Appendix 1A-3: Comments on the *2007 South Florida Environmental Report –Volume I* 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, this appendix was not edited or spellchecked by the SFER production staff and appears as posted on the District's WebBoard.

Subject: **Public Comment to ISR on Trees (Forests)**

From: JamInfo@aol.com [mailto:JamInfo@aol.com]
Sent: Thursday, September 28, 2006 11:49 AM

Subject: Public Comment to ISR on Trees (Forests)

Memo for South Florida Environmental Report Peer Reviewers

Dear Dr. Meganck,

Per your request in getting something to you today, before close of ISR panel, noting that this is less than a 20 hour rushed response, the following summarizes the attached commentary and documentation of a fast growing forest:

- SFWMD primary consideration here is hurricane barriers that will protect STA's, some of which were ripped up badly in the last set of hurricanes. This is good, so why not combine hurricane protection (Katrina gulf coast lesson) with one good experiment - the previously suggested pilot project - to measure impact of trees for nutrient uptake, as well as for hurricane protection.

- Famous quote: One good experiment is worth a 1000 expert opinions.

- In the vegetation vs. trees argument to take up P: Marsh grass/Cattails/SAV, etc., is faster growing biomass, the attached report on the Lee Road Forest, attached, appears to defy that argument.

- On the argument that *trees do not sequester much P, the Cypress data is known, we can deduce what custard apple might contribute*, or words to that effect: This appears to be speaking with more confidence than is justified without producing the data. More than expert opinion appears needed. There is no visible data, especially regarding custard apple nutrient uptake, by the historic 60,000+ acre custard apple forest south of Lake OK, to justify this statement, back to bullet #1 and #2

- Restoration of Habitat is in the top ecological restoration goal and three objectives of CERP Table 5-1. (See Public Comment Handout [annotated] passed to ISR panel). There does not appear enough focus here with respect to restoration of trees, per Dr. Harris comments.

- Esthetics and environmental ed. The Marshall Foundation has a great program, perhaps the only program that is restoring Florida's ancient forests on the needed massive scale. The 75,000 + trees we have planted are a drop in the bucket needed. One more chance for public outreach in the scheme of the SFWMD public outreach program the Marshall Foundation supports, by partnering with the SFWMD.

- Long-Term Vision? There is uncertainty about how long SAV/Marsh STA's will remain effective, and move from a P sink to a P source; What does a tree the size of a telephone pole provide as a P sink long term? Is there long term thinking? Of course there is yet no long term data on STA long-term effectiveness

- Need for a conceptual ecological model in the historic northern everglades watershed (EAA region); Has the historic ecological value of the custard apple "filter forest" been overlooked because there is no conceptual ecological model for this region?

- Air Filter; there is lots of smoke when the sugar cane fields are burned.

- OK, carbon sequestration gets low priority, however smaller projects cannot be dismissed if we are to preserve the planet.

- What else?

The attached commentary is lengthy, but does provide the means for further debate on restoration of trees (forests) that were a significant portion of the historic Everglades ecosystem, in addition to justifying the summary, above. Art Marshall was about restoring historic flow and re vegetating. This is part of re vegetating, and as Dr. Harris has mentioned, fundamental to the "restore" word.

Thanks for your consideration here.

John Arthur Marshall

www.artmarshall.org

Dr. Tom Paulson, Professor Emeritus

John, Stuart, and Larry:

Here is a quick response; sequestration of carbon, nutrients, and toxins is an extremely complicated issue for which many of the needed techniques for study or data are not available or are under study. So be cautious of claims, like by "American Forests" of quantitative data on the uptake of substances and the cost savings (e.g. To public health). Savings are very important but most places, including those web sites / organizations that are promoting carbon credits via tree-planting do not give you their sources of data much less assumptions involved with calculations. [Note, Princeton University is into research here; need to take a look]

Though sequestration by plants certainly occurs, everything I have read (including two hours on the Internet last night) agrees that even potentially it is in the drop in the bucket compared to anthropogenic generation of CO 2, plant nutrients, and toxins. The old adage of an ounce of prevention is worth a pound of cure holds in spades. For carbon California's passed legislation (that Schwarz has agreed to sign) is the best way to go. In the vein of think globally and act locally, the Paulson drive two hybrid Toyota Pius (2002 at ~ 43 mpg and 2005 at ~ 53 mpg) and are switching to all compact fluorescent lights and keep the thermostat at 68 F in winter and 80 F in summer. For plant nutrients in the Everglades as a starter we need laws that hold Lissome farmers to the same BMP standards as EAA farmers.

OK, on to answers to John's questions about what trees can do in ecosystem services (we should think of goods too, as in sustainable harvest). My initial comments are based on first principles (e.g. Rates and surface area to volume ratios), what I have taught about physiological plant ecology, my studies of successions and old-growth forests, and what I read for two hours on the Internet last night. Of particular use were 85 abstracts all on carbon sequestration in Comparative Biochemistry and Physiology Part A 134 (2003): S 1 – 198 (yes I read all of them!).

Carbon sequestration by trees and forests: This is in principle easiest of the topics to study. The best studies use many plots (up to 1000 in some studies) in a forest to measure net primary productivity (CO 2 uptake = sink) and soil respiration (e.g. by decomposers and roots

CO 2 production = source) to calibrate models. They account also for leaf fall and woody litter. In general they find what I would hypothesize; that the greatest net sinks are fast-growing forest species, forests in early succession al recovery from disturbances like fire or hurricane, and the 20 – 75 year old forests (younger and older forests are either weak sinks or even sources!). Also calculations of uptake that do not account for seasons are over-estimates since the winter dormant seasons or dry season are usually net source times. To put this in context of STA's the District folks are right that cattails, SAV, and perihelion sequences do a much better job reducing not only nutrients but also sequestering carbon. Of course this does not figure in other benefits of trees!

Phosphorus and Nitrogen sequestration by trees and forests: This is very complicated to study so I am not surprised that I have yet to find any helpful research in the primary literature. The difficulties include the complicated chemistry of phosphorus and nitrogen and how they relate to availability to plants in the context of variables like soil Ph and red ox and myocardial and root nodule symbiotic. In water the inorganic vs. organic particulate bound fractions complicate matters. So, in my opinion the best approaches is to use standard plant photometers i.e. A bioassay approach. In the context of STA's, whether herbaceous or woody, folks have barely addressed the issues of progressive saturation of the systems, whether the systems becomes a source when you harvest the above-ground biomass, and what to do with the harvested material.
As an aside for carbon one neat study showed that short rotation coppice poplar did better than a mid-aged beech-oak forest by three times in yearly C uptake but also four times better in saving emissions of C from other sources by using the coppice as fuel instead of using fossil fuel. For harvested STA plants the parallel might be forage for domestic animals though I have heard of concerns about toxins. Oh, and do not forget Larry's hypothesis, which I like, that forests of cypress or pond apple slow water movement and so promote deposition of particulate-bound nutrients.

Toxins and air pollutants: I have only seen suggestions form American Forests on this subject and they reference no assumptions or techniques. In urban settings, like the initiative with Dolphin Stadium, planting trees cannot hurt.

Trees for STA Bern stabilization and windbreaks: I think this is a very good idea though I have heard concerns about the trees making beers leak (e.g. From the Florida Crystal land manager that our Sis met with this summer). The dense growth of cypress on the Refuge Lee Road site would appear to be a great windbreak but I expect its efficacy will decline as the trees get taller and shade out the lower branches.

Trees as habitat for other species: I continue to think we should give more emphasis to this ecosystem service. Remember that the Florida Crystal land manager said that volunteer Brazilian Pepper on beers were serving as wading bird roosts (of course native species would be preferable). To make plantings more attractive as roosts or rookery sites they should be planted as groups offshore (water as a partial barrier to raccoons and other predators). Of course forests of pond apple and especially cypress also have many other shrub, herb, fern, lichen, and air-plant species. [need to bring back the native plants!]

Trees and forest as aesthetic factors: In addition to the obvious for adults there is the issue of helping to save our children from nature deficit disorder .

That's it for now,
Take care,
Tom

Dr. Larry Harris, Professor Emeritus [Author, Fragmented Forests, Florida's Ancient Forests, etc.] Dr. Harris has worked on tree/forest issues longer than any of us.

Comments, paraphrased:

John, thanks for continuing with the issue. But is our commitment to "restoration" so damn weak that we must justify restoration of what every single sentient ecologist who knew south FL in middle of 6th Century new to be the 'natural system'. Do they understand even a semblance of the word restoration; do you not have access to dictionaries? Two of the chapters in an unpublished effort (that wanes) deal with: Forests of south Florida, and Rivers of the Everglades. Needless to say, these two are not independent; almost all of what we learned, reported, studied and knew as 'forests' were associated with the 'rivers'. This would include, for example, the 6-8 rivers flowing south out of Lake Ketch, there was NOT sheet flow there. It would include the dozen rivers flowing eastward from south FL south of the lake. cheers, LDH

Posted: 29 Sep 2006 10:29 AM

The Panel's Report & District's Response - 2007 SFER » District Request of Panel Regarding Comments

Subject: **District Request of Panel Regarding Comments**

The District's response to the Peer Review Panel's draft report included a request that the panel provide guidance regarding my comments about Chapter 3B and related appendices. The District specifically requested that the panel identify which of my comments are not valid.

