently have been studied? Are there plans to expand this study?

64) p. 6-17: The three islands sampled have BOTH different hydropatterns and different species yet the conclusions/discussions focus on the former with only a brief mention of the later. Don't the tree species have different growth forms that might significantly effect the patterns?

65) p. 6-17, last para (starts "However, hydrology is not the only factor…"): How is the tree island's "near tail" defined?

66) p. 6-17, last para: The largest problem with this section is the conclusion that hydrology influences the aboveground and belowground biomass allocation. This conclusion completely ignores the fact that there is an entirely different plant species composition on each of the tree islands. In the face of this difference, how is it possible to separate out the potential effects of hydrology? This section, and other sections, should be reviewed to reword conclusions to a form that are supported by the data.

67) p. 6-19, 2nd para: Needs references.

68) p. 6-22, Tree island vegetation ecology: 1st para does a good job of setting the context for this work. There is high relevance to restoration; good discussions as to the effects of hydrology.

69) p. 6-22, 2nd para, 2nd sentence: Suggest changing "relating forest structure to current hydrologic conditions" to "relating forest structure to historic hydrologic conditions." Forest structure reflects historic hydrologic conditions rather than just current conditions.

70) p. 6-22, 3rd para, 3rd sentence: missing ">" in front of 2.5cm.

71) p. 6-23, 1st para: It is not clear whether "wet" versus "flooded" refers to islands or plots on the islands. Table 6-4 shows that wet and flooded plots can occur on the same island. Why was six months chosen as the break point for "wet" and "flooded" islands?

72) p. 6-23, 2nd para, 2nd sentence: To improve clarity, replace "the water table generally falls below the soil surface" with "the water table is below the soil surface."

73) p. 6-24, 1st para: Perhaps could add some supporting detail for why these islands are characterized as having "natural hydrologic gradients."

74) p. 6-24, 1st para, 2nd sentence: Not clear that species diversity is lowest on tree islands with hydrologic extremes. Which part of Table 6-4 supports this statement?

75) p. 6-20, Table 6-4 caption: "Negative values indicate that the water level falls below soil surface." Doesn't the water level fall below the soil surface for all of the islands? Perhaps replace the sentence with: "negative values indicate that the water level is, on average, below the soil surface."

76) p. 6-24, Table 6-4: Does 3BS1 have a head?

77) p. 6-24, last para: How is CI calculated? Has it been used elsewhere? Please provide more details on how the CI is calculated.

78) p. 6-25, 1st para: Spell out genus names first time they appear. Also, a one-time listing of common names would be helpful for the "botanically challenged" of us.

79) p. 6-25, Belowground biomass: need a better explanation of why below ground biomass is important in terms the audience will understand. How does this relate to District activities and mandates?

80) p. 6-25: This section is in a different format than the others.

81) p. 6-25: It seems counterintuitive that greater basal area and high stem density coincide. Do mature forests have higher stem densities?

82) p. 6-27, Fig. 6-13: spell out scientific names in caption.

83) p. 6-28, Landscape Ecology: 1st para is awkward, it goes into detail on IKONOS project, but not others. It needs a better statement of relationship to District needs.

84) p. 6-28, Landscape Ecology: Are there any plans to investigate possible methods for conducting a "preemptive strike" on areas where the Old World climbing fern is colonizing? It is showing up in other WCAs, too, although reportedly not yet to the level that it has in WCA-1.

85) p. 6-28, 1st complete para: The reference to Fig. 6-3 should be Fig. 6-13.

86) p. 6-28, Tree island change: good tie back to management of water in WCAs.

87) p. 6-29, Tree Island change, 3rd para: Tree islands can be categorized as either floating or fixed varieties. Change to: Tree islands in WCA-3 can be categorized as either floating or fixed. Does the term fixed in this context mean all islands that are no longer just pop ups or just islands believed to be on limestone outcrops. There may be some confusion with the term fixed since in some context it is used to mean only the latter. Tree islands in the refuge may start out as floating, but most are not firmly "fixed".

88) p. 6-31, Fig. 6-14: Please clarify why the data points on Fig. 6-14 don't correspond with the data in Table 6-6. Text in the last paragraph of p. 6-29 indicates that Fig. 6-14 illustrates data for polygons, but the polygon data in Table 6-6 is different. Is Fig. 6-14 dealing with polygons or islands?

89) p. 6-32: As in last year's report, there still is not mention or citation of the SCT flow paper (Science Coordination Team. 2003. The role of flow in the Everglades ridge and slough landscape. Report to the South Florida Ecosystem Restoration Task Force Working Group). This paper was produced by the SCT, was externally peer-reviewed, and was authored by numerous scientists from many agencies and entities. That paper is one of several factors that led to an increased emphasis on flow-related research, and should be cited out of fairness to the 20-plus individuals (including District staff) that worked on it.

90) p. 6-32: It is not clear how the 18 quadrants were selected, or where they are located.

91) p. 6-32, Tree Island change: These are interesting analyses. What would be beneficial to a more general audience is to translate these landscape metrics into simpler terms to really explain the significance.

92) p. 6-33, 1st para, Spatial patterns in the ridge and slough: p. 6-29 first paragraph Within the quadrats awkward paragraph.

93) p. 6-33, 2nd para: Where are the PAN data presented?

94) p. 6-33, last para: ALS and AWS data not shown, so this text could be deleted.

95) p. 6-34, Fig. 6-17: Figure legend unreadable.

96) p. 6-34: Section on IKONOS data also a good section; very relevant to restoration. Good tie-back to need.

97) p. 6-35, 3rd para, 1st sentence: Should read "…classification procedures did NOT work well…?"

98) p. 6-35, 3rd para: Any numeric analyses of comparison between IKONOS data and ground-truth data?

99) p. 6-35, 3rd para, end: Reference to Fig. 6-20 must be incorrect. Fig. 6-21?

100) p. 6-40, 1st para: Actual citation to 1999 issue would be better.

101) p. 6-40, 5th para: Even though the overall rate of cattail expansion may be slowing, the most troublesome feature illustrated in the new map is the increase in "spots" of cattail in interior portions of WCA-2A that have always been considered relatively pristine. It would be good to include some discussion of the potential implications of these new areas.

102) p. 6-42, Fig. 6-23: Color-coded key is missing.

