Appendix 1-2: Peer-Review Panel and Public Comments on Draft Volume I

During September–November 2013, comments from the peer-review panel were provided on the 2014 SFER WebBoard (www.sfwmd.gov/webboards), and public comments were also received. SFER panel comments were prepared under separate Purchase Orders with the South Florida Water Management District. With the exception of spell check and reformatting some information for better readability, this appendix was not edited by the SFER production staff and appears verbatim as posted on the WebBoard.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3A

Level of Panel Review: Accountability
Reviewer: V. Novotny (AA)

WebBoard-Posted: 10/1/13

Accountability Review *for chapters and sections that are of a more routine nature, and deal with cross-cutting themes and content Questions:*
– Does the draft document present a defensible account of data and findings for the areas being addressed that is complete and appropriate?
-Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report?
-Are findings linked to management goals and objectives?

This chapter provides more or less routine but defensible assessment and analyses of water quality in the Everglades Protection Area (EPA) and in the Everglades National Park (ENP) during the Water Year (WY) 2013 which started May 1, 2012 and ended April 30, 2013. The EPA areas are the National Wildlife Refuge Area known also as Water Conservation Area 1 (WCA-1), and Water Conservation Areas 2 and 3. The chapter concludes that, in general, throughout the Everglades the water quality standards were met. In total, the chapter evaluated 10 water quality parameters out of 109 monitored within the EPA and a group of pesticides. The chapter provided summaries for Dissolved Oxygen, (DO), alkalinity, pH, conductance, turbidity and unionized ammonia as well as water quality status of phosphorus (total and orthophosphate), nitrogen, and pesticides throughout the Everglades. Essentially, the authors of the chapter used the same format (and title) as in the last 2012 report and reported similar results.

Similarly to the previous WY 2012 report, this WY report presents positive trends and progress in water quality improvement. However, several parameters were classified because of observed exceedances of DO, alkalinity, pH, and specific conductance. No problems were reported for turbidity, and unionized ammonia. Unlike in the previous year, sulfate loads and concentrations were not included because they received extensive coverage in Chapter 3B. The report is consistent with previous reports.

Generally, phosphorus concentrations throughout the Everglades were improving and the very stringent standards of 10 μg of P/L was mostly met as a geometric annual mean in the interior of the three conservation areas and ENP. However, geometric means the inflow into the Refuge area, which is the most northern area, were greater than the 10 μg of P/L criterion. The report again revealed, that in the southern portions of the park (WCA-3 and ENP), atmospheric P sources are responsible for the largest P loads into the system. Similarly, nitrogen sources are also primarily atmospheric. The authors did not attempt to link these loads to any specific global or regional source.

No pesticide or pesticide byproducts exceeded their guidance values. Herein, the maximum measured values were compared with the appropriate guidance priority criteria issued by the US Environmental Protection Agency and adopted by Florida. Few measurements were above the detectable levels but still within the acceptable limits. The toxic metal data analyzed in previous years were not presented. Apparently it was concluded that toxic metals do not represent a problem.

On page 3A-3 the purpose and goals of the chapter were presented as follows:

1. Summarize areas and times where water quality criteria are not being met, and indicate trends in excursions over space and time.
2. Discuss factors contributing to excursions from water quality criteria, and provide an evaluation of natural background conditions where existing standards may not be appropriate.
3. Present an updated review of pesticide and priority pollutant data made available during WY2013.
4. Present a preliminary TP criterion achievement assessment for different areas within the EPA for the most recent five-year period (i.e., WY2009–WY2013).
5. Summarize phosphorus and nitrogen concentrations measured in surface waters within different portions of the EPA.
6. Summarize the flow and phosphorus loads entering different portions of the EPA during WY2013, and describe spatial and temporal trends observed.
7. Describe and discuss factors contributing to any spatial and temporal trends observed.

Pages 3A-4 to 3-9 describe the monitoring program and analyses. As in the previous years the methodologies and quality controls were adequately described. The District has a very dense monitoring system of monitoring locations and collected a large number of samples to perform a representative detailed analyses and synthesis.

Page 3A-10 To perform trend analyses and comparisons with the past reports, the authors of the chapter organized the current and past data into four periods: (1) Historical pre-BMP period (1974-194) as baseline; (2) Intermediate (1994-2004); Period of Phase II BMP.STA implementation (2005-2012); and (4) WY 2013. From this information it can be deduced that the abatement period has been relatively short (less than 10 years); hence, it is satisfactory that the results of the abatement are becoming evident and significant. However, at this time it is not possible to differentiate between the effects of ongoing implementation of BMP/STA practices and the effects of meteorological facts, namely that the recent years were relatively dry with the exception of Hurricane Isaac.

Specific Comments

Page 3A-11 presents the list of parameter's that are generally monitored by the district. It was stated that metals may not be a problem and were not analyzed in the WY2013. To make the argument stronger, hardness could be added to the list of analyzed parameters because the water quality criteria for key metals (excluding mercury which is extensively covered in Chapter 3B) and their toxicity are related to hardness. If the waters of the Everglades are soft (they seem not to be) than even small concentrations of toxic metals could represent the problem.

Page 3A-14 discusses and presents the effects of Hurricane Isaac. Generally, hurricanes are disruptive and upset the chemical balance of the system, e.g., by stirring sediments and releasing some pollutants. Table 3A-1shows some elevated values of some parameters at the diversion structures but the subsequent analyses of long term trends did not reveal that the system was upset and needed a long recovery as it happened with some previous hurricane events.

Page 3A-14. The authors mixed US and SI (metric) units in one sentence (acre-feet, mt=metric ton, and mg/L). The latter two units are well established SI units while acre-ft is unknown outside of the US and now most universities and government agencies switched to metric m3. Please, provide metric conversion to acre-ft on this page or change to m^3 throughout the chapter.

Pages 3A15-17 present tabular summaries of water quality parameter excursions for four phases, starting with 1974. Generally, the number of excursions are going down for pH, ammonia, specific conductance and alkalinity. This sounds positive but there are some underlying issues. The biggest problem is DO that is judged differently for inflows and outflows where Florida Class III standard is applied (minimum DO 5 mg/L) to inflows/outflows while interiors of the management areas are judged according to a site specific harmonic function related to the time of year and temperature. This led to an obvious paradoxical dichotomy which was addressed by this reviewer at least twice in the previous reviews. In the past years it was agreed, however, that the authors of the chapter are obliged to use the standing legislature approved criteria for the assessment and extensive discussions on adequacy or reality of the standing model that was accepted by the authorities seemed to be pointless. But the dichotomy became obvious again as presented below. The evaluations and statistics of the number of excursions presented in Table 3A-2 on DO excursions revealed miserable compliance with the Class III standards at the points where it was applied, i.e., inflow and outflow monitoring points. At these points the DO standard was violated up to 88% of the time and the table indicated significant worsening of the frequency of violations between the baseline Phase I and WY2013. On the other hand, the harmonic criterion for the interiors was almost never violated but there is no reason for celebration. It was pointed out in the previous reviews that the harmonic criterion is extremely lenient and allows DO to drop to very low levels approaching lethal levels (there have been periodic fish kills reported by the media in the past). Theoretically, the water quality DO concentrations in the outflow from the area are the same as those in the interior. So, until it is recognized that the present criteria are either not applicable to the Everglades (Class III criteria) or are so lenient that they do not protect much (criteria for interior areas), it is indeed pointless to make any judgment on DO conditions except report the situation and continue to analyze trends as it was done adequately and professionally in this chapter. As in the past, this comment on the problem with the DO assessment is not aimed at the authors; it is directed towards the agencies that developed and/or accepted the harmonic formula and its application to averaged DO concentrations and now may be forcing the authors and the District to use Class III criteria for a marsh system that is naturally dystrophic. Without solving the inappropriate standards problem, DO assessments and abatement programs will go nowhere. The situation may be helped by the fact the low DOs are linked to eutrophication which is controlled by the TP standard. However, the TP standard is mostly met throughout the WCA 2, WCA 3 and ENP, yet DO problems persist and appear to be getting worse in some areas.