The District's request that the peer review panel's report identify which of my comments are not valid seems to be completely at odds with the panel's mandate and the spirit of public participation. If the District wishes the peer review panel to determine the validity of my comments, please direct the panel to review the information I have accumulated, and analyses I have conducted, in support of my comments. If the District itself wishes to determine the validity of my comments, then I invite the District to meet with me.

My comments were submitted for the benefit of the chapter authors, peer review panel members, and anyone else with an interest in the subject. That said, no one - authors, peer review panel members, or anyone else - is obligated to consider or accept my comments.

Donald M. Kent, Ph.D.
Executive Director
Community Watershed Fund

Posted: 11 Oct 2006 05:17 PM

2007 South Florida Environmental Report
Chapter 2

Comments by Pamela Telis, USGS
September 2006

General comment: The report is almost completely reporting the yearly data status, I have no technical comments. A few editorial comments are worth noting:

1. Page 2-25, line 597 – I think the word "aerial" should be "areal"
2. Page 2-31 – I think the term "10-year dry return period" and "10-year wet return period" should be defined
3. Page 2-41 – Referring to the datum NGVD should be NGVD29 – defined as the datum NGVD of 1929.

Posted: 22 Sep 2006 09:57 PM

Subject: **Kent, CWF: Chapter 3B; Appendices 1-3**

D.M. KENT, COMMUNITY WATERSHED FUND

GENERAL COMMENTS

1. The draft SFER, especially the Chapter and Appendices regarding sulfur and mercury, are being made available for public review much too late to encourage public participation and effective evaluation.
2. Listing multiple authors on an SFER chapter implies: 1) that each listed author has reviewed the material prior to public release, and 2) that each author agrees with the statements contained within the chapter. If these assumptions are not accurate then the process for listing authors should be explained.

GENERAL MERCURY COMMENTS

According to Chapter 3B, sulfate from Everglades Agricultural Area (EAA) fertilizer application is transported via the canal network to the Everglades Protection Area. Once there, the sulfate stimulates methylmercury production and stimulates the release of phosphate. Sulfide accumulation in the soil likely promotes the replacement of sawgrass by cattail.

Canal surface water sulfate concentrations are highest in EAA canals. An isotope study suggests that the sulfate originates from agricultural soil amendments. Soil and shallow and deeper groundwater are also potential sources of canal sulfate. The relative contribution of all sources should be investigated.

Bacteria reduce sulfate under anaerobic conditions, resulting in the production of methylmercury. The level of sulfate resulting in enhanced methylmercury production is unclear from Everglades Protection Area (EPA) monitoring data. Nor was the relationship between sulfate concentration and methylmercury delineated by WCA-3A mesocosm experiments. Mercury levels in fish are not consistently related to sulfate concentrations. The relationship between sulfate, methylmercury, other factors (e.g., redox), and mercury levels in fish and wildlife needs to be determined.

Soil sulfide could theoretically impact EPA biota, although no impacts have yet been demonstrated. Details of a mesocosm study of sulfate inducement of soil phosphate release should be provided to facilitate review and evaluation.

RECOMMENDATIONS

1. Establish a core group of scientists, led by the SFWMD and FDEP, responsible for mercury/sulfur research management, data evaluation and interpretation, and consensus building.
2. Engage all stakeholders in a partnership.
3. Re-consider the existing hypothesis and develop new hypotheses as necessary.
4. Identify data gaps, develop research programs to fill the gaps, and prioritize efforts.
5. Initiate research that attempts to prove the hypothesis (hypotheses) wrong.

CHAPTER 3B: MERCURY MONITORING, RESEARCH AND ENVIRONMENTAL ASSESSMENT IN SOUTH FLORIDA

Research Progress

Quantify the no-effect level for Everglades' fish-eating birds

1. What is the level of mercury in the birds?
2. Differences in nest initiations, eggs laid, nest attempts, and no apparent differences in hatchling survival between control and dosed first year ibises are interesting findings. However, it is premature to describe these findings as strong evidence of mercury effects in the Everglades (line 190). Let's see wait until we have the results from two to three breeding seasons.
3. Why are wading bird populations increasing in number if mercury in prey consistent with

white ibis dosing levels is having strong effects?

Quantify global versus local atmospheric mercury sources
1. The estimate of local source contributions to atmospheric mercury deposition (range 8 to 80 %, lines 335 - 346) needs to be better defined to enable effective decision-making about: 1) mercury source control, and 2) methylmercury management.

Revise the Everglades Mercury Cycling Model
1. The model, and its inputs, should be carefully and objectively reviewed in a transparent process.

Research geochemical controls on mercury methylation
1. ACME mesocosm experiments should be continued until the relationships between mercury, sulfur, DOC, and methylmercury are better understood. Initial findings are not fully supportive of statements made in the 2006 and draft 2007 SFERs. For example, the 2003-2004 studies described in Appendix 3B-2 do not indicate a consistent relationship between surface water sulfate concentrations or DOC and surface water or soil methylmercury concentrations.

Determine sulfur sources
1. Neither the sulfide toxicity nor the phosphate liberation results are discussed thoroughly enough in Chapter 3B or the Appendices to support independent evaluation (lines 418-421). Moreover, the sulfide toxicity study described in Chapter 3B was conducted hydroponically, diminishing certain application to conditions in the Everglades.

Trends in Atmospheric Deposition of Mercury
1. Any opportunities to continue reducing mercury from local sources?

Concentrations of Mercury in Everglades Fish
1. What is the detection limit for mercury in fish muscle tissue?
2. Loxahatchee National Wildlife Refuge Marsh site largemouth bass mercury levels exceed U.S. Fish and Wildlife Service and U.S. Environmental Protection Agency predator protection criteria (Table 3B-1). How does a site with background sulfate levels seemingly generate relatively high levels of methylmercury? Could you explain this contradiction with your theory that methylmercury production is optimized at 2 - 10 mg/L sulfate and 5 - 150 µg/L sulfide?
3. WCA-2A U3 largemouth bass mercury levels are also high, yet sulfate levels exceed the predicted optimum range for methylmercury production. According to the 2006 SFER (Figure 2B-12) sulfide levels at this site should be suppressing methylmercury production. Could you explain this contradiction with your theory that methylmercury production is optimized at 2 - 10 mg/L sulfate and 5 - 150 µg/L sulfide?
4. Any reason for the apparent decrease in largemouth bass mercury levels at the L-67A Canal site (Figure 3B-13)?
5. The high largemouth bass mercury levels at the North Prong Creek site are curious given the very low sulfate concentrations and high sulfide concentrations (Figure 3B-14). Could you explain this contradiction with your theory that methylmercury production is optimized at 2 - 10 mg/L sulfate and 5 - 150 µg/L sulfide?
6. Is the variability in largemouth bass mercury levels evident in Figure 3B-16 indicative of natural variability or analytical imprecision?

Sources of Sulfur ... Effects
1. References would be appropriate to support the statements of lines 689-693.
2. The SFER does not provide information about high levels of mercury in Everglades' mammals (line 696). Please provide summary information or references.
3. The effects of sulfate on methylmercury production are related to porewater sulfide concentrations. Please provide figures indicating porewater sulfide concentrations corresponding to figures 3B-17 and 3B-18. Similarly, please add figures indicating surface water MeHg concentrations and fish mercury concentrations.

4. Does the L-67 Canal discharge at the S Structures or further south at the terminus of the canal extension?
5. Figure 3B-17 seems to suggest that sulfate-enriched water was conveyed via the L-67 Canal to ENP in November 2005. Do you have sulfate concentration data at points along the canal? A figure depicting sulfate concentrations over time at this measurement point would help us evaluate the meaning of the November 2005 value.
6. The scale colors are different for Figure 3B-17 and 3B-18.
7. Lines 719-720 state that sulfate contamination originates in canals draining the EAA. Please note that the sulfate appears to originate disproportionately from canals draining the eastern EAA.
8. Appendix 3B-3 Map #2 indicates high sulfate levels at locations in WCA-3A and eastern ENP that do not obviously relate to canal discharges. Could you explain the high sulfate levels at these locations?
9. The statement "It has been clearly demonstrated that Typha has a significantly greater ability to transport oxygen to its roots and to produce a more extensive oxidized rhizosphere than Cladium" (lines 760-762) requires reference.
10. Mendelssohn references (lines 774-776) missing from Literature Cited.
11. Theoretically, high porewater sulfide could be toxic to Everglades' biota. Evaluation of this issue should proceed with an open mind, and not with the presumption that toxicity is occurring. For example, Lamers et al. (1998) suggest that plant toxicity should occur at 350 µg/L, and Mendelssohn's hydroponic experiment (described in Chapter 3B pages 3B-33 through 35) indicates an effect beginning at 24 µg/L. Yet sawgrass is the dominant emergent at site WCA-2A U3 in spite of a sulfide level of ca. 3,800 µg/L (Axelrad et al. 2006). The ACME mesocosm study failed to produce porewater sulfide concentrations high enough to determine if Typha or Cladium are impacted by Everglades' sulfate/sulfide levels.
12. Mendelssohn's short-term hydroponic experiment produced interesting results at levels consistent with sulfide levels observed in northern WCA-2A (pages 3B-33 through 35). That said, the results should not be presumed to reflect the response of Cladium in situ (lines 821-822). An attempt should be made to replicate the results in-situ.
13. Figure 3B-22 appears to indicate an increase in surface water phosphate at 100 mg/L (surface water, 2 and 5 cm porewater), and at 50 mg/L (15 cm porewater). Sulfate concentrations do not reach 100 mg/L in Everglades' marshes, and rarely reach 50 mg/L (Axelrad 2006, this document).
14. Please provide design details and statistics for the experiment illustrated in Figure 3B-22 so the results can be independently evaluated.
15. A conclusion about the effect of drying and wetting cycles needs to be supported by presentation of evidence (lines 984-989).
16. A conclusion that hydrological alterations have changed sulfate distribution needs to be supported by presentation of evidence (lines 990-996).
17. Lines 997 – 1004 are more speculation than conclusions.