103) p. 6-46, Wading Bird Studies: This study looks like it will provide welcome data to refine current concepts about foraging of wading birds. Are there any plans to expand it beyond fishes (e.g., to the two species of crayfish, *Procambarus alleni* and *P. fallax*) as prey base(s)?

104) p. 6-46, 2nd para: This paragraph seems a little outdated.

105) p. 6-46 and 6-47: Again, don't see the usefulness of text on proposed or future data collection efforts. These sound like great things to do, but no need to include at this point until data and analyses are available.

Chapter 7

1) General Comment: This document is silent on the role of what is currently referred to as the Operations Planning Team. Since adjusting operations is key to adaptive management, the current thinking on the integration of this subteam, even if it is not

constituted according to current thought, should be incorporated as a basis for future considerations under RECOVER, even if current thought is in its infancy.

2) p. 7-2, 2nd full para (starts "RECOVER is developing an adaptive management program…"): Should not expected responses, as well as unexpected ones, be explicitly noted to bolster the underlying planning hypotheses? It seems that both forms of feedback should be important in applying adaptive management.

3) p. 7-2, 3rd full para (starts "A total system…"): It should be noted that while much has been accomplished to improve previous drafts, this conceptual ecological model has not yet subject to comprehensive review. The anticipated date of that review would be a welcome addition to this chapter.

4) p. 7-3, RECOVER Activities: Present information about the status of the peer-review of ELM, the agency review of NSM and WMM by the MRT.

5) p. 7-3, RECOVER Activities: Present the effort to standardize development/acceptance of Evaluation Performance Measures between the RET and WQT.

6) p. 7-3, RECOVER Activities: Another significant effort was focused on the CERP Evaluation Methodology Workshop and products.

7) p. 7-4: Appendix 7-1 does not list the ATLSS Performance Measures and the targets for the HSI Performance Measures.

8) p. 7-5, 1st para: Additionally, RECOVER members are actively working on a process to conduct Performance Assessments, including the use of historical data, current MAP data, supplemental research, and modeling tools.

9) p. 7-7, Table 7-1: The American crocodile is one of the interim goals indicators.

10) p. 7-9, Evaluation: Many of the hydrological performance measures applied to the SFWMM and, potentially to the RSM, address ecological considerations. For example, during the Restudy, SFWMM output included the number of days that water levels exceeded X level on a continuous basis during Y season (e.g., the deer herd performance measure), or similar hydrologic performance measures based on ecologic considerations. This section does not capture this integration of hydrologic performance measures with ecological consideration, and so does not do justice to the process that has evolved during the Restudy.

11) p. 7-9 to 7-10, Simulation Models: This section is silent on on-going efforts to integrate RECOVER with the PDTs. Since this is where the "rubber meets the road," this chapter would only be strengthened if it were to acknowledge ongoing efforts to establish this link.

12) p. 7-11, Tables 7-3 and 7-4: Do these tables only projects funded through the District? Are they the latest lists? Does it include projects already started? For example haven't the soil mapping LILA, Wading bird Synthesis, and Regional Hydrology monitoring network- elevations been funded and started? It would be good to show which projects have already been implemented. How much of LILA was funded exclusively by CERP? Was Alligator distribution etc. targeted for FY04 funding also? FY03 is covered by CESI funds but there is uncertainty about FY04.

13) p. 7-14, Related Efforts: Would this (or some new section) not be the appropriate place to link to other, non-CERP efforts that will affect CERP planning? Candidate projects would include Istokpoga Canal works, the Interim Operational Plan (IOP), and the developing Combined Structural and Operational Plan (CSOP). The last of these seeks to integrate operational considerations of the Modified Water Deliveries program and the original C-111 Project, its GRR, and its Supplemental GRR.

Chapter 8A

1) See Comment #1 in Chapter 4B above.

2) p. 8A-4: Remove reference here (and elsewhere) to DOI technical representatives' involvement in the "consensus approach" of the Long-Term Plan.

3) p. 8A-12, Phosphorus loads to the EPA: The section is good but should be amplified to provide comparisons with historical loads. See comment #1 in Chapter 4A above.

4) p. 8A-12, Phosphorus loads to the EPA: There should be an accounting and discussion of the cumulative P loads to the refuge and WCAs relative to 1978-1988 conditions and evaluation of compliance with the consent decree's load reduction requirements. From previous TOC discussions, the District agreed to include this in their annual report.

5) p. 8A-14, Table 8A-7: Please list the areal deposition rates (mg m^{-2} yr^{-1}) assumed here, not just the reference.

6) p. 8A-15, Table 8A-8: Add mass balance on Rotenberger, impacted by STA-5. While not in EPA, it is still relevant to the regional P balance, especially since outflows from Rotenberger enter the EPA.

7) p. 8A-15, Table 8A-8, Loads into WCA-3A. The L3 input includes G88. Flows through G88 pass through S8. Therefore, the G88 flows & loads may be double counted here.

8) p. 8A-15, Table 8A-8: Where are the bypass loads from STA-5?

Chapter 8B

1) General, C-11 West Basin: The new pump station 9A was constructed as a seepage – 5) return system. This pump was design to return WCA-3A seepage water back into the EPA. Checking out the phosphorus data summary and the box / whisker plots in the appendix, there is no difference between the mean TP values of S-9 and S-9A. It seems this system is not functioning as designed. Please discuss.

2) General: S-178 in the C-111 Basin has a mean TP value over 20 ppb. What does the Everglades Stormwater Program have planned to bring the discharges from this structure into compliance?

3) p. 8B-5, Table 8B-1: The high values for Boynton Farms, despite the high sample size (n =55), deserves further discussion.

4) p. 8B-6, 1st full para: What is the % at or below 50 ppb (Phase I levels)?

5) p. 8B-8, 2nd para, last sentence: "The District is continuing to monitoring these structures to ensure collection of quality data." This is contrary to the results presented in Table 8B-1 (p. 8B-5) where there are records of "no data available" for "Total Flow Volume", "Number of Days with Positive Flow", "Sample Size", "Total Samples Collected During Flow", Flow-Weighted Mean Concentration", and "TP Load" and Table 8B-4 (p. 8B-10) for "Average Annual Load". Please change this language.