Page 3B-19 line 477. CaCO3 is not alkalinity. Alkalinity is a sum of OH-, HCO3- and CO3 = ions but it is often expressed as CaCO3 equivalent. At pH levels typical for the Florida Everglades and other waters, CO3= and OH- components may be negligible and alkalinity is mostly bicarbonate. The reviewer agrees that occasional low pH and alkalinity are mostly natural. If the inflow to the refuge interior is dominated by atmospheric inputs, it is obvious that the refuge and EPA water will have naturally low pH and alkalinity. Typically, natural unpolluted rainfall has pH around 5.7 and has very low alkalinity. If worries about the excursions of the two (interrelated) parameters persist, a Use Attainability Analysis would most likely fix the problem and adjust the standards or identify an anthropogenic cause of the excursion that could be remedied by a TMDL.

Page 3A-20 – Problems with specific conductance. The freshwater groundwater zone in Florida is known to be relatively thin and has been depleted by anthropogenic overdrafts causing intrusion of salt water into groundwater zone drainage canals. This has not been addressed; however, it appears that the situation is improving and the excursions are decreasing.

Pages 3A-25to 3A-35 evaluated phosphorus. Phosphorus has been recognized as a limiting nutrient for the Everglades (EPA). To protect the biota and limit potential eutrophication a criterion of 10 μg/L of Total Phosphorus was designated for the areas. Phosphorus drives the eutrophication processes in the Everglades (as well as in Lake Okeechobee and other important water bodies) and is also linked to DO. The most important outcome of this chapter is continuing evidence that the phosphorus concentrations thoughout the Everglades system water bodies are decreasing and the geometric means of concentrations are at or below the relatively stringent standard of 10 μg/L.

The chapter justifiably speaks about "dramatic" decreases which are good but one could ask for an explanation when considering that the BMP/STA abatement program is not fully finished. As a matter of fact the chapters on nonpoint pollution in the current and last year's reports speak about BMPs in progress and being planned. Is the weather helping more than the anthropogenic improvements? Note that on pages 3A-32 and 33 atmospheric deposition was identified as the major source of TP (and by the same reasoning ortho-phosphate) loads to the lower Everglades system. This is partially explained in Table 3A-6 on Page 3A-34 and Figure 3A-9 in page 3A-35 which show that the annual P loads "dramatically" decreased from the base line period to the WY 2013 in the Refuge and conservation areas (anthropogenic improvements?) but remained steady, about 11 SI tons/year, in the ENP protected area. Evidently, the conservation areas have long been attenuating the TP loads. And this is good news for protection of the ENP.

Figure 3A-8 shows that Total P standard of 10 μg/L for annual geometric means within the Everglades areas is generally met in the EPA Park and WC-2 and WC-3 and very close to being met in the Refuge Area WC-1. The situation that years ago seemed relatively serious, almost hopeless, is beginning to look like the BPM/STA actions thoughout the watershed seem to be working and more improvement may be coming because there usually is a lag time between the implementation and response in water quality. However, these positive trends should not result in complacency because it is still not clear whether or not these positive changes are caused by drought. Furthermore, the concentration maxima, while also dramatically decreasing, are still in WY2013 higher than the standard in the interior and, obviously, more so in the inflow and outflow. It is not clear whether these deviations are part of the statistical distribution of all samples or these higher values indicate areas that are more impaired. The map on Figure 3A-9 indicates that the areas of higher TP concentrations are very small and isolated only to outflow/inflow (the outflow from one area is the inflow to the next downstream) and apparently to some canals.

There is an unexplained anomaly in Table 3A-4 on Page 3A-28. The outflow and rim TP concentrations in the Refuge area for most of the time (Phases II, III, IV) are greater than those in the interior. The only explanation could be some kind of shortcutting of the inflow into the outflow and rim. Is there an explanation?

On Page 3A-29 lines 775-777 the authors seem to be surprised that the interior TP concentrations are smaller than the inflow. This is always so for settling and/or degradable compounds like phosphorus existing predominantly in a particulate form.

Pages 3A-35 to 3A-38 cover orthophosphate concentrations and loads. OP is a bioavailable mostly soluble part of TP, hence, it exhibited the same decreasing trends in the conservation areas and more or less steady state in the ENP area. OP concentrations currently are very low and since no criterion is available for assessing the impacts the conclusions would be the same as those for TP; i.e., the situation is improving.

Pages 3A-39 to 3A-43 present the data statistics and trend for Total Nitrogen. The District collects and analyzes nitrogen separately for TKN (Total Kjedahl Nitrogen = organic N and Ammonium) and nitrate/nitrite N and the Total N is then their summation but reports only Total N. There is now new and strong evidence that TKN and nitrate affects the eutrophication process differently. Both TKN and nitrite/nitrate are nutrients that could stimulate eutrophication; however, in the Everglades system phosphorus seems to be clearly the limiting nutrient. However, some overlooked older and new literature references noticed phosphorus release from the sediments is suppressed in waters with higher nitrate content and, as a result, the water body, be it an impoundment or coastal wetland fen, would not become highly eutrophic or hypereutrophic based on the nutrient loading (Andersen, 1982; Hemond and Lin, 2010; Lehman, 2011) as long as the nitrate content in the water above the sediment and top sediment layer is not exhausted. Also Lehman (2011) documented and concluded that phosphate and iron are reduced and released only when both oxygen and nitrate are depleted. In another case study of the nitrate effect on eutrophication of coastal wetland fens (Lucassen et al., 2004), eutrophication was prevented by high nitrate loads in groundwater. Liška and Duras (2011) reported the same phenomenon in a small headwater in the Czech Republic and, similarly, Lehman (2011) on an impoundment in Michigan. Selig and Schlungaum (2003) observed two dimictic lakes and found sediments released ammonium and phosphate only when both oxygen and nitrates were absent. Andersen (1982), in a study of Danish eutrophic lakes, noticed phosphate release from sediments occurring if NO3- -N concentrations were less than 0.1 mg/L and no release into the anoxic hypolimnion if NO3 - - N was greater than 1 mg/L. It is therefore recommended that the authors should provide separate information for TN and nitrate. Fortunately or unfortunately Figure 3A-13 shows a close correlation of Total Nitrogen to the concentration of the total organic carbon indication that most of the TN is organic (KN) and ammonia is the nitrogen compound released into sediment by decomposition of the organic matter in the sediment.

With respect to the trend of the TN loads, it appears that the loads to the conservation areas over the last 25 years remain fairly steady (Figure 3A-12, page 3A-42 and Table 3A-10 on page 3A-44). Atmospheric deposition is a significant source of the TN but it is highly variable from very small values in the remote areas of the Everglades to large deposition rates in urban zones. It should be pointed out that traffic is a very important source of atmospheric deposition of NOx which is obviously related to the traffic density of the highways near or transecting the Everglades and car mileage (Novotny, 2002). With improved mileage mandated by the US EPA, the NOx emission rate will be reduced.

Closing comments

This chapter as written fulfilled its purpose, i.e. it was well written and presented good statistical analyses of concentrations, trends and sources of several key pollutants. The chapter did not venture into identifying quantitatively causative factors for the problems; hence, it does not provide adequate information for those developing or being interested in abatement of the problems.