Posted: 25 Sep 2006 12:37 PM
Originally Posted: 25 Sep. 2006 12:33 PM

D.M. KENT, COMMUNITY WATERSHED FUND

APPENDIX 3B-1: ANNUAL PERMIT COMPLIANCE MONITORING REPORT FOR MERCURY IN DOWNSTREAM RECEIVING WATERS OF THE EVERGLADES PROTECTION AREA

Summary of the Mercury Monitoring and Reporting Program

Preyfish

1. Were 100 to 250 mosquitofish, 20 sunfish, and 20 largemouth bass collected per site during a sampling event?

Monitoring Results

Rainfall

1. Is dry deposition mercury monitored? If so, what is the relative contribution from atmospheric dry deposition and rainfall?
2. Storms are natural events. Why remove storm event data from calculation (Page App. 3B-1-22, Paragraph 1, Lines 2 and 3)?

Fish from ECP and Non-ECP Interior Marshes

1. Table 6 (Page App. 3B-1-29) - 2 of the 4 sites with the highest mosquitofish THg concentrations in 2005 have surface water sulfate and porewater sulfide levels below (LOX4) or above (RotenC) the predicted optimum range for methylmercury production (2 to 10 mg/L sulfate, 5 to 150 µg/L sulfide). Similarly, 1 of 4 sites (CA2U3) with the highest cumulative average THg concentrations has sulfate and porewater sulfide levels above the predicted optimum range. How reliably can we rely on surface water sulfate levels to predict mercury bioaccumulation? What could explain the unexpectedly high THg at LOX4, RotenC, and CA2U3?
2. Sites CA27 Alt (Z4) and CA27 Alt (N4) listed in Table 6 should be represented on a figure.
3. Table 7 (Page App. 3B-1-30) and Table 8 (Page App. 3B-1-31)– Again, Site CA2U3 has unusually high THg in sunfish and largemouth bass given its surface water sulfate and porewater sulfide concentrations.
4. Where is site N2 (Page App. 3B-1-32, Paragraph 2, Line 2)?
5. Figure 10 illustrates the period of record, not 2005 as indicated in the text (Page App. 3B-1-32, Paragraph 2, Lines 1 and 2).
6. Is the increase in fish mercury levels at the Holey Land Wildlife Management Area related to restoration activities?

Predator Protection Criteria

1. A summary table indicating whether each site achieved or exceeded the predator protection criteria would be helpful.

APPENDIX 3B-2: STATUS REPORT ON ACME STUDIES ON THE CONTROL OF HG METHYLATION AND BIOACCUMULATION IN THE EVERGLADES

Introduction

1. Please make the details (i.e., design, results, analyses) of the ACME mesocosm studies conducted between 2000 and 2003 available for analysis.
2. Please make the details (i.e., design, results, and analyses) of the Loxahatchee study(s) available for review. Information provided by Dr. Orem suggests that the methylation rate is enhanced from 10 mg/L sulfate up to at least 96 mg/L sulfate if porewater sulfide remains low.

Section 1: ACME Studies

A. Mercury methylation and bioaccumulation response

1. What is background DOC at the site?
2. How do the DOC concentrations created in the mesocosms compare to natural DOC concentrations throughout the Everglades? Do you anticipate future DOC enrichment?
3. Does DM2 = DM3? Page App. 3B-2-7 uses DM3 in Figure 1 and DM2 in Table 1.
4. Statistics need to be provided to support conclusions, or conclusions qualified to reflect the absence of statistics.
5. With or without statistics, it would be helpful to average treatment replicates and provide an error bar to the figures.
6. The Fe experiment (see below) mesocosms received 7.69 µg 200HgCl2, which represents roughly the annual deposition of mercury at the site. The sulfate and DOC experiment mesocosms received 14 µg Hg, twice. Does this mean the sulfate and DOC experiment mesocosms received roughly 4 x the annual deposition of mercury within a two month period? If so, are you concerned that the Hg dosing obfuscated any sulfate or DOC effects on MeHg production?
7. None of the treatments isolate MeHg response to sulfate or DOC enrichment.
8. The controls should have been dosed with 22 µg/m2 mercury at the same time the treatment mesocosms were dosed to evaluate sulfate and DOC effects on MeHg. As constructed, the experiment allows us to look for MeHg responses to sulfate and DOC only within treatment groups (i.e., sulfate/Hg, DOC/Hg). The response of MeHg to the combination of sulfate/DOC/Hg cannot be compared to anything.
9. The top chart in Figure 3 suggests (no statistics provided) that: 1) MeHg production was greatest in the ambient, non-amended, environment, 2) the mesocosms may have inhibited MeHg production, and 3) neither increasing concentrations of sulfate nor DOC enhanced MeHg production.
10. The middle and bottom charts in Figure 3 do not suggest that increasing sulfate or DOC enhanced MeHg. The charts do suggest a significant difference between processes in mesocosms A and B; this should be investigated.
11. Redox measurements might have been more helpful than measurement of redox-indicative constituents (Figure 4).
12. Of the six charts comprising Figures 5 and 6, only the top chart of Figure 5 suggests a possible increase in MeHg in response to sulfate concentration, and then only at 20 mg/L sulfate. The corresponding B mesocosm contradicts this notion. Collectively, the six charts do not support the contention that MeHg increased in response to increasing sulfate concentrations.
13. Similarly, the six charts in Figures 5 and 6 do not support the contention that MeHg increased in response to increasing DOC concentrations.
14. The charts comprising Figures 7 and 8 also do not support the contention that MeHg increases in response to increasing sulfate and DOC concentrations.
15. The left panel of Figure 9 suggests that MeHg increased in response to increasing sulfate concentrations during one of four measurement events. What is the source of the data points in the chart that suggests a MeHg/sulfate relationship? Is Day 57 equivalent to 18 August 2003? If so, then there should be 10 data points (and not eight). Are two data points overlapping? I cannot make some of the points in the Figure 9 chart correspond with points in Figures 5 and 6.
16. The statement that sulfate exerts a strong response in net MeHg accumulation in surface soils does not seem to be supported by the experiment and results. For example, sulfate effects were not isolated in the experiment, Figures 5 through 8 do not indicate a MeHg/sulfate relationship. Clarification of the seemingly supportive chart in Figure 9 is warranted.

B: Mesocosm studies to assess the impact of Fe

1. What are typical Everglades' soil Fe concentrations? Is there significant spatial variability in the Everglades Protection Area? If so, do any areas of the Everglades Protection Area have enough soil Fe to significantly effect MeHg production? For example, do you think iron reducing bacteria might be important in low sulfate regions? Do you anticipate Fe enrichment of the Everglades in the future?
2. Are these the same mesocosms used for the sulfate and DOC experiment?
3. The statement that there was a significant difference among treatments in the production of 200MeHg between day 3 and day 59 (Page App3B-2-21) is unimportant if the difference also

extends to the control. The key analysis is whether the Fe treatments enhanced or restricted soil 200MeHg compared to the control. From the figure, it does not appear that Fe had an appreciable impact on soil 200MeHg.
4. Log transformations should be used and interpreted cautiously when there are only two data points.

Section 2. Laboratory Studies
A. Hg methylation by iron-reducing bacteria and sulfate reducing bacteria
1. Are iron-reducing bacteria a significant part of the soil fauna in an part of the Everglades?
2. Are the iron-reducing bacteria tested common in the Everglades?

B. Examination of the effect of DOC
1. Does this study suggest that DOC may inhibit MeHg production?

APPENDIX 3B-3: PRELIMINARY ASSESSMENT OF SULFUR SOURCES, TRENDS AND EFFECTS IN THE EVERGLADES
Summary and Conclusions
Sulfate Distribution
1. Is there an explanation for the high sulfate sites in northern WCA-3A and southeast ENP (Map #2)?
2. Why would sulfate concentrations be higher at the terminus of the L-67 Canal than further upstream if the source of sulfate is the EAA (Map #2)?
3. Why is rainwater sulfate concentration relatively higher in WCA-1 and the nearby canal than other EPA sites (Map #5)?
4. Why is the sulfide concentration so high in WCA-1 (Map 6)?
5. Sulfide concentrations seem to be disproportionately high in some parts of WCA-2A and WCA-3 compared to sulfate concentrations.
6. Map #8 indicates no trend in surface water flow at the S12 structures (north ENP). Doesn't this contradict the oft-repeated statement that increased flow is conveying sulfate to the park?