Chapter 8C

1) General: This chapter is pretty broad and doesn't get into cost-share issues. One relevant question would be: "How much land is left to buy for individual projects?"

Chapter 8D

1) p. 8D-6: What is the cost of STA-1E currently in relation to the projected cost when designed?

Chapter 8E

1) General: Very good information and well written.

2) General: What is missing from this chapter is a discussion of what effort were undertaken in WY2003 (and what the preliminary results/conclusions were).

3) p. 8E-8, 1st line: Explain to the reader (a) what the cambium is, and (b) why it is important.

4) p. 8E-8, Herbicide Toxicity to Wildlife: Define for the reader what LC_{50} is.

5) p. 8E-13, 2nd para, 5th sentence: Include the scientific name, *Oxyops vitiosa,* for the melaleuca snout beetle.

6) p. 8E-13, 4th para, 2nd to last line: should read, "current infestations of melaleuca and to prevent their….".

7) p. 8E-14: As with last year's report, there are several recent *Lygodium* citations missing:

Brandt, L.A. and Black, D.W. (2001) Impacts of the introduced fern, *Lygodium microphyllum*, on the native vegetation of tree islands in the Arthur R. Marshall Loxahatchee National Wildlife Refuge. Florida Scientist. 64(3): 191-196.

Darby, C. and McKercher, L.R. (2002) Bones wrapped in *Lygodium microphyllum* rachis suggest a potential problem for wildlife. Wildland Weeds. Fall, 2002. p. 14.

8) p. 8E-15, 5th para titled "Control", first and second sentences: The first two sentences should be updated. Biocontrol options have been explored since 1998. Moths and sawflies which feed on Old World climbing fern (*Lygodium mycrophllum*) have been found in Australia and Southeast Asia and these insects appear to have promise as biological control agents of Old World climbing fern. The discovery of promising natural enemies of *Lygodium* will lead to the utilization of some of these biological agents in the control of the fern in Florida. Host-range tests in U.S. quarantine have been completed for a defoliating moth, *Cataclyst camptozonale*, to ensure the safety of the organism relative to local native plants, agricultural crops, and ornamentals. This month (September 2003) the Technical Advisory Group of the U.S. Dept. of Agriculture is being petitioned for permission to release *Cataclyst camptozonale* into the United States for control of Old World climbing fern (*Lygodium mycrophllum*). After obtaining all the necessary Federal and State approvals, release of this insect for control of Old World climbing fern is expected within the next year.

9) p. 8E-26, 4th para, 1st sentence: Include the scientific name, *Oxyops vitiosa*, for the melaleuca snout beetle.

Chapter 8F

1) General: Some water is supposed to be supplied to the environment, correct? Yet, other than MFL's there is no mention of any water supply for the environment. What was done in 2003?

2) General: A table of consumptive use permits by county by year would be valuable.

3) General: A south Florida water budget (completed) and then an analysis of the LEC area compared to the year 2020 would be of value to determine if there will be enough water. This report would be a good place for that to occur.

4) p. 8F-1, last para, 2nd sentence: The statement, "Consensus on the wording of the rules was developed through stakeholder workshops with the WRAC," is not an accurate description of the process that occurred, as consensus was neither required nor achieved prior to action by the Governing Board. A more accurate description of these workshops would be the statement, "Stakeholder concerns and suggestions on the wording of the rules were identified through a series of public workshops with the WRAC."

5) p. 8F-3, Reservation of Water for the Environment and Assurances for Existing Legal Sources: If this section is to be useful in guiding readers to the relevant statutory requirements and the current District white paper on water resource assurance strategies, it should include citations of the relevant sections of Chapter 373 and WRDA 2000, as well as a full citation of the title, date and weblink, for the District white paper.

6) p. 8F-3, 1st para, 4th sentence: This sentence should read, "Federal law requires protection of existing legal sources from elimination or transfer" not "existing local sources." The citation of the relevant section of WRDA 2000 would be helpful.

7) p. 8F-3, last para, 3rd sentence: The term "CERP Guidance Memoranda" should be replaced with "guidance memoranda required in the CERP Programmatic Regulations." ("CERP Guidance Memoranda," as the term is currently used, are not mandated in the Programmatic Regulations but are developed and adopted by the Corps and District on an as-needed basis.) Also, it would be more appropriate for this sentence to state that "the concepts and methodologies presented in the paper may be used as a starting point" for the guidance memoranda. Although it is virtually certain that the white paper's concepts and methods will be used to help development of the guidance memoranda, a definitive statement about what the GM process will entail cannot be made until the regulations have been issued.

8) p. 8F-3, last para, last sentence: Please clarify whether the opening words, "This document . . .." refer to the white paper or to one or more guidance memorandum.

9) p. 8F-3, 1st para, last sentence: Please check the statement that "State law also requires the adoption of water reservations" and provide a citation from the statute. The reservations statute by itself only states that reservations may be adopted. However, the Federal and State agreement for CERP water reservations (the "President-Governor's Agreement") does affirm the State's commitment to adopt water reservations for CERP projects; it would therefore be appropriate cite it here.

Respectfully submitted,

The Everglades Program Team, U.S. Department of Interior

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Topic: Arthur R. Marshall Foundation & Florida Environmen (1 of 1), Read 18 times NEW
Conf: PUBLIC COMMENT
From: Trudy Morris -Webboard Manager tmorris@sfwmd.gov
Date: Tuesday, September 30, 2003 08:54 AM

Arthur R. Marshall Foundation & Florida Environmental Institute, Inc.

Public Comment to Everglades Consolidated Report (ECR) Peer Review Panel

NOTE: This is also public comment on the Long-Range Plan for Achieving Water Quality Goals (Attention: Gary Goforth)

ABSTRACT

ARM FEI Public Comment was provided to The ECR Peer Review Panel (PRP) during their open session Sept 24, 2003. Areas addressed included:
I. Lack of Trees as BMP's in Conceptual Long-Term Plan for Water Quality and CERP Implementation.
II. RECOVER Monitoring & Assessment Plan (MAP) deficiency: Lack of a Northern Everglades Watershed Conceptual Ecological Model (CEM)
III. Lack of focus on officially approved CERP Table 5-1 goals and objectives in approach to achieve a consistent evaluation methodology. [bottom-up approach prevails as
distinguished from a top down approach where CERP Table 5-1 is the central organizing theme.]
IV. Imbalance in Philosophical Approach [e.g. LILA - new comment]

Hard Copy Handouts to ECR PRP:
1. Environmental Action Committee (EAC) Resolution for Establishment of a Northern Everglades Watershed (NEW) Conceptual Ecological Model (one page, as previously provided to SFWMD Gov Board twice, WRAC, CROGEE, Task Force, and SCT)
2. Impacts on CERP implementation due to absence of NEW CEM (one page, double sided on other side of recent issues of #1 above)
3. Reprint of CERP Table 5-1 Goals & Objectives, with comments (one page, same distribution as 1. and 2., above, with essentially same comments)

The Following is a paraphrase of oral comment provided to ECR PRP with some amplification.