The chapter pointed out obvious long lasting problems and controversies with the DO concentrations in the Everglades which are partially natural, partially caused by anthropogenic sources of nutrients to the system but did not acknowledge that the standards are inappropriate.
However, it was agreed in previous discussions that the authors are bound by the standing standards no matter how inappropriate they are. It appeared that the DO problem was not improving but to what degree it is caused by the natural dystrophic nature of the marshes and what is caused by more intensive decomposition processes, sediment oxygen demand and algal respiration stimulated by anthropogenic nutrient inputs is still not clear but, apparently this was not the task to be addressed by this annual report. The reduction of the annual means of the Total

Phosphorus loads is promising, especially when considering that more of the BMP/STA controls are being planned. Generally, most BMPs reduce both nitrogen and phosphorus.

The authors should also clear up some inconsistencies with units and provide conversion factors since they intermixed US units (mostly unknown to the international audience reading the report) and metric SI units. It was noted that most units in the chapter are metric, as they should be.

Helpful references

Andersen, J.M. (1982) Effect of nitrate concentrations in lake water on phosphate release from the sediment, *Water Research* 16(7): 1119-1126.

Hemond, H.F. and K. Lin (2010) Nitrate suppresses internal phosphorus loading in an eutrophic lake, *Water Research* 44:3645-3650.

Lehman, J.T. (2011) Nuisance cyanobacteria in an urbanizing impoundment: interacting internal phosphorus loading, nitrogen metabolism, and polymixis, *Hydrobiologia* 661: 277-287.

Liška, M., and J. Duras (2011) VN Švihov – monitoring water quality in the catchment and its results (in Czech with English abstract) *Vodní Hospodářsví (Water Management)* No 3:93-98

Lucassen, E.C.H.E.T., A.J.P. Smolders, A.L. Van der Salm, and J.G. Roelofs (2004) High groundwater nitrate concentrations inhibit eutrophication of sulphate-rich freshwater wetlands, *Biogeochemistry*, 67:249-267.

Novotny, V. (2002) *WATER QUALITY – Diffuse pollution and watershed management*, J. Wiley Hoboken, NJ.

Selig, U. and G. Schlungbaum (2003) Characterization and quantification of phosphorus release from profundal bottom sediments in two dimictic lakes during summer stratification, *J. Limnol.* 62(2):151-162.

WebBoard-Posted: 11/4/13

This chapter described water quality of the Everglades system that include the National Wildlife Refuge Area known also as Water Conservation Area 1 (WCA-1), and Water Conservation Areas 2 and 3. This chapter was well written and presented good statistical analyses of concentrations, trends and sources of several key pollutants. The authors satisfactorily addressed the reviewer's comments and agreed to incorporate the suggestions into the final text of the chapter. Specifically:

Comment #1 – Both the reviewer and authors agreed that metal concentrations may not be a problem in the system based on past years' observations. The authors will add information on hardness that affects the metal toxicity and determines the magnitude of the standards to next year's report.

Comment #2 – The authors confirmed that Hurricane Isaac did not significantly disrupt the Everglades system and water quality.

Comment # 3 – Conversion from acre-ft to m^3 will be added throughout the chapter.

Comment #4 – Authors and the reviewer extensively discussed the problem with DO standards. These discussions repeat every year because the site specific harmonic standard allows at this time very low DO concentrations that normally would be considered harmful to fish. The new caveat is the use of Florida Class III (freshwater) water quality DO standard for outflows while far more lenient site specific harmonic criterion is applied to the interior areas that are naturally dystrophic (have natural low DOs). Physically, in absence of some massive mechanical aeration of the outflow, the outflow concentrations are the same as those in the interior. Consequently, compliance with the freshwater Class III standards in the outflow is miserable. The authors pointed out that recently (September 2013) the US EPA approved a revision of the statewide marine and freshwater criterion that takes into account location (bio-region) and time of the day. Obviously, at this time it not clear whether or not this criterion would solve the problem of the very low DOs in the Everglades system. Addressing this problem will be included in bullet "Key Findings and Recommendation" closure below.

Comment # 5 – It was agreed that the Everglades, dominated by atmospheric rainfall, have occasionally low pH and alkalinity that leads to the violation of pH and alkalinity criteria. The reviewer and the authors agreed that a UAA and resulting site specific criterion acknowledging the natural conditions would solve the problem. However, the district apparently put a low priority on solving this problem. This will be also added as a future item to "Key Findings and Recommendations".

Comments # 6 – Alleged problems with specific conductance were satisfactorily explained by the authors and the situation which is not serious is improving.

Comments # 7, 8 and 9 – Phosphorus evaluation. The most important finding of the chapter is the fact that the phosphorus loads to the system are decreasing and the stringent geometric mean limit of 10 μ P/L is generally met in the ENP and in most of the other conservation areas. The authors also explained the anomaly of the outflow and rim of the Refuge area that had higher P concentrations than the interior of the area from which the outflow originates.

Comment #11 – Extensive discussion was devoted to the impact of nitrogen on the Everglades system. Nitrogen is the second nutrient that can accelerate eutrophication but the system is clearly phosphorus limited and ongoing efforts to reduce phosphorus loads have already resulted in significant reductions of P concentrations. The systems are most likely naturally dystrophic, i.e., they exhibit low dissolved oxygen concentrations and are incapable to meet the Class III Florida DO standard at the outflow from the management areas. The reviewer pointed out that there are some cases where nitrates in the water column and upper sediment, similarly to oxygen, can partially (and temporarily) block P release from anaerobic sediments; however, the authors pointed out that nitrate levels in the system are very low. Adding nitrate, as it has been done in some marine and freshwater systems to control P releases from sediments would be highly inappropriate. Nevertheless, the authors agreed to look into these often overlooked effects of nitrate, both positive and negative, in the future.

Key Findings and Recommendations

- This chapter (3A) provides routine but professional, realistic and quality assessment of the progress in improving water quality and reducing nutrient loads in the National Wildlife Refuge Area known also as Water Conservation Area 1 (WCA-1), and Water Conservation Areas 2 and 3.
- The phosphorus loads to the system are decreasing and the stringent geometric mean limit of 10 μ P/L is generally met. The current improved status and trend should not give the agency a reason for complacency, the problems with low dissolved oxygen and dissolved oxygen standards may continue and the local impairment of the trophic status in the WCA 2 and 3 must be reduced.
- The agencies (both regulatory and the district) have clearly a problem with distinguishing natural and anthropogenic problems within the system, namely, those related to low dissolved oxygen, pH and alkalinity. It looks that having two vastly different DO standards applied to the internal waters and the rim and outflow areas is confusing and to some degree inappropriate. For example, physically the water quality of the outflow and interior are the same yet one standard says that the status is excellent and the other classifies it as miserably violating. The agencies (and that also includes USEPA) must

cooperate and develop UAA site specific standards that would be both protective and recognize the natural specificities of these extremely important natural resource areas.

- A similar but not as controversial problem is pH and alkalinity. Because the precipitation is the largest input into the system, the waters on occasion may have naturally low pH and alkalinity violating the common water quality standards which to some may give the wrong impression of a problem. The district seems to be aware of this dichotomy but put a low priority on development of a site specific standard. This problem should be corrected.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3B

Level of Panel Review: Technical
Reviewers: O. Stein (AA); V. Novotny (A)

WebBoard-Posted: 9/29/13

Technical Questions:

1. Are the findings and conclusions supported by "best available information," or are there gaps or flaws in the information presented in the document?

2. Are there other interpretations of the data and other available information that should be considered by the authors and presented to decision makers? If so, the panel shall identify specific studies that should be addressed or available data to support alternative findings.