Influence of Sulfate on Hg
1. I disagree with the statement that the mesocosm study at 3A-15 showed a significant positive correlation between surface water sulfate concentrations and net MeHg and bioaccumulation for sulfate levels between <0.5 and 20 mg/L (lines 210-213). Please see my comments regarding Appendix 3B-2.
2. If the < 0.5 - 20 mg SO4/L optimum were accurate, then background sulfate concentrations would stimulate MeHg production.
3. The statement that the optimum sulfate concentration is above 20 mg/L (line 214) contradicts earlier statements that the optimum sulfate concentration is 2 to 10 mg/L (Axelrad et al. 2006, Chapter 3B), and the conclusion from the 2003-2004 mesocosm study.
4. The statement that sulfate-contaminated waters are being transported further into the ecosystem as more water is moved south for restoration (lines 227-228) is not supported by flow trends (Map #8) and is poorly supported by sulfate trends (Map #7).
5. Insufficient information is provided to facilitate independent evaluation of the long-term sulfur toxicity study.

Assessing Sources of Sulfur
1. Spatial isopleths would be a helpful method for illustrating sulfate, sulfide, MeHg, and fish mercury levels in the Everglades.
2. Soils and groundwater cannot be ruled out as sources of sulfate in EAA canals until they are investigated.

Introduction
Effects of Sulfur Contamination
1. The statement that maximum mercury methylation occurs at sulfate concentrations

between 10 and 100 mg/L (lines 307-308) contradicts earlier statements that the optimum is 2 to 10 mg/L (Axelrad et al. 2006, Chapter 3B) and greater than 20 mg/L (this Appendix, line 214).
2. Some Everglades sites do not conform to the hypothesis that MeHg production is greatest at intermediate levels of sulfate relatively low sulfide levels (lines 339-354). Mosquitofish mercury levels are high at LOX4 in 2005, despite very low sulfate and sulfide levels (Appendix 3B-1 Table 6, Chapter 3B Figure 3B-17, this Appendix Figure 3). Mosquitofish mercury levels were high at RotenC in 2005, which has sulfate concentrations of 20 to 40 mg/L (sulfide levels not found). Sunfish and largemouth bass mercury levels were high CA2U3 in 2005, despite relatively high sulfate levels (40 – 50 mg/L), and very high sulfide levels (ca. 3,000 µg/l).
3. The mesocosm studies described in Appendix 3B-2 do not seem to support the statements made about mercury, sulfate, DOC, and Fe (lines 355-364).
4. Insufficient information is provided to facilitate independent evaluation of the dry and re-wetting study.

Sulfur Species Distributions
1. The maps may be more helpful as spatial isopleths.
2. It is difficult to appreciate how canal water influences sulfate concentrations at Site 3A-15 (lines 484-485). The site is a considerable distance west of the L-67, which has been suggested as a likely source. In the event that the L-67 levee was degraded, water would flow south and not west. Possibly a degraded levee on the C-123 Canal would facilitate the flow of high sulfate water south to Site 3A-15, but there is no evidence of this occurring. In the absence of demonstrated flow from a canal to the site, it might be more fruitful to ask what local factors might be influencing sulfate and MeHg concentrations.

Trends in Sulfate Load and Concentration
Trends in Sulfate Loading at Major Structures
1. STA-6 and STA-5 Cells 2A/B appear to reduce sulfate concentration (Figure 8) in contradiction of the statement that the Stormwater Treatment Areas ... do not reduce sulfate inputs to the ecosystem (lines 678-679). The period of record for the STAs should be examined for sulfate loads in the inflows and outflows.
2. Investigation of the entire period of record (beginning in 1974) for the S5A structure suggests that sulfate concentrations may be decreasing.
3. The statement that sulfate concentrations tended to increase at the structures near the L-67 Canal (line 731) is not entirely accurate. According to Table C, only the S333 structure was determined to have a recent increase in sulfate. Moreover, review of the top chart in Figure 9 suggests that the apparent increase in sulfate concentration at S333 is no greater than concentrations in 1994. The seasonal Kendall analysis should be carefully reviewed for appropriateness and accuracy. Concentrations cannot be fully evaluated without considering flow; neither flow nor sulfate load have increased at S333.
4. No trend in sulfate concentration or load was detected at the S12D structure, and concentrations and loads at S12A-C either decreased or stayed the same over the period of record.
5. Collectively, trends at the S12 and S333 structures suggest that increasing amounts of sulfate are not reaching ENP, and it may be that there is a slight decrease in sulfate reaching ENP. That said, sulfate concentrations may be slightly elevated at the terminus of the L-67 Canal relative to surrounding areas and possible impacts should be carefully evaluated.

Trend Mapping
1. Spatial isopleths would be very helpful for examining trends.
2. As discussed above, sulfate concentration does not appear to be trending upward at the L-67 Canal as it crosses into ENP (lines 839-840).
3. As noted previously, it is very difficult to understand how the L-67 Canal could have influenced sulfate concentrations at 3A-15. Perhaps a more detailed description of water flow in the area, and sulfate concentrations between the L-67 Canal and 3A-15, would improve understanding. One alternative is that Site 3A-15 had recently experienced a burn. This notion is supported by observations in WCA-3A of sulfate concentrations decreasing over time after a

burn (information provided by Dr. Orem). Unfortunately, no information about sulfate concentrations before the burn were provided.
4. Lines 896-898 state that sulfate-addition mesocosm studies show that net MeHg production and bioaccumulation at this site (3A-15) is (sic) highly sensitive to sulfate concentration (see Appendix 3B-2). Again, I disagree that the sulfate-addition mesocosm studies reported in Appendix 3B-2 indicate that MeHg production and bioaccumulation at 3A-15. Please review my comments regarding Appendix 3B-2.
5. Table C does not support the statement that restoration activities have rerouted water to ENP (lines 929-930).
6. The data used to construct Figure 14 should be re-examined to: 1) eliminate negative values, and 2) determine the validity of extreme values.
7. The 2006 SFER Volume I, Chapter 3 does not state that mercury levels in largemouth bass in northern Shark (River) Slough have risen over the last few years. In fact, narrative (beginning on Page 35) and Figures 12 and 13 in Appendix 3B-1 (2007 SFER) indicate that mercury levels have not increased in largemouth bass in northern Shark River Slough over the last few years.

Interim Assessment of the Sources of Sulfur to the Everglades
Preliminary Conclusions
1. Stable sulfur isotope data are consistent with agricultural sulfur as a source of sulfate to EAA canals (lines 1010-1011). However, EAA shallow and deeper groundwater has not been subjected to the same analysis. Therefore, contribution from these sources cannot be ruled out. EAA soil also has an isotope signature similar to fertilizer and must be considered (Bates et al. 2002). Identifying the relative contribution of all sources of sulfate is necessary to establish effective management practices.

Approaches to Assessing Sulfur Sources to the Everglades
1. The statement that agricultural sulfur (a form of elemental sulfur that is 98% S0) is extensively used as a soil amendment (lines 1066-1067) may not be accurate and should be verified through discussions with EAA landowners.
2. Sulfate extracted from the upper 10 cm of soil in an active sugarcane field in the EAA has a ?34S value of 15.6‰ ... Thus, sulfur isotope (?34S) results are consistent with agricultural sulfur applications being a major contributor to sulfate content of EAA canals (lines 1073-1077). The important issue is whether current fertilizer application is the major contributor to sulfate content in the EAA canals. Sulfur application may have diminished in recent years, which is one conclusion that could be reached by evidence of less sulfur in the top 30 cm than deeper soils (Bates et al. 2002). Schueneman (2001) came to the conclusion that current fertilizer application was not the major contributor to sulfate content in the EAA canals.
3. Deep groundwater at the S-10C structure and shallower groundwater in WCA-2A had sulfate concentrations as high or higher than surface water (lines 1107-1110). Groundwater is dismissed as a contributor to surface water sulfate on the basis of ?34S values (lines 1110-1112). However, Bates et al. (2002) Figure 7 indicates considerable scatter among relatively few values for groundwater beneath the Hillsboro Canal. A ?34S signature similar to that of the canals cannot be ruled out. Bates et al. (2002) Figure 9 also indicates that shallow and deeper groundwater in WCA-2A and the ENR (STA-1W) cannot be ruled out as contributing to surface water sulfate on the basis of ?34S values.
4. The ?34S value at WCA-1 (Loxahatchee, Bates et al. 2002 Figure 7) is similar to that of agricultural fertilizer and EAA canals water, yet agricultural sulfate does not reach this location. How can this be explained?
5. Groundwater is also dismissed as a significant contributor to canal sulfate levels on the basis of sulfate/chloride ratios (lines 1132-1137). However, shallow well water in western Palm Beach County and in the EAA have sulfate/chloride ratios of 0.79 and 0.43 respectively (see references in Chen et al. 2006). Chen et al. (2006) report sulfate/chloride ratios ranging from 0.47 to 0.98 for EAA farm canal water.
6. In the EAA, ditches or wells penetrating underlying rock may significantly increase pumping requirements, and the water is so highly charged with minerals the it cannot be used for household purposes or irrigation (Jones 1948, Parker et al. 1955, Chen et al. 2006). Some wells in the EAA yield water high in sulfate (Parker et al. 1955).

Studies of the Impacts of Sulfur Loading on the Ecosystem
1. The details (e.g., design, results, analyses) of the sulfate, sulfide, phosphate, and ammonium experiment should be presented if conclusions are to remain in Appendix 3B-3 (lines 1215-1251). Based upon information provided by Dr. Orem, the sulfate dosing levels seem to be a bit high (40 to 200 mg/L) for the Everglades. Taken at face value, increases in phosphate occur at the highest dosing levels (200 mg/L) in the surface water and at porewater depths of 2 and 5 cm. By contrast, an increase in phosphate was only associated with a dose of 100 mg/L sulfate at a porewater depth of 15 cm. Sulfide appears to increase in response to increasing sulfate concentration.
2. The details (e.g., design, results, analyses) of the sulfide toxicity experiment should be presented if conclusions are to remain in Appendix 3B-3 (lines 1252-1275). Averaging five sulfate levels obscures any meaningful evaluation of Figures 18 and 19.
3. If the STAs have high sulfate and sulfide levels, why do we find a rich and abundant fauna? Similarly, why does sawgrass occur in some of the STAs?