I. Lack of Trees as BMP's in Conceptual Long-Term Plan for Water Quality

A. There is a dearth of data on the uptake of Phosphorus (P) by trees. District cites the 1984 Cypress Swamps (Ewel & Odum) as the reference for the govt to state the lack of a case for Trees as BMP's: "...literature is clear that trees provide minor TP removal in treatment wetlands".

B. ARM Florida Environmental Institute (FEI) position is that:

1. This qualitative assessment of minor effect based on 20 year old study in a difference context based on Cypress study cannot be quantified until the pilot program

recommended by the ECR PRP last year is undertaken. I.e., the pro-con arguments will continue unresolved, based on out of context data. In the ARM Lox National Wildlife Refuge 2001 Planting, there is visual evidence that newly planted cypress trees grow much faster in high concentrations of P. This is an indication that cypress swamps may be much more effective in sequestering P in early stages of growth than indicated in previous studies, which consider mature forested wetlands.

2. Regarding Custard Apple, there is essentially no studies or data to indicate the habitat and functional effect of the filter forest that preexisted south of Lake Ocheechobee on the uptake of P. Anecdotal evidence and observations are:

+ It is generally accepted that pre-drainage P level in Lake OK was about 40 ppb.
+ Lake water flowed through Custard Apple Forest
+ Custard apple forest, when removed, provided one of the richest ag soils (Cracker history of Okeechobee)
+ South of Custard apple forest was pre-drainage oligotrophic Everglades.
+ Pre-drainage condition for oligotrophic Everglades was/is P < 10 ppb.
+ Questions remain:
??? Where did the P go?
??? Is this question self-answering?
??? Why has an answer, or the question, not popped up in CERP process?

3. Lack of a NEW CEM, per handout 1, resulted in no focus on the habitat and function of the Custard Apple filter forest, thus a major question remains unanswered in the
implementation of CERP.

4. Regarding panel question: What would be the cost of a pilot program [left unanswered in oral comment, except for a possible trial in the planting of 40,000 custard
apple trees on Torry Island]. We think the initial answer is about $100,000.

5. We responded to a $4.75 Million SFWMD RFP for Public-Private Partnership Proposals for P reduction in Lake OK. We priced the start of a pilot project measurement at $96,000. Our proposal was graded #4. The entire $4.75 million was given to the top two responders. We calculated that the cost per pound of P removal with trees over a 50 year life cycle was about $1 per pound. This compared to $50 to $250 per pound using more exotic technologies as briefed in the ACME Basin B estimates. We note from the Conceptual Long-term Plan, that:
--- "Substantive scientific uncertainties remain";
--- "Additional measures will be needed to achieve water quality standards";
--- "remaining uncertainties include [long-term] performance of STA Enhancements"

6. There is no long term uncertainty about the overall value of forests in an ecosystem, including the long-term sequestration, of P, N, C and contaminants. This is
indirectly assessed by:
+ Dollar value of a tree for habitat and function over a 50 year life cycle as $196,000 (see www.wesaveyourforests.com)
+ Anecdotal observation that even 1% of a 20,000 pound biomass x 500 trees per acre is a lot of P.

7. ARM FEI position is that trees [forested wetlands] as BMP's is just another tool in

the BMP tool box, or should be, where hydrology and preexistence dictates. In this regard, the 3000 trees planted in STA - 5 is a start.

8. Additional observations on govt underconsidering the functional and habitat value of trees/forests. This represents:

--- Employing more engineering than natural science, in the search for solutions, when natural solutions are low-tech, low-cost, low-risk easy to implement, compared to
hi-tech, high-cost, high-risk engineering approaches
---- Failure to focus on cost-effectiveness of natural solutions in the monitoring effort, by declining to conduct a pilot program on P uptake by forests, thus precluding a
cost-effectiveness analysis, per CERP Section 7.5.3.
---- Single problem orientation that under-considers multiple problem solutions that meet all CERP objectives, by a single action; this includes one of the largest public-private outreach programs that involves the public hands-on, in all the hoopla over govt public outreach, where hands-on is rarely accomplished.
NOTE: This is yet another indication of the lack of integrated focus on CERP Table 5-1 CERP goals & objectives, as discussed in section III herein.

---- Summary: While the govt has acknowledged the role of trees in restoring habitat, in addition to P uptake being under-considered, the following value of forested
wetlands is also under-considered.
+ Air Pollution controls
+ Value of forested wetlands in reducing evapotransporation.
+ Role in Flood Control
+ Value as urban and community outreach projects
+ Role in soil accretion and soil oxygenation
+ Cultural Value
+ Cost to society in removal of trees and soil, rather than replacement
+ $$$ Value of a Tree over the Life Cycle of CERP ($196,000)
+ Fact that restoration of tree habitat meets all CERP Table 5-1 Objectives

RECOMMENDATION:

+ Repeat the Call for a pilot program to measure the long-term effectiveness of P uptake by trees (forested wetlands).

+ Plant more forests in CERP implementation.

+ Add an appendix to the Long-Range Plan that details the measurement methodology for ascertaining that the P = 10 ppb standard be met.

+ Focus more on CERP Table 5-1 CERP Goals & Objectives, as noted in Section III, herein.

II. RECOVER Monitoring & Assessment Plan deficiency: Lack of a Northern Everglades Watershed Conceptual Ecological Model (CEM):

Lack of a NEW CEM continues to undermine CERP implementation due to lake of

numerous stressors in this area of concern. Hard Copy provides details. Cites Orally:

A. Complete lack of consideration of habitat and function as a filter forest of ~ 60,000 acres of custard apple forest that existed south of the lake.