The review is divided in to several sections. First are several broad questions and comments regarding the interpretation of reported results followed by relatively minor questions about specific sections, sentences, phrases, etc. This is followed by comments on figures and tables, editorial suggestions and lastly summary and recommendation comments.

Broad questions and comments that should be addressed
Each year the South Florida Water Management District (District) prepares its Environmental Report. In many past years, the report is comprehensive and includes both assessment and interpretation of trends in the data. Specifically for this chapter these data include fish tissue contamination by mercury, mercury deposition levels, and sulfate levels that are alleged to cause methyl mercury evolution and fish tissue contamination, including factors and processes that link mercury and sulfate to fish tissue contamination and the effects on human health.

Last year's report (2013) updated the status of mercury and sulfur monitoring in the Everglades and pointed to the serious problem of high and unacceptable methyl mercury contamination in higher trophic level organisms, specifically largemouth bass (LMB), alligators, Florida panther, and fish eating birds and exceedances of the safety criteria for human consumption. The Everglades have the highest methyl mercury contamination of fish and other biota in Florida. The last year also provided cutting edge scientific information on how the formation of methyl mercury (MeHg) is related to the biological activity of sulfur reducing bacteria (SRB) which reduces sulfate to sulfide. This reduction process releases immobile iron and aluminum bound phosphorus into pore water as phosphate and also is driving eutrophication, increasing dissolved organic matter, which is also one of the factors affecting the formation of MeHg. The 2013 report presented an extensive synthesis compiled from sub-reports of multiple authors who are members of a number of eminent institutes and agencies along with the team from the SFWMD.
This 2014 report is much less comprehensive focusing primarily on data and trend statistics without venturing into documenting the scientific hypotheses, focusing mainly on monitoring results of sulfates and mercury and not necessarily on the synthesis.

The sources of sulfate include: (1) inputs from atmospheric deposition, (2) inputs of connate seawater (connate water= water entrapped in sedimentary rocks during the time of their formation) into canals, and (3) export of sulfate from the Everglades Agricultural Area (EAA).

One important event that occurred in 2013 and could affect the future status of the mercury-sulfate problems is the ratification and finalization by the Florida legislation of the Florida's Total Maximum Daily Load (TMDL) program for these pollutants (page 3B-2). This TMDL has not yet been posted nor made available to the reviewers.

Appendix 3B-1 to this chapter contains an interim report of a study authorized by a cooperative agreement between the District and the Florida Department of Environmental Protection (FDEP) to evaluate factors affecting mercury methylation in Florida marshes. The study is finding that *in situ* sources of sulfate (rainfall, groundwater, internal recycling of reduced sulfur species) in the Everglades may be adequate to support environmentally detrimental levels of methylation by sulfur reducing bacteria (SRB since $SO_4^=$ levels as low as 0.3 mg L^{-1} without added Hg^{2+} supported modest MeHg accumulation in the laboratory incubation. Although this report might have a tremendous effect on attempting to control and manage mercury contamination of fish and other species, it was not peer reviewed by the agencies and will not be further evaluated and mentioned in this review.

Largemouth Bass (LMB) Evaluation (Pages 3B-4 to 3B-10) contains evaluation of mercury contamination of Largemouth Bass (LMB). 339 LMB species were collected from 13 locations within the Everglades Protection Areas (EPA) and Everglades National Park (ENP). This year the report repeats the same finding, i.e., the largest MMB tissue concentrations occur in the ENP which has the lowest Hg and $SO_4^=$ inputs and concentrations (Table 3B-1). Higher concentrations of Hg in LMB were also found in WCA 2 and WCA 3 areas. Last year's review commented on the important discovery by the research team that the formation of MeHg is maximal in the "Goldilocks' window" of sulfate concentration between 1 to 5-10 mg/L and also depends on the concentration of *aromatic* dissolved organic carbon. It was found ironically that MeHg was produced in mostly oligotrophic zones of the EPA in areas which have the lowest sulfate concentrations. The dilemma is that increasing the sulfate concentrations might lead to more phosphorus release from the sediment.

What this means is that sulfate concentrations greater than 10 to 20 mg/L actually suppress the MeHg formation (based also on the dissolved organic carbon) and reduction of the sulfate concentrations might lead to extending the zones of MeHg formation northward. This review and others proposed establishing a site specific criterion of MeHg creating compounds based on the "Goldilocks' window" which was not mentioned nor accepted in this year's report.

Regarding the trends in LMB, there has been very little change after year 2000 (page 3B-8) with concentration medians varying between 0.3 to 0.55 mg/kg. Human health risks with LMB consumption required recommending limitations or even no consumption of certain fish species throughout the EPA and ENP. However, these recommendations appear to be only advisory.

Mosquitofish and Sunfish monitoring statistics and problems are covered on pages 3B11 to 3B17. The report states that there was no significant variation or a trend in the fish tissue contamination (Figure 3B-6) but the plot can be also interpreted that between 2000 and 2006 there was a significant drop and more or less leveling off or a slight increase thereafter. Interestingly, unlike the concentrations for LMB, the highest fish concentrations did not occur in the ENP. On the other hand, sunfish concentrations in the ENP were much larger than in the other three investigated areas of the EPA. Overall, mosquito and sunfish concentrations exceeded the federal criteria for wildlife protection.

Mercury sources are described and analyzed on pages 3B-18 to 3B-23. At the beginning of the chapter, reference is made to the state TMDL program requiring a reduction of 86% of mercury loads from all sources (local, regional and global) to a level of 23 kg of Hg/yr. This appears to be an insurmountable goal when considering that most of the Hg inputs are atmospheric and global, as stated throughout the chapter and in the previous report, and Figure 3B-11 shows no changes in the atmospheric deposition over the years. However, as stated previously, the review will not criticize the TMDL because the document is not available.

Sulfur sources and trends are then presented in the remainder of the chapter (pages 3B-24 to 3B-36). In the beginning of the review it should be said that, unlike the 2013 report, this chapter is not attempting to quantify causative relationships between sulfur concentrations and loads and methyl mercury and its contamination of fish. The author of this section on page 3B-24 even states that "the role SO_4^{2-} plays in the sulfur biochemical is relatively unknown". This is surprising since the 2013 Chapter 3B contained a wealth of new cutting edge scientific hypotheses and scientific explanations with data of the sulfate effect on MeHg formation. This year's chapter simply covers sulfate concentrations and loads and their trends. The lack of recognition of the information relayed last year seems to be a glaring omission in this year's report and the reviewers concur that comparatively, this report is scientifically inadequate. We fully expected information on the progress of the methylation research would have been provided. Table 3B-5 and Figure 3B-13 indicate that there was a decrease of sulfate concentrations in about 2/3 of areas and measuring points. As eluded in the previous portions of this review and in last year's review (2013), the decrease of the concentrations that are above 10 mg/l may increase the chances of more MeHg contamination in fish on one side, but less P release from the sediment.
The authors of this section on pages 3b-30 (bottom) and 3B-31 (top) expressed reservations against the site specific standards for sulfate and questioned the proposal of establishing a goal of 1 mg SO_4^{2-}/L as a standard. In their arguments, they assumed a linear relationship between the sulfate concentrations and harm done by the MeHg formation. Again, this contradicts last year's finding of the research that the relationship between MeHg formation and sulfate (also dissolved aromatic organics) is distinct and non linear. A linear relationship would imply that the areas of higher sulfate levels (> 10 mg/L) would have an increasing MeHg problem which was found not to be true. The highest problems are in zones of very low sulfate concentration, so the concept of having 1 mg/l as a standard is nonsense. The cutting edge research work reported last year should not be dismissed and should be continued, and, eventually, in the near future result in a site specific standard and abatement plan. The wealth of information presented in the 2013 report on the linkage of sulfate concentrations to MeHg was overwhelming but it may take some time before it gets into the literature and will result in a standard.