Literature Cited

Axelrad, D.M., T.D. Atkeson, C.D. Pollman, and T. Lange. 2006. Chapter 2B: Mercury monitoring, research and environmental assessment in South Florida. South Florida Environmental Report. South Florida Water Management District and Florida Department of Environmental Protection.

Bates, A.L., W.H. Orem, J.W. Harvey, and E.C. Spiker. Tracing sources of sulfur in the Florida Everglades. Journal of Environmental Quality 31:287-299.

Chen, M., S.H. Daroub,T.A. Lang, and O.A. Diaz. 2006. Specific conductance and ionic characteristics of farm canals in the Everglades Agricultural Area. Journal of Environmental Quality 35:141-150.

Lamers, L.P., H.B.M. Tomassen, and J.G.M. Roelofs. 1998. Sulfate-induced eutrophication and phytotoxicity in freshwater wetlands. Environmental Science and Technology 32:199-205.

Parker, G.G., E. Ferguson, S.K. Love, and others. 1955. Water resources of southeastern Florida with special reference to the geology and groundwater of the Miami area. U.S. Geological Survey Water Supply Paper 1255. Reston, Virginia.

Schueneman, T.J. 2001. Characterization of sulfur sources in the EAA. Soil Crop Science Society Florida Proceedings 60:49-52.

Posted: 25 Sep 2006 12:37 PM
Originally Posted: 25 Sep 2006 12:33 PM

Subject: **Kent, CWF (2): Chapter3B; Appendices 1-3**

[Editor's note: Comments from Donald Kent are supplemental to his comments provided on 25 Sept 2006. ncc.]

D.M. KENT, COMMUNITY WATERSHED FUND

MERCURY/SULFUR INFORMATION REQUESTS
The following information referenced in either the presentations or the report is requested to facilitate independent evaluation of presenter/author statements.
1. Rumbold, Lange, Axelrad, and Atkeson manuscript titled Ecological Risk of Methylmercury in Everglades National Park, Florida, USA.
2. Pollman et al. 2005. Have changes in atmospheric deposition of mercury caused concomitant changes in mercury concentrations in largemouth bass in the Florida Everglades?
3. Report by Irv Mendelssohn to FDEP on sulfide toxicity effects on sawgrass including hydroponic study summarized in Chapter 3B (Lines 800-818) and the field mesocosm study summarized in Appendix 3B-3 (Lines 1215-1251).
4. Water quality monitoring data for ENP sampling sites represented on slide 18 of Cynithia Gilmour's presentation.
5. Details and data from the sulfur/mercury/DOC experiment represented in slide 46 of the C. Gilmour presentation.
6. Details and data from the dry/-rewet experiment represented in slides 21 and 22 of the W.H. Orem presentation.
7. Details and data from the dry/re-wet experiment represented in slides 23 and 24 of the W.H. Orem presentation.
8. Details and data from the sulfate/mercury Gambusia bioaccumulation experiment represented in slide 31 of the W.H. Orem presentation.
9. Details and data from the mesocosm sulfur toxicity experiment represented in slides 34 - 39 of the W.H. Orem presentation and Chapter 3B Figure 3B-22.
10. Details and data from the 2000 – 2003 mesocosm studies referenced in the introduction of Appendix 3B-2.
11. Details and data of the Loxahatchee study(s) referenced in the introduction of Appendix 3B-2.
12. Details of the Fe mesocosm studies summarized in Appendix 3B-2 (Section 1, B).

MERCURY/SULFUR PRESENTATION COMMENTS
Don Axelrad
1. Mercury level in fish was stated as being elevated in ENP because of sulfate in the L67 canal. Please provide data to support this statement.
2. Please make available the Rumbold, Lange, Axelrad, and Atkeson manuscript referenced in slides 8 and 9.
3. What is the background sulfate level at the site(s) referred to in slide 9?
4. The opportunity for sulfate originating in the EAA canals reaching and influencing sulfur and mercury dynamics at the North Prong Creek site seems implausible. Is there any evidence of a connection between sulfate at North Prong Creek and EAA canals? What is the salinity at this site?
5. Slide 36 refers to declining mercury and sulfate inputs to the 3A-15 site. What input sources were measured (e.g., rainfall, surface water, groundwater)? Please provide a copy of the Pollman et al. paper or a complete reference.
6. Please provide a copy of Irv Mendelssohn's hydroponic sulfide toxicity report referenced in slides 38 and 39.
7. Quantification of mercury amounts (both dry and wet) and sources is of critical importance. The cumulative data suggests that mercury bioaccumulation will occur even in the absence of sulfate enrichment.

Cynthia Gilmour

1. Slide 18 indicates water quality sampling sites in ENP. Please make data for these sites available for review.
2. Different temporal trends in sulfate concentration at adjacent structures (e.g. southeastern EAA) raises questions about the reliability of the analysis.
3. Slide 46 summarizes the results of a 2002 mesocosm experiment at 3A-15. Please provide details of this experiment to facilitate independent evaluation.
4. The presentation, while appreciated, failed to clarify questions about the reported 3A-15 connection to the L-67 Canal, the results of the 3A-15 mesocosm experiments, continued changes to the purported optimal sulfate concentration range for MeHg production, and the relationship between EAA sulfate and ENP fish mercury concentrations.

William H. Orem
1. All lands that drain into the EAA canals are not farmlands.
2. Slides 21 and 22 summarize a dry/re-wet experiment. Please provide details of this experiment to facilitate independent evaluation.
3. Slides 23 and 24 summarize a dry/re-wet experiment at 3A-15. Please provide details of this experiment to facilitate independent evaluation.
4. Slide 31 refers to a sulfate/mercury dosing study in the Loxahatchee National Wildlife Refuge. Please provide details of this experiment to facilitate independent evaluation.
5. Slides 34 – 39 refer to a mesocosm sulfur toxicity experiment. Please provide details of this experiment to facilitate independent evaluation.
6. The presentation, while appreciated, failed to clarify questions about the source of sulfate to EAA canals, the reasons for temporal changes in sulfate concentration at 3A-15, continued changes to the purported optimal sulfate concentration range for MeHg production, or sulfide toxicity effects on sawgrass.

Irving A. Mendelssohn
1. Very good presentation.
2. Sawgrass oxidizes its environment only through root tips whereas cattail oxidizes its environment along the entire root. Does sawgrass have more root tips than cattail (compensation mechanism)? Does sawgrass release more oxygen to its environment per release surface than cattail?
3. What was the sulfate load in your hydroponic study? Was new sulfate added daily to maintain levels or only once at the beginning? What was the volume of your vessels?
4. Lake Okeechobee sulfate levels (30 – 50 mg/L) would appear to be high enough to inhibit sawgrass even if the EAA were to disappear.
5. Should cattail also be inhibited by sulfate levels in the northern EPA?
6. Low soil sulfide concentrations may be the reason that no effect on species growth was observed in the field. Other explanations are possible (e.g., Everglades' biogeochemistry is unique and findings from other geographic areas/soils do not apply) and should be dismissed out of hand.
7. Please make a copy of the report available for review.

Posted: 28 Sep 2006 02:26 PM

Subject: **Chapter 6 comments [Donald DeAngelis]** USGS

Page 6-12. This is an extremely interesting study on nestling wood storks. Direct experiments of this type are useful and should be employed when possible. One additional bit of information that might be added is whether the 'control' nestlings, which were not hand-fed, were disturbed to control for the fact that the hand-fed nestlings suffered some disturbance in the process.

Line 407. What does '98 percent atom' mean? I am assuming that it means that 98% of the carbon atoms in the sodium bicarbonate are C-13 atoms.

Line 407. The '3' in CO3 should be a subscript and the '4' in H4 should be a subscript, not superscripts.

Line 411. Some acronyms in this chapter should be added to the 'Acronyms and Abbreviations' section. For example, PLFA, CHIP.

Line 434. Should be 'measured'

Pages 6-15 to 6-17. This isotope pulse study is clearly an interesting one, which could also be enhanced quite a bit by the use of simple uptake kinetics modeling.

Line 573. 'decreased' should be 'decrease'

Posted: 21 Sep 2006 10:10 AM

The 2007 Draft South Florida Environmental Report Comments Submitted by U.S. Fish and Wildlife Service, South Florida Ecological Services Office, Vero Beach, Florida September 22, 2006

Staff of the U.S. Fish and Wildlife Service South Florida Ecological Services office have reviewed several chapters of the draft report and offer the attached technical and editorial comments. The office has few comments this year because the report is a continuation of the 2006 Report, which we have reviewed more extensively in previous years. The report is regularly used as a reference by the office's scientific staff. Please direct questions to Dave Martin at (772)562-3909, ext 230.