B. Nick Aumen's comments that expressed concerns over potential impact to Rotenberger Track on using this area for storage, or words to that effect.

RECOMMENDATION: Call for establishment of a MAP NEW CEM.

III. Lack of focus on officially approved CERP Table 5-1 goals and objectives in approach to achieve a consistent evaluation methodology.

A. Failure to focus on these objectives as a central organizing theme, is a major deficiency in the present CERP RECOVER/MAP process.

B. Additionally, per hard copy hand out, major trade-off assessments have yet to appear. CERP Section 7.5.3 requirement for Cost-effectiveness analysis in considering
alternatives is germane.

C. Restoration of Florida's ancient forests meets all seven Objectives of CERP Table 5-1 Goals and Objectives.

RECOMMENDATION: For top-down approach in CERP implementation and evaluation, focus on CERP Table 5-1 Goals & Objectives.

IV. Imbalance in Philosophical Approach
Regarding the LILA (Loxahatchee Impoundment Landscape Assessment) approach to expand knowledge of the effects of flow: This approach brings to mind, that one of the flaws in the present process is an imbalance in philosophical approach. Such an approach has two "Dimensions". Epistemological (knowledge) and Metaphysical (reality). It appears that matters metaphysical are being excused in the quest for knowledge. The metaphysical reality is that flow existed as a primary characteristic of
the Everglades ecosystem, and in context with the definition of restoration, should be a primary thrust in a reality based approach to restoration. Same for trees (forested
wetlands). A more balanced approach would place reality in terms of what previously existed (habitat and function) as the primary reason for restoration rather than attempting to develop the knowledge of why this is so; knowledge would then flow from the metaphysics of the matter, rather than vise-versa. While the pursuit of knowledge is a noble endeavor, would it not be more effective to temper this with a dose of reality. The present approach is frequently out of touch with the reality of what
pre-existed. Existence existed. QED?

This brings to mind the philosophical approach of Art Marshall, who stated: "I have to believe as all scientists should, that the more exact we define realities, the closer society will adhere to them. If this is not true, then many of our careers are personal opiates with little hope for the world."

RECOMMENDATION: Balance the approach with more adherence to reality;

knowledge is sure to follow.

Respectfully submitted,
John Arthur Marshall
President/CEO
www.ArtMarshall.org

Topic: Comments on ECR Panel Report (1 of 1), Read 25 times NEW
Conf: ECR Panel Report-DRAFT
From: Sujoy Roy sujoy@tetratech.com
Date: Friday, October 03, 2003 02:14 PM

Comments on ECR Panel Report by Gary Bigham and Chris Mackay of Exponent, Inc., and Sujoy Roy of Tetra Tech

Posted October 3, 2003

We have reviewed the October 1st Draft Report of the Peer Review Panel concerning the draft 2004 Everglades Consolidated Report (2004 ECR), and the statement on page 16 that "biodilution does not reflect reality." We respectfully disagree with that statement, and ask that you reconsider it in view of the comments we posted on the 2004 ECR web-board on September 24, 2003 under the folder entitled Chapter 2B.

As you will note, no data or analyses have been produced in the 2004 ECR to refute evidence of demonstrable biodilution. Furthermore, key data produced by SFWMD and used by Exponent and Tetra Tech to support the manifestation of biodilution appears to have been systematically ignored. The SFWMD's measurements of mercury concentrations in Gambusia along the WCA-2A nutrient gradient, show a nutrient-Gambusia Hg relationship where the low fish mercury concentrations are present in zones of high phosphorus.

The 2004 ECR proposes that low mercury bioaccumulation in the northern portions of WCA-2A is being driven by sulfate (limitation of mercury methylation in sediment due to sequestration of mercury by sulfide) and is unrelated to phosphorus. However, the data also show that sulfate concentrations and mercury methylation rates across the nutrient gradient, as monitored by SFWMD, are relatively uniform. We believe that the action of sulfate on mercury methylation rates in WCA-2A cannot explain the clear gradient in fish mercury concentrations. The 2004 ECR does not address this issue or explain the nutrient gradient data. Instead, the report dismisses bioaccumulation by stating that measurements of periphyton show lower concentrations in the zones of high phosphorus. We believe that the existing periphyton data are not representative of total primary production, the mechanism that drives biodilution.

Mercury behavior in the Everglades is complex, and biodilution cannot clearly explain every observation regarding mercury in the Everglades, however, in the well-studied WCA-2A nutrient gradient, biodilution remains the strongest hypothesis. Therefore, the consequences of regulatory phosphorus reductions on Everglades fish mercury levels could be significant and should not be ignored.

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Comments on Section 2B (Mercury) of the Draft 2004 Consolidated Everglades Report

Gary Bigham and Chris Mackay, Exponent, Inc., Bellevue, Washington
Sujoy B. Roy, Tetra Tech, Inc., Lafayette, California

Chapter 2B:

This chapter contains limited new information, and generally appears to have been written largely independently of the Appendices. The authors' general approach is to discuss only data that are supportive of their preconceived model being discussed (i.e., reduced emissions lead to reduced deposition, which leads to reduced fish mercury levels). The equally significant presence of data to the contrary are not presented or considered. For example, in the discussion following Figure 2B-6, the fact that laregemouth bass (LMB) mercury levels in 3A-15 have not really decreased is ignored. Furthermore, the data shown in Figure 2B-6 are not consistent with data shown in Appendix 2B-5, page 27, which show LMB have substantially more mercury, and contrary to expectations, show slight increases on average over 1998-2002.

Perhaps most significant, the report acknowledges that mercury deposition monitoring has shown no decrease since measurements began in 1994 and hypothesizes that deposition reductions (unmeasured) prior to 1994 are responsible for currently claimed reductions in fish concentrations. This idea of decade-long response times cannot be reconciled with the experimentally supported observation on page 2B-1 that "Newly deposited mercury is converted to methylmercury over a period of hours to days." Yet, the authors do not address this apparent contradiction.

Chapter 2B does seem to accept, at least in principle, that nutrients and primary production can produce biodilution of mercury, although the mechanism is erroneously stated to not be important for the Everglades. The authors reject the plausibility of the mechanism because of the reduction of periphyton with increasing total aqueous phosphorus (TP), and pay no attention to experimental observations of mosquitofish and LMB (See Figure 4), collected by the SFWMD, that show high mercury levels in the presence of low TP concentrations. These data, originally presented in last year's Consolidated Report (Appendix 2B-5), are plotted in Figure 1 below.