In conclusion, this chapter is an off year presentation of data, statistics, and trends. It does not attempt to quantitatively identify the causative factors and linkages between MeHg and sulfates. As a report on concentrations, loads and trends it may be adequate. The conclusion that may be drawn from the chapter is that not much is going on, the main source of mercury, atmospheric deposition, is remaining the same, fish contamination by Hg is not changing either, and nothing will be done because not much is known about the relationship of sulfates to MHg formation. What is disturbing, however, is that fact that the authors apparently misunderstood, or, worse, are not fully familiar with the past and ongoing cutting edge research on the relationship between the sulfates and some other factors (aromatic organics) on the development of MeHg and consequent mercury contamination of fish, which at least could give some guidance to the abatement and developing the site specific standard based on the nonlinear ("Goldilocks") model.

Specific questions and comments by line number as appropriate

116-119: A brief description of how fish were sampled (as is done for Mosquitofish and Sunfish) is warranted including number of fish (total and per site), size etc.

188-191: Could differences in source water (compared to other sites) explain the sharp gradients in Hg across the WCA 2?

320-329: Sampling protocol is more complicated for mosquitofish (MF) and sunfish (SF) and required a bit more description to aid in data interpretation. It appears MF individuals were composited at each site but SF were analyzed by individual fish (and site), but how sunfish species were separated by size and species this is not clearly stated. As written there is no way to know how many of each species were collected or to know if size was only a factor in sunfish (or for that matter why size matters if "up to 10 billgill") of a specific size were collected. Please be more explicit here.

353-367: What is the typical lifespan of MF? Is it short enough to assume that annual variation in Hg concentrations reflects the annual variation in bio-available Hg? If so, this should be more clearly stated (even if only assumed) as this variation could be a co-determinate of the influence of wet and drought years and other factors influencing bio-availability.

361-362: Something in these lines is worded oddly enough ("historically high in WY 2012"??) to make the meaning unclear.

369-372: Does each point in the graph represent a specific year at the designated site? If so this could be clearly stated.

398: It is not clear why SF data are not presented in a manner similar to the MF data, e.g. this year's data then the POR Thus a figure similar to 3B-5 is a logical addition.

Figure 3B-6: Since the goal of this figure is to compare temporal variation within a specific site (line 357), it makes more sense to separate the data by site then by year. Probably the easiest way to do this is to split the data into four separate panels, one for each site. As presented it is quite "noisy" and hard to decipher.

Figure 3B-7: Error bars are missing.

412-436, Figure 3B-8 and 3B-9: At the least there is an error in referencing figures in these lines. The time trends mentioned on lines 412-415 are quite apparent in Fig 3B-9 (not 3B-8). More importantly, it is not clear what Fig 3B-8 represents (why is the data for this year compared to the POR)? An incomplete description of the sampling methods further complicates this section.

514: How were areas of each "region" assigned? As with precipitation one appropriate technique would be Theissen polygons.

558-570: The reason for, and importance of, the analysis and discussion of the hypothetical surface load is not apparent. If the surface is measurable why calculate a hypothetical load based on an assumed concentration?

587: Data are presented for both sulfate concentration and sulfate load on pages 24-34. A casual comparison of data in Figures 3B-13 and 3B-14 suggests that there is much less variability of influent concentration than in load at any location so that any correlation between the two parameters is likely weak. It is interesting that concentration is less impacted by variation in hydrology, one would presume that if the sources had a constant release rate, load would be the more stable parameter as additional water diluted the sulfate. Therefore it is plausible to conclude that more water in a wet year exposes more sulfate sources counteracting the dilution effect. Regardless, a more systematic analysis of concentrations and loads from various locations could provide considerable insight into the sources of sulfate entering EPA. The short paragraph on lines 845-852 appears to "dance around" the important issue of likely sulfate sources to the EPA.

571-575: These statements are certainly not supported by the data presented in Fig. 3B-11 which display a remarkable steadiness of annual values. Any "trend" over two or three years is well within inter-annual variation.

750-753: What would sulfate be limiting to, biological processes? This is not clear.

775-776: Something is misworded and obscures the meaning of this sentence.

Table 3B-8: It looks like the mean and SE are reversed for flow volume of EPA inflow. It is hard to have a negative inflow.

906: Same comment as on line 514. How where point values assigned to areas or regions?

Editorial page and line comments

223: where *the* populations

676: used to determine *?if?* [sulfate] load

745-746: Reword to "…influencing the trend of decreasing sulfate concentrations with time could be…"

872-938: This section needs some editing for simple mistakes such as missed "s"es on plurals, lack of punctuation etc.

875: lawmakers

866 deposition to the EPA.

WebBoard-Posted: 11/8/13

In the following closure the reviewers will focus on the issues where there were differences of opinion between reviewers and authors or where reviewers make additional recommendations. If a comment is not specifically numbered below, the reviewers believe the authors' response satisfactorily addressed the original comment.

Reviewers took issue with the format of this 2014 Chapter 3B which is much less comprehensive than previous years, relying primarily on data reporting and trend statistics without venturing into synthesis of the data or documenting the scientific hypotheses. The authors respond that this change reflects the thinking of the state resource agencies, placing more emphasis on "data driven assessment". However previous years' versions were equally heavy in current year data reporting and simple statistical analysis, the biggest difference between those versions and this one is that most, if not all, data interpretation was removed this year. Who better to draw appropriate conclusions from the data (i.e. assess the data) than a consortium of researchers acknowledged to be leaders on the topic of mercury methylation in wetland systems? It was the high quality interpretation in previous years that set Chapter 3B above many others in the SFER and in our opinion, the current format is a move in the wrong direction that should be reversed in future years.

Gratefully, the authors provide additional interpretation of the data in this response, but with an apparent focus to suggest that previous years' analysis was jumping to conclusions not fully supported by the available data. The reviewers disagree, scientific conclusions are always open to new interpretations as more data becomes available, but decision makers must be presented with the most likely conclusions based on what is currently known. Data without expert interpretation is not very useful to someone having to make decisions (see Comment #1 in last year's closure).

As an example, the response contains specific discussion on the so-called "Goldilocks" hypothesis applied to sulfate concentration and mercury methylation. We disagree with the statement, "Based on the available data, FDEP and SFWMD have concluded that it is not currently possible to quantitatively identify the causative factors and linkages between MeHg and sulfate". We agree that the Goldilocks hypothesis is not perfect and is a simplification of factors influencing methylation, but it is a great leap forward from no interpretation and dismissing it rather than building on it is a mistake. Furthermore both reviewers are confused by the discussion of marine versus freshwater sulfate concentrations. All sampled areas within the Everglades appear to be freshwater. The provided Figure 1 suggests that the Goldilocks window is at the cusp between fresh and estuarine water at concentrations between approximately 100 and 1000 micromole SO_4/L. This corresponds to approximately 10 and 100 mg SO_4 /L, which brackets many of the observed values of sulfate in the EPA, presumably due to anthropomorphic sulfate loading of this freshwater system. The measured concentrations, regardless of source, would seem to confirm at least the *potential* importance of current sulfate concentrations on Hg methylation production.

Obviously, the linkages between fish tissue MeHg contamination and water column sulfate concentration are even more difficult to ascertain as assimilation is dependent on many more factors that just methylation production. Fish are mobile and Hg tissue accumulation is gradual, cumulative and dependent on trophic level; hence a correlation to sulfate concentration, or any one parameter, is expected to be weak. Nevertheless, MeHg production is obviously the first step in the process of bioaccumulation and factors influencing production should be a continued research focus.