Chapter 6: Ecology of the Everglades Protection Area (Dave Martin)

Lines 48-49. This is the first reference in the document to numerous plant species, so it would be appropriate to provide full latin names of the plants—Asimina, Ilex, Salix. In the next paragraph, common names might be helpful—gumbo-limbo, dahoon holly, wax myrtle, cocoplum, redbay (or swamp bay—the trees in the Everglades are treated as swamp bay (*Persea palustris*) in the Wunderlin and Hansen atlas of Florida vascular plants). http://www.plantatlas.usf.edu/main.asp?plantID=2064

Line 60. First use of *Lygodium microphyllum* should spell out the genus, and might provide the common name, Old World climbing fern.

Line 94. "controlled burns"—"prescribed fires" is usually the preferred term

Figures 6-1 and 6-2. These are well-presented, and the "good" and "bad" labeling is effective.

Figure 6-4. (after line 199) Awkward wording: "Hydrology in Northeast Shark River Slough in relation to peat conservation and wading bird foraging. There is no regulation schedule in this area, and tree island elevations are very high~~; thus~~, negating the need for an upper tolerance level."

Line 359. Faecal. "Fecal" is the usual US spelling.

Line 399. This paragraph is unfriendly to naïve readers. It could be worthwhile to describe the layout of the mesocosm site (one sentence would suffice) before describing the experiment.

Line 493. . . . a number of tree . . .

Line 564. This paragraph might coordinate use of scientific/latin plant names with lines 48 and 49.

Line 651. "Virginia Technological University" Presumably Virginia Polytechnic Institute and State University?

Line 800, 809. Using "monotypic" to describe vegetation consisting of a single species (cattails) is not appropriate. The word is used mainly in taxonomy, to mean a genus or family comprising a single species. "Cattail monoculture" might be clearer.

Figure 6-15 (after line 863) and Figure 6-16. Presumably the standard error is both above and below the top of each bar in this graph.

Lines 977 and 981. In keeping with the rest of the report, it would be best to use the scientific names without the authors: *Cladium jamaicence* and *Typha domingensis.* If the authors' names are used, they should not be italicized.

Line 999. Although prescribed fire is a common tool for vegetation management in the Everglades, ~~there are a~~ number of fire-related processes ~~which~~ are not well understood.

Line 1004. ~~There are~~ Two main questions drive the Fire Project study: (1) will multiple fires cause a . . .

Line 1064. It might be worthwhile to explain what a "Class B" benchmark is.

Line 1095. "Based on the data . . ." rather than "Based on this data collected"

Line 1100. The rest of the paragraph gives measurements in meters, so it would be appropriate to say 0.10 m rather than 10 cm. The work on tree islands is clearly explained and very useful.

(comments on Chapter 7B and Chapter 9 have been placed with those chapters)

Posted: 21 Sep 2006 05:39 PM

Chapter 7B RECOVER Implementation and Monitoring

Subject: Comments by FWS South Florida Office

The 2007 Draft South Florida Environmental Report
Comments Submitted by U.S. Fish and Wildlife Service,
South Florida Ecological Services Office, Vero Beach, Florida
September 22, 2006

Staff of the U.S. Fish and Wildlife Service South Florida Ecological Services office have reviewed several chapters of the draft report and offer the attached technical and editorial comments. The office has few comments this year because the report is a continuation of the 2006 Report, which we have reviewed more extensively in previous years. The report is regularly used as a reference by the office's scientific staff. Please direct questions to Dave Martin at (772)562-3909, ext 230.

Chapter 7B RECOVER Implementation and Monitoring (Liberta Scotto)

No comments. Scotto has utilized the 2006 version of this chapter regularly.

(Comments on Chapter 6 and Chapter 9 have been entered with those chapters)

Posted: 21 Sep 2006 05:42 PM

Subject: **Comments on Chapter 9 [Donald DeAngelis]** USGS

A few more acronyms from Chapter 9 should be listed in the 'Acronyms and Abbreviations' section. These include SFERTF, HID, ARS, EDRR

Line 171. Should be 'complementary'

Line 397. Should be 'these data are'

Tables 9-3 to 9-10. What are the 'three module-level questions' ?

Lines 614-615. Here and a few other places (e.g., 1623-1627), there is some overtyping (line numbers and text). It's not a major problem.

Line 1115. Should be 'swamphens'

Line 1312. Insert 'Old World climbing fern' and put '*Lygodium microphyllum*' in brackets.

Line 2012. 'has' should be 'have'

Posted: 21 Sep 2006 10:12 AM

Chapter 9: The Status of Nonindigenous Species in the South Florida Environment

Subject: Comments by FWS South Florida Office

The 2007 Draft South Florida Environmental Report
Comments Submitted by U.S. Fish and Wildlife Service,
South Florida Ecological Services Office, Vero Beach, Florida
September 22, 2006

Staff of the U.S. Fish and Wildlife Service South Florida Ecological Services office have reviewed several chapters of the draft report and offer the attached technical and editorial comments. The office has few comments this year because the report is a continuation of the 2006 Report, which we have reviewed more extensively in previous years. The report is regularly used as a reference by the office's scientific staff. Please direct questions to Dave Martin at (772)562-3909, ext 230.

Chapter 9: The Status of Nonindigenous Species in the South Florida Environment (Art Roybal)

Roybal notes that the animal part of this chapter has evolved substantially over the years.

1. Nonindigenous species are presented by occurrence within eight geographic divisions, or Modules, related to the South Florida restoration programs. Line #315 mentions module name Western Big Cypress. Figure 9-1 mentions that the nonindigenous species information in this report is organized using the terms, geographical references, and structure developed by Restoration Coordination and Verification (RECOVER). Figure 9-1 depicts a module or geographic division named Big Cypress. There seems to be frequent use of the Western Big Cypress geographic division but it is apparently not a RECOVER module according to Figure 9-1.

2. Table 9-6 mentions that it is the stoplight table for priority plant species in the Western Big Cypress Module, however, the table itself mentions Big Cypress Module.

3. There is frequent interchanging use of Western Big Cypress and Big Cypress in describing this module. The authors should consistently use one or the other in the text and tables.

4. Page 9-23, Green Iguana. Introduced iguanas have contributed to the recent candidate listing of the aboriginal pricklyapple *(Harrisia aboriginum)* under the Endangered Species Act by preying on this critically imperiled plant. This cylindrical-stemmed cactus currently occurs in coastal strand vegetation and tropical coastal hammocks on coastal islands of Sarasota, Charlotte, and Lee Counties, Florida, from Longboat Key south to Buck Key in the J.N. ''Ding'' Darling National Wildlife Refuge.

(Comments on Chapters 6 and 7B have been posted with those chapters)

Posted: 21 Sep 2006 05:44 PM

2007 South Florida Environmental Report
Chapter 9: Invasive Exotic Species in the South Florida Environment

Comments from Bill Loftus, USGS
September 2006

At a meeting this past summer, I reviewed Table 9-1 and gave the comments to Amy Ferriter. There are several fishes on this table that may have been listed because just one specimen has been collected in the module area. Whether they belong on such a list is a decision of the authors. I personally would include only species that are established or persistent. There are other fishes listed for which I am unaware of any records in the state, and which should be deleted from the table.

Other questionable records:

Channa marulius is not in BC to my knowledge
Cichlasoma managuense is in BC
C. meeki has not been collected for years anywhere here.
C. salvini is not known from SE
Colossoma spp. – where is the SE record?
Danio and Geophagus brasiliensis – no records I know of.
Check on the Hypostomus and Liposarchus records – I think most are mis-identifications of Pterygoplichthys.
Hybrid tilapias and Sarotherodon – I know of no records from here
Barbs – I know of no records.

It is my opinion that most of these species have never been collected in these modules or are known from one stray release. I would be conservative here and only list verified records of species that are established or persistent.

Channeled apple snail – the authors should contact Dr. Tim Collins at FIU who has determined the true identity of this species.

Under Information Gaps and Needs, the state and federal governments need to have research into better control methods for introduced fishes as a research funding priority. Right now there are very few options available for controlling at-large fishes in south Florida.

Line 2149 – the authors should cite the work of Loftus, Kline and Trexler presented in published abstracts at the last two GEER meetings which demonstrates that CERP actions may be exacerbating the introductions of fishes into the Everglades Protection Area.

Posted: 22 Sep 2006 10:01 PM

United States Department of the Interior

U.S. GEOLOGICAL SURVEY

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September 21, 2006

RE: Review of the 2007 South Florida Environmental Report, Chapter 10: Lake Okeechobee Protection Program – State of the Lake and Watershed

Thank you for the opportunity to review Chapter 10 of the 2007 South Florida Environmental Report. The chapter is well-written, informative, and clearly represents the diverse talent of the authors. I have the following questions and comments on this chapter:

1. Pg. 10-2, lines 47-50: I suggest plotting flow and TP concentration over time during WYs 2005 and 2006 on the same graph (perhaps as part of figure 10 -) to illustrate your point that although flows were higher in WY2006, the TP concentrations were substantially lower in WY2006 so as to produce lower overall loads.
2. Pg. 10-3, line 86: Please replace the word "Act" with "LOPA".
3. Throughout document: Please make sure that all acronyms are defined before they are used. In addition, it is not necessary to define an acronym unless it is used later in the document. There seem to be many, many acronyms – it may be beneficial to include a table of acronyms at the beginning of the document or chapter for the reader's quick reference.
4. Pg. 10-3, line 117: Delete "in" in the phrase "resuspension in during the water".
5. Pg. 10-3, lines 131-132: All of the types of littoral vegetation are listed to show a comparative change over time, except for bulrush. Do you have an estimate for areal coverage of bulrush in 1996? If not, you may want to remove it from this list.
6. Pg. 10-3, line 133: Please change "is being revised" to "was revised" since you are referring to WY2006.
7. Pg. 10-4, line 137: Remove the comma after "USACE".
8. Pg. 10-5, line 158: Consider changing the word "extremely" to "relatively".
9. Pg. 10-6, Fig. 10-1: Please also define "D" and "LD" on the map.
10. Pg. 10-7, line 191: I continue to be concerned about the accuracy of the estimate for atmospheric deposition. Wet and dry deposition can, of course, vary significantly by season and year. For example, in a "dry" year, nutrient-laden dust from the watershed may be suspended by wind and deposited on the watershed, possibly increasing the amount of phosphorus deposited on the lake. If true atmospheric deposition is determined to be much higher than expected, would the tributary load TMDL have to be reduced to meet the in-lake phosphorus concentration goal?
11. Pg. 10-7, line 202: Remove the second period after the end of the sentence.
12. Pg. 10-8, lines 236: What exactly do you mean by "full implementation of the LOPP and LOWP" – that of the goals of LOER is to make sure that LOPP and LOWP are implemented as planned, or that LOER complements the implementation of LOPP and LOWP? May need slight rewording for clarification.
13. Pg. 10-12, Table 10-1: I suggest switching the columns for 2003 and 2006 so that the most current estimates are shown second.
14. Pg. 10-13, Table 10-2: TP loads for 715 Farms and East Beach DD are shown as 0 – it appears that the loads are small (<0.01) but not 0, which is very different. I suggest showing the actual load value or stating "negligible", but not "0". Also, please change the "0" loads for South Shore/South Bay DD and Nicodemus Slough to "Not available".

15. Pg. 10-16, line 333: Change latter part of line to "The two goals of the AgNMPs were to develop whole-farm nutrient balances..."
16. Pg. 10-17, Table 10-3 and Pg. 10-21, Table 10-4: Are the annual TP reductions to the lake considered actual, projected, or ideal? Please specify in the column heading.
17. Pg. 10-21, Table 10-4: This is an excellent table.
18. Pg. 10-23, lines 503-508: FYI --- The USGS now operates a LOWP sub-basin monitoring station on Lemkin Creek next to the constructed wetland and the Sheriff's Department Rifle Range. Data from the station may be of use to SFWMD in the operation and evaluation of the urban STA.
19. Pg. 10-23, line 511: Add the word "of" between "Release" and "the".
20. Pg. 10-23, line 513: Remove the period after the word "project".
21. Pg. 10-23, lines 522-528: How is this LOPA requirement enforced when a land parcel is subdivided into smaller parcels or ranchettes? In other words, how would one allocate pre- and post-division contribution to phosphorus loads from the parcel?
22. Pg. 10-24, line 562: Remove the second comma after "phosphorus removal".
23. Pg. 10-26-27, Table 10-5 and in general: Are landowners and ranchers encouraged to restrict cattle's access to a stream as part of the farm BMP process? Cattle in streams seems to be a very common problem in the watershed, and most of the fencing that we see is inadequate (or inadequately maintained) to restrict access to a stream, which results in severe streambank erosion, direct input of nutrients to the stream, and resuspension of streambed sediments.
24. Pg. 10-28, line 640: Clarification – the USGS operates 17 sub-basin sites in the LOWP boundary.
25. Pg. 10-28, line 642: Please change "samplings" to "samples". In addition, please add that the USGS calculates weekly loads for the parameters listed.
26. Pg. 10-28, line 652: Add a space between "District" and "can".
27. Pg. 10-28, lines 660-661: Why do you think that these nine stations showed higher TP concentrations in the watershed than at the outlet to the lake? Is it due to assimilation of the TP in streambed sediments, dilution as flow increases, or some site-specific characteristic? Identification of possible reasons may be important for analyzing water quality trends in the future.
28. Pg. 10-29, line 670: Change "was" to "were".
29. Pg. 10-30, line 692: Remove the "a" before "sub-basins".
30. Pg. 10-32, Table 10-7: Excellent table. I recommend adding "(100%)" somewhere in the cell on secchi disk visibility goals since the other columns are referenced in percent.
31. Pg. 10-33, Table 10-8: Please clarify "mean lake P mass". I'm assuming this is the mass in the water column only, not including bed sediments – is this correct? Why was the net change in lake content calculated between point samples at the beginning and end of the year and not the mean lake P mass? I can understand that you want to represent the time element, but your point samples may be biased high or low depending on the circumstances at the time of the measurement. Using the mean may result in a more representative net change in lake content. Also, I believe the value for net load in 2006 is incorrect – I think it should be -29, not -329.
32. Pg. 10-34, Figure 10-8, A: Please label which symbol represents TP load and which represents surface inflow.
33. Pg. 10-34, Figure 10-8, C: Please change the angle symbol on the y-axis to "σ".
34. Pg. 10-36, Figure 10-9 and elsewhere in document: Please maintain consistency in the types of North arrow symbols used.
35. Pg. 10-36, Figure 10-10: Please label the two types of data shown.
36. Pg. 10-37, line 859: Remove the comma after "TP" at the end of this line.
37. Pg. 10-39, Table 10-9: How were "non-detects" handled for these water-quality parameters in the calculation of the mean? If there were substantial non-detects present, the mean may not be the best statistic for this evaluation. Consider using the median or a trimmed mean.
38. Pg. 10-41, Figure 10-14: Why are percentages shown for the SRP scale on the 3rd graph? Does this mean "% of TP"? Please clarify on graph. Also, make sure the order of the labels "Nearshore", "Offshore", and "Littoral" is consistent among all the graphs. Please identify or remove the 2nd y-axis on the bottom two graphs.
39. Pg. 10-43, Figure 10-16: I suggest removing this figure – it doesn't strongly support your statement on pg 10-37, lines 868-871. The text is sufficient.
40. Pg. 10-43, line 915: Change "(Table 10-10)" to "(Table 10-9)".

41. Pg. 10-44, Figure 10-17: If the SAV transect sampling is conducted monthly, why are only bi-monthly and, in some cases, tri-monthly sample results shown?
42. Pg. 10-48, lines 1003-1006: This is a very interesting study. Could the growth of eelgrass be promoted over the growth of hydrilla or other SAV types in order to more-effectively reduce flow velocities and sediment transport in the nearshore region? I look forward to reading more about this study.
43. Pg. 10-51, Table 10-11: I suggest adding reference values for mean taxa richness, mean diversity, and mean evenness for a "healthy" aquatic system.
44. Pg. 10-52, Figure 10-24: I suggest reducing the y-axis scale to a maximum of 40 or so.
45. Pg. 10-54, line 1171: Remove the "m" after "expedited".
46. Pg. 10-54, line 1194-1197: How were the invasive species treated – chemically, burned, or physically removed? Please add a brief explanation to the text.
47. Pg. 10-55, line 1199: Add a comma after "has varied".

Please contact me at the number above or by e-mail at mswood@usgs.gov if you have any questions regarding these comments. I look forward to hearing more about the progress and successes of the LOPP.

Sincerely,

Molly S. Wood, P.E.
Hydrologist

Posted: 22 Sep 2006 10:03 PM

Subject: Review by Lynn Wingard - Chapter 12 USGS

Review comments **Chapter 12: Management and Restoration of Coastal Ecosystems**

General Comments:
I am surprised that the SW coastal area is not included in the Coastal Ecosystems since Shark River Slough outflow has a significant effect on the salinity patterns of Fla Bay. In addition, the SW coast itself provides unique habitat and is a distinctive part of GEER.

Biscayne Bay section
p. 12-21, lines 539-541. Would recommend adding a reference to rising sea level contributing to shift from freshwater estuary to marine lagoon

p. 12-21, lines 570-572. Suggest rewording – reference to higher could be construed as referring to salinity not performance – "Systemwide, ~~both~~ current flow and salinity ~~performed~~ values were below restoration targets, but were ~~higher~~ closer to the targets in the wet season ~~relative to~~ than in the dry season."

p. 12-23, lines 609-610, and tables 12-2 to 12-5. This section seems to be addressing minimum flows and levels, but I do not see a specific reference to minimum flows. The mention that South Bay exceeded the target in the wet season raises the question of too much water in too short a time. It seems there should be maximum flow values (not to exceed) attached to these targets as well, so that the estuarine system does not receive too dramatic a pulse of freshwater.

Florida Bay section
p. 12-27, lines 654-655. "Most of the Bay's bottom" . . . substitute percent value for "most"

p. 12-28, lines 678-679 – which type of SAV habitat – mixed? Thalassia? Halodule? Ruppia? Since salinity thresholds are different for different species, I think it is important to specify which type.

p.12-37, lines 894-905 – Discussion about model runs and Halodule being severely impaired by hypersalinity – this doesn't seem to agree with observations following the 87-88 die-off. For example, in Johnson Key Basin vast beds of Thalassia died off during the hypersalinity events and were replaced by Halodule. Halodule was still the dominant species in much of Johnson Key Basin as of 2001. Observation indicates Thalassia is slower to return to a disturbed area and that Halodule is the opportunist species that can move in quickly and stabilize the substrate.

p. 12-38, lines 921-924. Statement that "the main goal . . . is to prevent a recurrence of the die-off". . . but isn't die-off at some scale part of the natural system, and if so, shouldn't our goal be to "let it be" – much like the NPS philosophy about forest fires? Perhaps what is meant here is to prevent anthropogenically induced die-offs, but the complex interaction of factors would make that hard to determine.