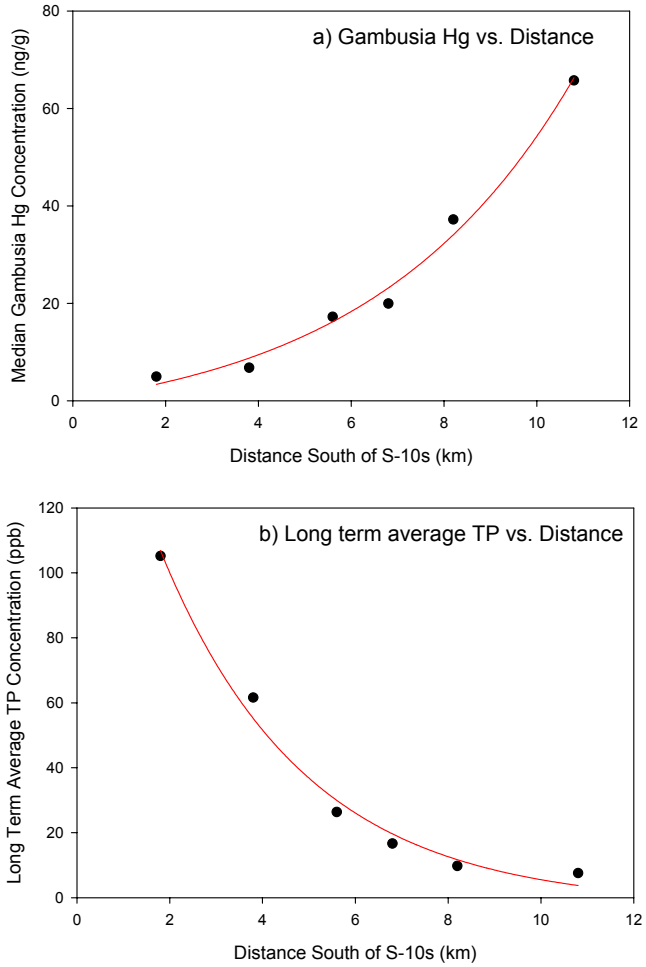

Figure 1. a) Median of Gambusia (mosquitofish) mercury concentrations plotted with distance from the S-10 inflow structures in WCA-2A, and b) long-term geometric mean total phosphorus (TP) concentrations at the corresponding sites with distance from the S-10s. Low fish mercury concentrations were seen at sites that were less than 6 km from the S-10s. These sites are also associated with higher total phosphorus concentrations.

The evidence for reduction in primary productivity with increasing TP shown in this section is highly misleading because it accounts only for benthic and floating periphyton and does not account for the mass of algae in suspension or periphyton associated with plant stems. Increased primary production has been demonstrated by SFWMD along the WCA-2A transect where it was observed that increased chlorophyll content in the water column was concurrent with increased phosphorus concentrations in the water (shown in Figure 2).

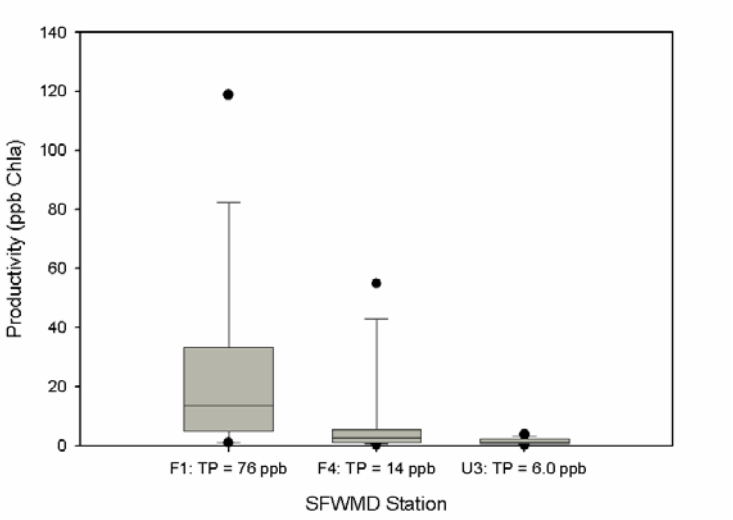

Figure 2. Primary algal productivity and median phosphorus concentrations along the SFWMD transect in WCA-2A (1994-1998).

On page 2B-16, the authors make strong statements about the role of sulfate, and the potential need to regulate it, especially because sheet flow promoted by CERP may transport sulfate further south. But the basis of the sulfate control hypothesis is the assertion that there is a hot spot south of which concentrations in fish again decline. This is not correct (see fish data on page 2B-5-25): Gambusia mercury levels in ENP (at site P-33) are as high or higher than concentrations in the middle of 3A. This is also true of sunfish, although there are no LMB data from ENP.

The report's hypothesis on sulfate/sulfide control of mercury methylation and hence bioaccumulation in fish is based on a comparison of data distributed widely over the Everglades. Yet, changes in sulfate and sulfide concentrations in pore water do not significantly change from north to south along the WCA-2A phosphorus gradient, a gradient over which the change in bioaccumulation in fish (Gambusia*)* is most pronounced. The report does not recognize this obvious contrary evidence.

Appendix 2B-1:

Influence of Drying and Rewetting

In the experimental data described, there appears to be some increase in MeHg in sediments post drying (even before rewetting), and a somewhat smaller increase after rewetting (Figures 12 and 14). The fact that MeHg in sediments increases even before rewetting indicates the possibility of experimental artifacts, possibly due to the disturbed nature of the core. It seems unlikely that this small difference can explain the extremely high methylmercury values seen in STA-5 (Appendix 2B-6) and STA-2 (Appendix 2B-7). In these experiments, mercury was also added in isotopically-labeled form, and in the water column, the controls (that remained wet throughout) methylated more mercury than the dried-and-rewet cores. Overall, the results of the rewetting experiments do not provide clear evidence that rewetting after drying is the mechanism responsible for the

almost unprecedented increases in methylmercury to several ng/l as observed in STA-2 and STA-5.