Interestingly there appears to be no discussion in either this year's Chapter or this response on one co-factor influencing the sulfate-MeHg couple, specifically the influence on the quantity and type of organic matter present in the system. This is despite the fact that all three key findings in last year's closure addressed the importance of examining this relationship. Perhaps the reviewers should not be surprised by this; the premise of the current year Chapter appears to be to dismiss the hypotheses as outlined previously, not build upon them. One encouraging point made by the authors in this response is that there appears to be some momentum to continue development of a sulfur mass balance for the EAA (and presumably the EPA as well).

Key Findings and Recommendations (SOW Task 3)

- The best opportunity to reduce mercury levels in higher trophic organisms is to better characterize the factors influencing the methylation of input mercury. Unfortunately, the scientific interpretation of potential causative factors in Chapter 3B of the 2014 Draft SFER appears to be limited compared to recent years. Several of the previously postulated hypotheses on factors influencing mercury methylation, especially the influence of sulfate concentration, are de-emphasized this year.

- Development of a sulfur mass balance for the EAA and the EPA will only be beneficial if the relationship between sulfate concentrations and methyl-mercury production in the EPA is further developed. Previously developed hypotheses, such as the "Goldilocks" window provide the best guiding framework to determine these relationships. The influence of additional co-factors such as phosphorous, oxygen and especially dissolved organic matter organic (DOM) will improve the understanding of mercury methylation.

- While continuing research and model development will refine the concentrations of sulfur and DOM exasperating the problem mercury methylation, current understanding is now sufficient to begin addressing mitigation strategies which may include source reductions and/or redirecting water flow to keep sulfate concentrations out of the "Goldilocks" range.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 4

Level of Panel Review: Accountability
Reviewer: V. Novotny (AA)

WebBoard-Posted: 10/1/13

Questions:
– Does the draft document present a defensible account of data and findings for the areas being addressed that is complete and appropriate?
-Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report?
-Are findings linked to management goals and objectives?

Chapter 4 and appendices (Appendices 4-1 through 4-4 of this volume) provide the Water Year 2013 (WY2013) (May 1, 2012–April 30, 2013) update on the nonpoint source control programs mandated by the Everglades Forever Act (EFA) and Northern Everglades and Estuarine Protection Program (NEEPP). These programs address the reduction of phosphorus and other pollutant loads through on–site measures that reduce or prevent pollution at its source. Most of the surface pollution to the Everglades originates from nonpoint sources. However, as pointed out in the previous year review, atmospheric deposition, not addressed in this chapter and only partially addressed in Chapter 3A, is greater than the surface NP sources. The chapter outlines the programs in Lake Okeechobee, St. Lucie River, Caloosahatchee River, C-139, C-111, Everglades Agricultural Area (EAA), and some smaller watersheds. Construction projects and point source programs are described in the Northern Everglades protection plans in Chapter 8 of this year SF Environmental Report.

Apparently, the detailed review of the NP programs is performed every three years and WY 2011 (two years ago) was the year for such review. Hence Chapter 4 of the 2014 SFER is again limited in substance and results and focuses primarily on listing of the programs, their statutory background and regulations, permitting, and some future outlooks. Nevertheless, both Chapter 3A and Chapter 4 reviewed by this reviewer revealed that the Total P loadings and concentration reduction actions (both anthropogenic resulting from the implemented programs and natural meteorological effects -a note by the reviewer) surpassed the expectations of the 25% reduction of the load. The total cumulative reduction of the TP load between 1994 and 2013 water years is 55%. Another positive outcome emerging from this chapter is the increasing reliance on regulation and permitting for nonpoint sources, e.g., in the Everglades Agricultural Area (EAA) which in the past in the Everglades region of impact were (and still are in most other states) voluntary. This should serve as an incentive and example to the US Environmental Protection Agency and other states struggling with their TMDL programs when they relied only on voluntarism of farmers. Obviously, as documented in the chapter, issuing the permits is not enough, it must be accompanied by the education of dischargers and the public, and continuing research as it is being done by the SFWMD and other agencies and universities involved in saving the Everglades efforts. NPDES permitting is mandatory nationwide for point sources such as urban runoff. This positive facet of the chapter was somewhat diminished by the report on participation of dischargers in the Northern Everglades which was low.

On Page 4-3 the authors describe significant and above the goal decreases of TP loads from C-139 and Non Everglades Construction Projects (ECP). The details and information on other projects are in the bulk of the chapter. No nonpoint TP reductions were reported for the Lake

Okeechobee watershed and the two coastal Caloosahatchee and St. Lucie River watersheds that drain the lake.

Table 4-1 on page 4-5 is a summary table of NP loads in metric tons – mt, unit loads in lbs/acre and median TP concentrations for all watersheds. The table provides conversions to the SI (metric) units. Only a few watersheds are responsible for most of the Total P load: Lower Kissimmee (only a fraction of the load because the river is a main tributary of Lake Okeechobee which also partly drains into coastal estuaries), EAA, Indian Prairie and Taylor Creeks. The C23, C24, and C44 watersheds also have elevated total and unit loads are not identified on the map on Figure 4-1.

Pages 4-7 to 4-15 provide an update on progress in the EAA basin. This basin is the largest source of TP both by area and the total load. It is the basin where most of the previously controversial sugar cane and other crops agribusinesses are located. It is noticed that permits were issued to the majority of dischargers and, as result of permitting and education, loads have decreased, using the terminology of Chapter 3A, "dramatically". However, after a closer look at Table 4-3 on page 4-11, the loads in the WY 2013 are greater than loads in WY 2011 and 2012. But the overall trend and 5-year moving averages (Figure 4-3 page 4-12) show indeed significant reduction of the loads between 1995 and 2005, but leveling off after 2000. That indicates that the improvement occurred at the beginning of the BMP implementation program and current changes could be year to year statistical variations. The chapter did not adequately address this issue. Table 4.3 needs conversion factors for inch and acre-ft, and replacing archaic ppb with μg/L. We are now in the 21st century.

Pages 4-12 to 4-15 discuss control strategies and source control activities for EAA. The control strategy relies on mandatory implementation BMPs, which began in 1996 and incidentally coincides with the noticeable beginning of the decreases of TP loads from the EAA area. Comprehensive BMP plans include generally water management, nutrient management and sediment controls. As stated previously, making BMPs mandatory was a foundation for success. The goals and permits were derived from the TMDL. Basic manuals and guidance documents were prepared in the 1990s, farm operators were educated and trained, and the plan was supplemented by other actions such as flow diversions, and BMP research.

Chapter 3A revealed that most phosphorus, which is a key pollutant causing the water quality (eutrophication) problems in the protected Everglades area, originates from difficult to control atmospheric sources. Atmospheric sources were not covered in this chapter. Apparently, BMPs and the structural measures discussed in the chapter may have only limited or no impact on reduction of atmospheric sources. Also in the last review (WY2012) it was pointed out that a large phosphate (apatite?) surface mine is located in the vicinity of the Everglades watershed but the effect of the mine, definitely a significant source of the atmospheric phosphate loads, was not ascertained.

Pages 4-16 to 4-23 present the activities and results in the C-139 basin. This basin is relatively small and the loads are also small, a fraction of EAA. Nevertheless, Table 4-5 and Figure 4-5 show a significant reduction of the loads which after 2010 became better than the target loads. Table 4-5 again needs conversions factors of inches and acre-ft to metric units and replacing ppb with μg/L.

Six basins that are not a part of the Everglades Construction Project Basins are covered on *pages 4-24 to 4-29.* Five of these basins have discharge structures operated by the SFWMD. The loads from these basins are relatively small, altogether their annual phosphorus load is currently around 15 tons and the loads exhibit a modest decreasing trend after 2006 (Figure 4-7).