Posted: 22 Sep 2006 03:43 PM

Comments for Appendix 3B-3: ***Preliminary Assessment of Sulfur Sources, Trends and Effects in the Everglades***

General Comments:

I suggest that the entire document use standard units when dealing with concentrations. There are ***too many*** units used throughout the document. It is not clear if SO_4^{2-} concentrations are reported in mg S or SO_4^{2-}/L. It would be less confusing if concentrations were reported as either m*M* or μ*M*. This would allow the reader to make direct comparisons of SO_4^{2-} and S^{2-} concentrations in Figure 2 (Page 3-18). I do not recommend using ppb or ppm since these units are more properly used for weight/weight ratios.

Figures 1 and 3 are not properly referenced and the reference does not appear in the reference section. Further, terms in Figure 1 need to be identified: DRP (dissolved reactive phosphorus), AR (accumulation rates), etc.

Page 3-3:

Line 21: I suggest using the word parameter rather than contaminant. I'm not sure if sulfur is a contaminant in the strict definition. It is essential for plant growth, as are nitrogen and phosphorus, but can impact the ecosystem if enriched.

Line 26-27.: Last sentence of first paragraph is awkward.

Line 33: change "control of" to "controlling"

Page 3-4

Line 71: "≤1 mg/L" is used here but later in the document (Page 3-17, Line 276) "<<1 mg/L" is used. Which is correct?

Line 93: Change 1,000 ppb to 1,000 μg/L or 1 mg/L. I suggest changing to 31.2 μ*M*.

Pages 3-5 to 3-13:

I don't believe these maps are very effective with the colors used to identify different concentration levels or site densities. The colors of the circles need to be more contrasting to the colors on the map. Further, Map #6 (Page 3-10) uses mg/L for S^{2-} while the document uses μg/L or ppb (no consistency). Maps #7, #8 and #9 have no units identified.

Page 3-14

First bulleted paragraph: How valid is it to base 3 months worth of load on one data point? IS the assumption being made that SO_4^{2-} concentrations do not change over the period of 90 days? Does data exist to support this assumption? As described, the method for calculating SO_4^{2-} loads is biased and flawed.

Line 156: Flow did not trend downward at S10E (one of the S10s). Practically from May 1997, S10E did not have any flow. Presently, the structure is closed and has been closed for some

time. S6 flow has been diverted to STA2 since May 2001; therefore no flow would be recorded from the S6 structure.

Page 3-17:

Line 271: Change the word "contamination" to "enrichment" throughout document.

Line 276: In this part of the document, "pristine" parts of the ecosystem have sulfate concentrations <<1 mg/L while the same statement earlier in the document (Page 3-4, Line 71) suggests that these "pristine" areas have sulfate concentrations ≤1 mg/L. Which is correct? Are there really parts of the Everglades that are pristine, (especially since samples are collected at those sites)? I think "less impacted" is better term.

Lines 277-278: What is the intent of the sentence starting with: "Note that there is..."? Change "10X" to "10-fold".

Line 288: "sulfur-contaminate" change to "sulfur-enriched"

Last Paragraph: What is the S^{2-} concentration range required for inhibiting MeHg production?

Page 3-18:

Figure 1: Where are the two stations identified in this figure located? I've checked DBHydro and could not find any alkalinity level at 10 meq/L. Is the information presented in this figure based on one sample (and one sampling event) or is it an average? Can a proper reference be provided for this information? Need to identify abbreviations in figure caption.

Figure 2: The units need to be uniform. Are the SO_4^{2-} concentrations expressed as mg SO_4^{2-}/L or as mg S/L? Suggest that m*M* or μM be used instead. Do not try to fit a line to pore water concentrations, better to connect the points. The use of a spline for the S^{2-} plots very misleading. The WCA2A-F1 site has a non-existent maximum concentration at approximately 5 cm due to the interpolation used in generating the spline. This is also true for WCA2A-U3. How do these profiles change seasonally? Need a proper reference for the figure.

Page 3-19:

First Paragraph: Is there a reason that concentrations are now being expressed in μM in this part of the document?

Line 332: Now the SO_4^{2-} concentrations for the "pristine" Everglades are <1 mg/L. Why the inconsistency?

Page 3-20:

Line 367: Are you saying that there is no SO_4^{2-} reduction occurring in the water column? Does reduction only occur in the sediments? Have you looked at Cl^- pore water profiles to see if groundwater may be a contributing source?

Page 3-21:

Item 2: How were data with no MDL handled that used a SO_4^{2-} method with a MDL greater than 0.1 mg/L? Historically, MDL for SO_4^{2-} for SFWMD changed from 2 mg/L to 0.1 mg/L. What is the justification to set these MDLs that are greater than 0.1 to 0.1 mg/L?

Page 3-22:

Line 461: Contradictory to earlier concentrations of SO_4^{2-} in "pristine" Everglades.

Last Paragraph: Temporal variability is mentioned. Were seasonal concentrations compared for the northern and southern portions of the Everglades?

Lines 529 – 540: Based on data in DBHydro for the period 1995-2000, Lake Okeechobee SO_4^{2-} concentrations ranged from <1 to 118 mg/L. Line 532, change "ware" to "were". DBHydro SO_4^{2-} data for the period from 1995 to 2000 averages <15 mg/L, at S154 the average is 49 mg/L, S191 the average is 33 mg/L and FECSR78 (Fisheating Creek) the average is <10 mg/L. Where did 50-500 mg/l range come from?

Lines 546-551: What were the SO_4^{2-} atmospheric loads to the South Florida system on an annual basis? How did these loads compare with surface water loads?

Page 3-25:

Figure 3: What period do the data presented in this figure represent? Can a more appropriate reference and/or monitoring program be provided rather than just USGS?

Page 3-26:

Lines 634-637: Why is the average flow for the quarter used to calculate load? Should you be using the total flow? Was the flow data integrated around the SO_4^{2-} sampling date or was the flow prior to the sampling event used to calculate load? How valid are any trends based on quarterly data extrapolated to annual loads?

Pages 3-28 through 3-32, 3-34:

Figures 4-9: Figures are too busy. Since the sampling is disjointed bar graphs are preferable to line plots. Why report the loads in megagrams? Report loads as metric tons.

Page 3-54:

First Bulleted Conclusion: Change "contamination" to "enrichment". The last sentence suggests that SO_4^{2-} levels in affected areas could less than 2-3 orders of magnitude, is this true?

Approaches to Assessing Sulfur Sources to the Everglades:

While Figure 15 for WCA2 demonstrates well areas where SO_4^{2-} levels are enriched, can you provide an explanation as to why SO_4^{2-} levels in the Miami Canal for the WCA3 and ENP transect drop from 59.4 mg/L in the EAA to 12.4 mg/L in the marsh? Figure 16 does not have $\delta^{34}S$ for WCA3, how would you expect the data for this region look like once plotted? Are there any plans to collect $\delta^{34}S$ for this region? Can you provide an explanation why WCA1 (a rain driven system) exhibits a similar $\delta^{34}S$ range as the EAA yet has lower SO_4^{2-} concentrations?

Based on Bates et. al. (2002), Lake Okeechobee and Kissimmee River exhibit similar $\delta^{34}S$ levels as the EAA. I suggest that a table be provided showing $\delta^{34}S$ from different sources (fertilizers, seawater, lakes, and/or rivers). This would provide a better perspective regarding SO_4^{2-} and $\delta^{34}S$ in the South Florida environment.

Final Thoughts:

The δ34S data presented in this document appears to be preliminary (covering 1995-199) and has not been updated since the Bates et al (2002) paper was published. Based on what is presented in the document, the conclusions are compelling. However, more information will be required to confirm the conclusions presented.

A section discussing the reactions leading to the production of S^{2-} through the oxidation of organic matter would be helpful:

Organic matter oxidation under oxic conditions (after Froelich et. al. 1979):

$$(CH_2O)_{106}(NH_3)H_3PO_4 + 138O_2 \Leftrightarrow 106CO_2 + 16\,HNO_3 + H_3PO_4 + 122H_2O$$

Organic matter oxidation under anoxic conditions (after Froelich et. al. 1979):

$$(CH_2O)_{106}(NH_3)H_3PO_4 + 94.4NO_3 \Leftrightarrow 106CO_2 + 55.2\,N_2 + H_3PO_4 + 177.2H_2O$$
$$(CH_2O)_{106}(NH_3)H_3PO_4 + 53SO_4 \Leftrightarrow 106CO_2 + 16\,NH_3 + H_3PO_4 + 53S^{2-} + 177.2H_2O$$

Page 3-18 of the report has Redox (E_h) potential for two sites across a north-south transect (Figure 1). Based on work done by Takai and Kamura (1966), Turner and Patrick (1968), Ponnamperund (1972), Presley and Trefry (1980) and others, oxygen disappears in pore water under +250 mV with SO_4^{2-} reduction occurring below -150 mV. That could be one reason the "pristine" area has low sulfides in pore water (as represented in this figure). Further, have the authors looked at the effect of organic matter deposition on S^{2-} production? It seems that the organic carbon accumulation rate at the northern site (Figure 1) is 20 times higher than at the southern site. That in conjunction with SO_4^{2-} levels should have a large impact on S^{2-} production.

Comments by
Posted for:

Nenad Iricanin, Ph.D.
Lead Environmental Scientist
Water Quality Assessment Division

Posted: 18 Oct 2006 02:22 PM