Appendix 2B-2

The E-MCM modeling, although important in the scientific understanding of mercury cycling in the Everglades, is not adequately developed and should not be used to evaluate management scenarios without experimental support. For example, the authors say on page 3-2 that:

> "In particular, biological methylation and demethylation rates were effectively tuned at each site to generate good results for methylmercury concentrations. Thus the application of E-MCM at these sites to-date represents a calibration exercise, not a true predictive exercise. As will be discussed further in the document, the effects of sulfides and bacterial activity are not sufficiently established in the literature or the model to robustly predict methylation and demethylation rates at a given site."

Furthermore, on page 6-9, the authors state:

> "The predicted relationship between fish Hg levels and surface water TP concentrations is very sensitive to our assumed relationship between surface water TP concentrations and particle fluxes (Tetra Tech, 2002), and it should be recognized that this is an area of uncertainty that requires further critical study and analysis."

The use of the E-MCM model to explore management scenarios later in the Appendix is premature, and the uncertainty in the model parameters and outputs far exceeds the range of predicted values such as that shown between methylmercury, largemouth bass mercury and TP concentrations at sites F1 and U3 in WCA-2A (Figures 6-11 to 6-20). Although some uncertainty analysis is performed (Appendix B), it was not applied to this set of results. Without these, it is not possible to say whether the predicted changes would be swamped by the uncertainty or not.

Appendix 2B-3

On page 3 it is stated that:

> "The net effect of this sulfate gradient is to produce a MeHg net production response distribution whereby MeHg is highest in the middle of the remnant Everglades where sulfate concentrations are about 2-10 mg/L, and lower on either end. At the high sulfate end, it has been postulated that MeHg formation is inhibited by excessive sulfide levels that result in porewaters; whereas in the low sulfate areas (principally the National Park) MeHg levels are moderate to low because of low sulfate availability."

This crucial statement is not supported by the data. Data from the REMAP do not consistently show this pattern in all seasons that were sampled, and far more important, the most recent District data do not show these patterns in fish. Fish in the ENP have mercury levels that are as high as or higher than the values that are measured in the middle of WCA-3A (see fish data on page 2B-5-25). Hence, the proposed relationship does not hold in the field.

The hypothesis of abiotic methylmercury formation at site F1 in WCA-2A (page 3) is an important new development in the Everglades. What makes F1 so unique? If the abiotic pathway is correct, what is the role of sulfate and sulfide in the production of methylmercury at this site? Why can the same mechanism not apply in STA-2 and STA-5? No data in support of this mechanism are provided in the Appendix. Given the novelty of this pathway, and its potential to affect other parts of the system, this statement must be supported by data.

Figures 2B and C compare MeHg formation in mesocosms in WCA-2A (F1) and WCA-3A (3A-15). The enriched site in WCA-2A is shown to produce MeHg maxima an order of magnitude higher than 3A-15. This appears to be in direct opposition to the sulfate control mechanism presented in Appendix 2B-1. Furthermore, despite this greater initial production of methylmercury, Gambusia Hg levels in F1 are far lower than in 3A-15. What causes this difference? Are methylmercury removal or dilution processes (e.g., nutrient-promoted settling of particulates or increased biological productivity) not as important as the reactions producing methylmercury?

Figure 3 shows the uptake of Hg in fish. In an earlier discussion of these data, shown to the 2003 ECR review panel and reproduced below, data from F1 and U3 were also shown. These data indicated that, other things being the same, F1 bioconcentrated mercury to a lesser degree than U3 or 3A-15. This is an important finding and should be included in this chapter.

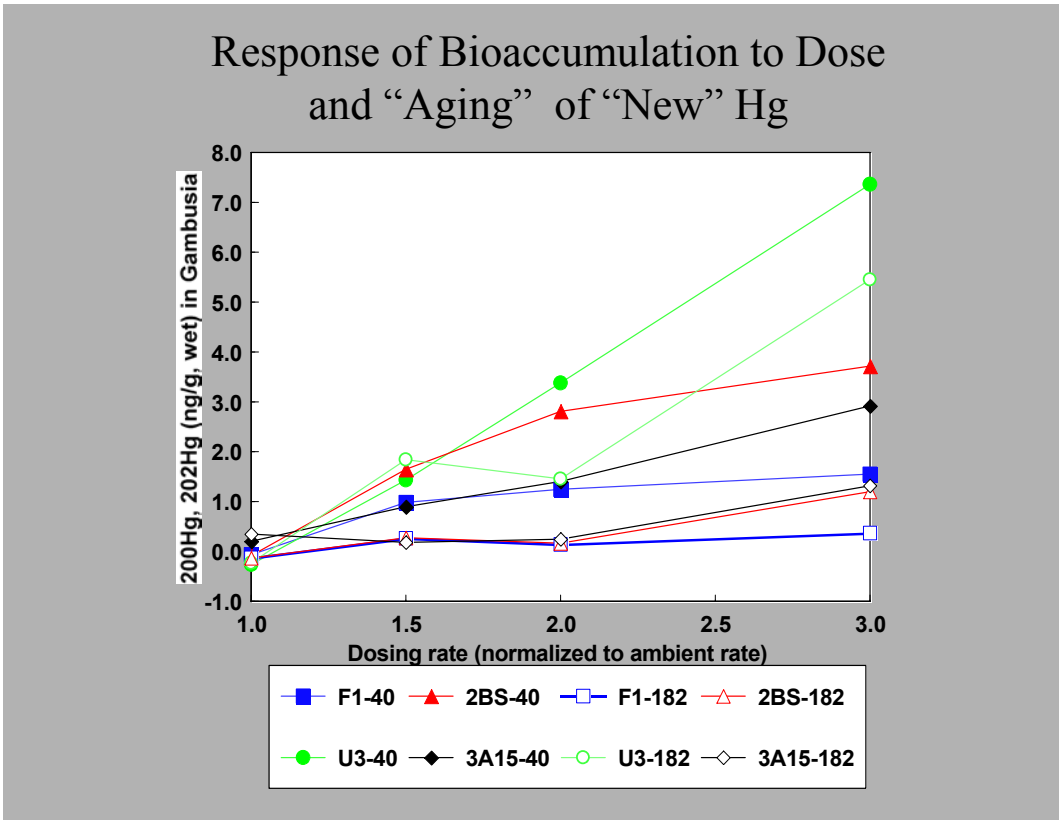

Figure 3. Uptake of mercury in fish at different locations in the WCA-2A, WCA-2B, and WCA-3A for two different points in time after dosing. F1, a nutrient enriched site, shows lower uptake in Gambusia than do oligotrophic sites (U3, 2BS, and 3A-15). Figure presented by W.H. Orem to 2003 Peer Review Panel on the Everglades Consolidated Report.