Pages 4-30 to 4-42 cover the North Everglades and Estuarine Protection Programs NEEPP. The area covers the watersheds of Lake Okeechobee, the Caloosahatchee River and the St. Lucie River. The two rivers/estuaries drain part of the outflow from Lake Okeechobee. The entire section extensively describes regulatory programs and the status of the activities. Unlike the previous sections, it does not provide any information on the progress and trends of nutrient loads. Some information on Lake Okeechobee and on the two river/estuaries is included in Chapter 10. In Chapter 10 nutrient loading information for the two rivers seem to be overwhelmed by a lot of other information and essentially lost. This could be because the hydrologic regime of the lake outlet and the two rivers is very complex, part of the flow from the lake enters the Everglades and a part feeds the rivers and goes to the Gulf of Mexico (Caloosahatchee River) and the Atlantic Ocean (St. Lucie River).

Thus the NEEPP section describes a "phased, comprehensive, and innovative "program containing source controls, construction projects, and research components that will be tied to the TMDL for the receiving water bodies. Apparently, this plan was described in detail in Chapter 8 of the 2012 South Florida Environmental Report and was reviewed and critiqued then. In this (WY 2013) report, Table 4-7 lists the nutrient control programs but no results. This section suffers from poor editing. It uses a lot of acronyms which are difficult to decipher. For example, an odd acronym WOD (works of district) was very difficult to find until it was located hidden in the title of Table 4-7. Also lettering on Figure 4-10 is very small and difficult to read. In general, the section on Northern Everglades provides no data and is lacking defensive synthesis and findings that would be linked to goals and objectives. The only quantitative result reported in this section is Table 4-8 reporting that only about 25% nonpoint nutrient dischargers (33% in the Lake Okeechobee, 12% in the Caloosahatchee River, and 3% in the St. Lucie River, watersheds) participate in the SFWMD programs. This is somewhat disappointing participation which makes the program at this time irrelevant as to its impact on the TP loadings to the Everglades.

The last four pages of the chapter (4-43 to 4-46) introduce the similar agricultural NEEPP programs carried out under the auspices of the Florida Department of Agriculture and Consumer Services. Herein, with about the same acreage (~ 2.1 million acres) the participation of agribusinesses is much larger, more than 71%. (Table 4-10). This table needs conversion factor from acres to hectares.

Closing Comments

Similarly to last year, the WY 2013 was apparently an "off-year" 2 of the three year assessment cycle . This caused that more than half of the chapter just described the programs according to some kind of prescribed outline which led to redundancies and repetition, especially for the Northern Everglades and estuarine water bodies but almost provided no data and results. These sections reported almost no results, the most important outcome were two simple tables listing the percentages of discharges that participate in the permitting program which was not that much for programs administered by the SFWMD. The report on watersheds south of Lake Okeechobee provide more information on reduction of the Total Phosphorus loading, namely from the Everglades Agricultural Area, C-139 and several non ECP small watersheds. The information on EAA (the largest of the reported watersheds), C-139 and non ECP watershed which are small, agrees with more detailed information on TP loading presented in chapter 3A.

Because of very few qualitative data being presented in the chapter it is almost impossible for the reviewer to answer the first two questions: (1) asking for assessing completeness and appropriateness of data and findings in the chapter, and (2) whether or not the synthesis is presented in a logical manner and consistent with the earlier version of the reports.

Clearly, the previous reports were more comprehensive. They included results from research sites (e.g., in Taylor creek watershed). None were presented in this chapter. No results on loading and research were presented for the Northern Everglades. The outline of the programs was impressive and contained a lot of good ideas. It is an integrated plan and the strategy includes (1) mandatory implementation of BMPs for phosphorus reduction, (2) regulatory programs, mostly for stormwater discharges, (3) voluntary programs, (4) educational programs, and (5) integration with local and regional water quality projects. But as shown in the second half of the chapter, participation and implementation for the northern watersheds are still far from satisfactory for SFWMD managed programs (about ½ of acres to be managed). This chapter still has a problem with the consistency of units, mixes both metric and US units (with and without US to metric conversion factors).

WebBoard-Posted: 11/4/13

This chapter outlined the nonpoint pollution programs in the Lake Okeechobee, St. Lucie River, Caloosahatchee River, C-139, C-111, Everglades Agricultural Area (EAA), and other smaller watersheds, some outside of the Everglades drainage area. The reviewer in his comments indicated that the chapter did not present enough qualitative data so it was impossible to answer the pertinent assessment questions given to the reviewer by the SFWMD**.** The reviewer attributed this deficiency to the stated fact that WY2013 was the off year for such evaluations. The authors stated in the responses to Comments 1 and 15 that the primary purpose of the Chapter was to provide annual updates on non-point pollution source control programs aimed at the reduction of pollutants in stormwater runoff. While very little of this data was presented in the bulk of the Chapter, they referred to Appendices for this information.

Inasmuch as one would expect summary data on trends of concentrations and loads would be included in the chapter itself, the reviewer's problem was that during the writing of the review the Appendices were not available or were not downloadable. It is obvious that the appendices were also not fully ready during the time when the chapter was submitted for review, otherwise logic would dictate that the authors would include key charts and summaries in the chapter instead of mostly providing older descriptions of the programs without assessment of their progress. Hence, some discussion on the trends and progress should be included in the closure and should be added to the chapter.

Appendices 1 to 4 present data for Northern Everglades (1), Everglades Agricultural Area (EAA) Construction projects (2), Non Everglades Construction projects (3), and Lake Okeechobee protection plan (4). Most of the trend plots for phosphorus and nitrogen (some key plots should have been included in the bulk of the chapter) show no up or down trends in the last 13 years with the exception of the Upper Kissimmee (Figure 18) where the trend was decreasing before 2007 and steady thereafter. In the Caloosahatchee River no long-term trend was observed except in Tidal Caloosahatchee (Fig. 64) which exhibited increasing TP UAL (Unit Area Loads) and FWM (Flow Weighted Mean) concentrations. For St. Lucie River five year moving averages of FWM TP concentrations in C-23 subwaterhed (Figure 80) were increasing until 2008 and remained more or less steady in the last five years with some up and down fluctuations.

Appendix 1 of Chapter 4 also provides data and charts of Total Nitrogen UALs and FWM concentrations for the two estuaries. This information is important because unlike the Lake Okeechobee, Everglades Water Management and National Park areas which are clearly phosphorus limited, the estuaries may be sensitive to nitrogen loads, i.e., there is some evidence (see Chapter 10) that they may be nitrogen limited or both P and N limited. Nitrogen limitation is typical for tidal and marine systems. Both estuaries have a history of developing Cyano-Harmful Algal Blooms. The data and charts in the appendix show mostly no statistically significant changes in the five year moving averages of FWM concentrations with the exception of the C-44 watershed on the St. Lucie River where the TN concentrations are increasing. The loads from the watershed are affected by the regulated discharges from Lake Okeechobee which is nutrient rich and limited by phosphorus.

Appendix 2 provides summaries for the EAA basins, which also includes the C-139 watershed. The data are primarily for WY 2013, no long term trends were provided for phosphorus UALs and FWM concentrations.

Appendix 3 provided and described data for Non ECA watersheds draining into ENP. The trends are presented in Table 1 and on a few plots. Several watersheds listed in Table 1 have missing data in the five to six years preceding the WY 2013, which perhaps was the period of construction. Visual observation of the table revealed that the FWM concentrations in WY 2013 are not statistically different from the preceding years.