Appendix 2B-4

In this Appendix, a study is reported that examined measured mercury concentrations in feathers collected from birds within the Everglades between 1990 and 2000, and museum specimens ranging from the turn of the century to 1990. In brief, the study reported significantly higher mercury concentrations in the feathers collected in the field in and around the Everglades since 1990, compared to those identified and collected from the museums. This is the sole support for the conclusion that piscivorous wading birds are exposed to higher mercury concentrations since 1990, than they were previously.

There was a confounding factor in the study that was not considered in the experimental design that draws into question the quantitative value of these observations. The primary problem is the types of feathers used in the comparison. For the birds that were sampled in the field after 1990, the feathers that were collected for mercury analysis were taken from the scapula region. For the museum samples tested, the feathers were body contour feathers taken from the abdomen. These feather types vary greatly in structure and

growth pattern. Although the patterns differ between species, in general, the scapular feathers are larger and slower growing than the contour. Furthermore, the contour feathers (with the exception of the postnuptial body feathers) are not subject to synchronized molt, are fast growing, and may last longer than 1 year. The scapular feathers tend to follow a synchronized annual molt pattern (Proctor and Lynch 1993).

The variability in mercury concentrations between different feather-types collected simultaneously has been documented in numerous studies across different species. Montreiro and Furness (2001) reported 10-fold differences between breast and scapular feathers collected from Cory's Shearwaters. Frank et al. (1983) found 3-fold differences between back and belly feathers taken simultaneously from common loons. Furthermore, these investigators also demonstrated that the rate of mercury incorporation into feathers was significantly affected by the nutritional status of the bird. The molting order also appears to affect the rate of mercury accumulation in feathers. It has been observed that feathers developed early in the molt phase will have higher mercury concentrations that those developed later (Furness et al. 1986). The variation in mercury concentrations in different feather-types between species has also been observed (Becker et al. 1994). Braune and Gaskin (1987) showed not only differences in average concentrations on mercury between abdominal and scapular feathers, but also that the percent variation was significantly higher in the scapular compared to the contour feathers. This may explain the high variability observed in the field-collected samples.

The evidence available in the literature pertaining to mercury accumulation in feathers strongly indicates that different feather types are not comparable. The morphological and physiological differences dramatically affect the mercury concentrations in the feathers under identical mercury exposure conditions. Since the study only reported differences between museum collected samples verses field collected samples, there is no way to determine whether this was the result of differences in mercury exposure, or differences in scapular verses contour feather accumulation patterns in the test species.

Appendix 2B-5

The data in the compliance monitoring report provide excellent support for the manifestation of biodilution within the Florida Everglades. In comparing the high phosphorus region of F1 to the low phosphorus region of U3, it is evident that the mercury concentrations in Gambusia, Sunfish, and Largemouth bass are lower where phosphorus concentrations are higher as shown in Figure 4 (this plot shows the large differences even on a logarithmic scale). This is particularly obvious for the Gambusia, which are the most localized of the fish species monitored. The conclusion that avian piscivores are still at risk can be modified to state that the avian piscivores are still at risk except in areas where the Average TP concentrations exceed 15 ug/L.

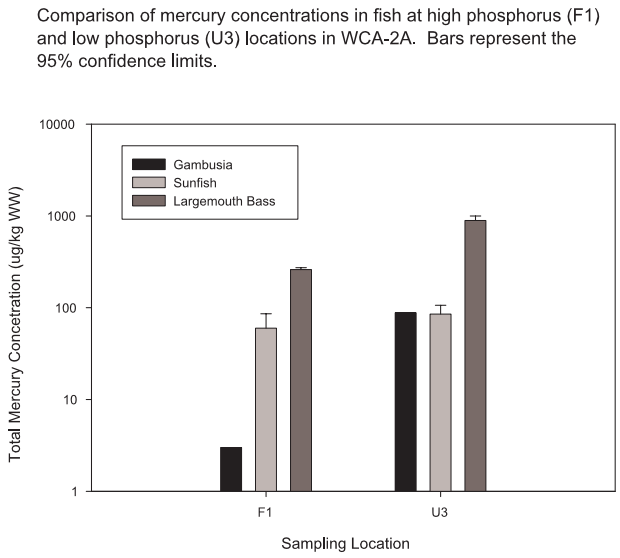

Figure 4: Observed mercury concentrations in gambusia, sunfish, and LMB as reported in SFWMD's permit compliance report for 2002 at high TP (F1) and low TP (U3) regions of WCA-2A.

Appendices 2B-6 and 2B-7

Both STAs 2 and 5 have now been found to be producing high concentrations of methylmercury, yet both contain enough sulfate in inflows to lead to sulfide inhibition as proposed in Appendix 2B-3 (Figure 7A). The fact that sulfide inhibition is not occurring at these sites is surprising. The hypothesized role of wetting/drying in not demonstrably supported by the laboratory tests described in Appendix 2B-1. The underlying causes of the differences in mercury behavior between the STAs and the Everglades still remain poorly understood, and it is possible that there remain many gaps in understanding of methylation in both systems. The data do not support the proposal of control measures for sulfur. The interactions of mercury and sulfur need to be more fully elucidated, and the levels of sulfide thought to be inhibitory specified more accurately.

Summary of Review Comments

Although a substantial part of this year's report is devoted to describing mercury-related monitoring and research, many important unknowns remain that are not adequately discussed. Principal among these are the absence of trends in largemouth bass mercury levels in WCA-3A, the high levels of methylmercury in some of the STAs despite high sulfate levels, and the decreasing mercury concentrations in Gambusia along the nutrient gradient in WCA-2A. These data illustrate that much remains to understood regarding the decrease of atmospheric deposition, sulfur and nutrient chemistry, and the effects of hydrological processes such as wetting and drying in the Everglades.

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