The analysis of the data in the appendices is an assessment on some watersheds in the Northern Everglades (Appendix 1) and Non Everglades watersheds in Appendix 2. In most watersheds the trend is zero. The increasing TP trends in the tidal Caloosahatchee River and in the St. Lucie River are troublesome because these rivers have a history of Harmful Algal Blooms (http://myfwc.com/wildlifehabitats/health-disease/cyanobacteria/).
The tables and charts in the appendices do not have conversion factors to metric units or replacing archaic ppb by uniformly accepted μg/L.

Key Findings

- This chapter and related appendices provide an update on the nonpoint source control programs mandated by the Everglades Forever and Northern Everglades and Estuarine Protection Programs. In a tabular form it provides a summary of unit area loads and concentrations for all watersheds enlisted in the program. This table shows that TP concentrations in flows from a number of watersheds are still high (tentatively a high TP flow weighted mean concentration would be more than 50 μg/L because these concentrations are borderline between eutrophic and hypertrophic conditions). Concentration greater than 100 μg/L, ubiquitous in the Northern Everglades watersheds, could trigger algal blooms and accelerate eutrophication as it has periodically occurred.

- Because of the likely nitrogen limitation and history of Cyano-HABs in the two estuarine systems, the district must give increased attention to reducing both phosphorus and nitrogen loads and concentrations to and in the St. Lucie and Caloosahatchee Rivers.
- The overall trend tables indicate that most of the loads reductions were achieved before 2000, some additional improvements occurred in the last five years.
- It was noted that the participation of the dischargers in the South Florida WMD works permits in Lake Okeechobee and Caloosahatchee and St. Lucie Rivers was very low, which requires attention.

- On the other hand participation in the Florida Department of Agriculture and Consumer Services managed BMP programs was relatively good (~70%).
- Perhaps because of still relatively low overall participation, the data and charts in the Appendices (primarily Appendix 1 featuring Northern Everglades and the estuaries) show mostly steady state of flow weighted concentrations. Unit area loads are impacted by precipitation and in the estuaries by regulated releases of flow from the nutrient rich Lake Okeechobee.
- Some St. Lucie River and Caloosahatchee River subwatersheds reported in Appendix 1 exhibit increasing P concentrations which are troublesome because these water bodies are susceptible to Cyano - Harmful Algal Blooms. These watersheds as of now have relatively low participation of dischargers and most of the remedial construction projects so far (see Chapter 10) focused on flow manipulation and storage.

Recommendations

The chapter as written does not contain enough narrative and graphical assessments of the trends and synthesis of the data. Summaries of the appendices with key charts; e.g., those indication positive and negative trends should be included in the bulk of the chapter. Consequently, the extensive narratives of the programs without assessment and synthesis should be abbreviated.

Without these editorial changes readers, and the reviewer, will have problems assessing the key questions given to the reviewer, i.e., (1) asking for assessing completeness and appropriateness of data and findings in the chapter, and (2) whether or not the synthesis is presented in a logical manner and consistent with the earlier version of the reports. Clearly, the data charts included in the chapter and in the appendix are not complete and a synthesis is missing both in the chapter and in the appendices.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5A

Level of Panel Review: Technical
Reviewers: P. Dillon (AA)

WebBoard-Posted: 10/2/13

It has been clearly demonstrated in the annual reports for a number of years that the phosphorus (P) levels in Lake Okeechobee and the Everglades in general are too high. Despite extensive efforts and expense, significant improvements are still required. This chapter describes new projects that will contribute greatly to the needed reductions in phosphorus.

Six new construction projects in three flow paths (described as Northern, Central, Western in the opening Summary; Eastern, Central, Western in later parts of the chapter) are described. Together they will add 6,500 acres of STAs (Stormwater Treatment Areas) and 110,000 acre-feet of water storage (116,000 acre-feet in the detailed project list) through construction of FEBs (Flow Equalization Basins). In addition, plans to implement additional source controls in the eastern Everglades where stormwater runoff has historically had very high P levels have been developed. Finally, a science plan whose purpose is to improve and optimize the functioning of the STAs was produced. These are all very positive developments for the future of the Everglades. It is particularly important that a science plan was prepared because it is made clear in other chapters of the annual report that the effectiveness of the existing STAs can vary greatly depending not only on external factors such as storm-driven hydrologic events but on controllable factors such as the type of aquatic vegetation used in each STA, the length of time that cells are dry, etc. I think that it might be reasonable to expect that the P retention efficiency of individual STAs could vary by a factor of 2 depending on the operational conditions employed.

These projects are scheduled over a 3-phase time frame lasting 12 years. Although this seems like a long time, it is reassuring to see how many of them have already been initiated. Several questions were asked of reviewers related to an accountability review. These are addressed below.

The proposed projects, with the aim of reducing P levels towards the 10 ug/L target, are all totally defensible. It is clear based on data collected from existing STA sites over a number of years that they can be effective sinks for P, with up to 90% removal under optimal conditions. Therefore additional STAs can only increase the total level of P reduction in the region. The science plan, specifically the optimization of the use of the STAs, is important as this is a cost-effective way of getting the most for the dollars spent. It has also been clear for some time that drought and drying of cells in the STAs is very detrimental to their performance, with substantive spikes in P concentration following the cessation of each drought. The development of the FEBs giving the ability to eliminate or minimize drying events should virtually eliminate this problem and lead to much improved P removal by the STAs. The sub-regional source control plans in the S-5A sub-basin will also contribute to greater P reduction.

The information is this chapter is presented in a logical manner and clearly linked back to earlier work and to other ongoing work. I found the chapter very easy to read (although there are a few inconsistencies, noted above), and consistent with the rest of the report and previous reports. The new projects are directly related to management goals and objectives. The WQBEL dictates the allowable P concentration of STA discharges with a cap of 19 µg/L as a flow-weighted average in any year and not more than 3 years out of 5 exceeding 13 µg/L as a flow-weighted average. The new projects are necessary if these targets are to be met.

In summary, the new projects, which seem to have got off to a very good start, have the potential to result in very substantial improvements in water quality in the Everglades. They are entirely consistent with the long-term objectives of all levels of government and stakeholders.

WebBoard-Posted: 11/12/13

Closing Comments

In this chapter, 6 new construction projects that will augment or create additional Stormwater Treatment Areas are described, and a plan to implement additional source controls in the eastern Everglades where stormwater runoff has historically had very high P levels is outlined. A science plan whose purpose is to improve and optimize the functioning of the STAs was produced; this is an important step because it is clear that the effectiveness of the existing STAs varies greatly depending on both external factors such as storm-driven hydrologic events and on controllable factors such as the type of aquatic vegetation used in each STA, the length of time that cells are dry, etc. These are all very positive developments for the future of the Everglades.

The author of chapter 5A provided a reasonable response to the one (minor) technical point raised in the review. There are no points of disagreement between the authors and the reviewer.

Findings and Recommendations

1. Based on data collected from existing STA sites over a number of years, it is apparent that they can remove up to 90% of the TP input to them. Additional STAs can only increase the total level of P reduction in the region. The science plan, specifically the optimization of the use of the STAs, is important in this regard as this is a cost-effective way of reducing TP.

2. Drought and drying of cells in the STAs is very detrimental to their performance, with substantive spikes in P concentration following each drought. The development of the FEBs giving the ability to eliminate or minimize drying events should virtually eliminate this problem and lead to much improved P removal by the STAs. The sub-regional source control plans in the S-5A sub-basin will also contribute to greater P reduction.

3. The new projects are directly related to management goals and objectives. The WQBEL dictates the allowable P concentration of STA discharges with a cap of 19 µg/L as a flow-weighted average in any year and not more than 3 years out of 5 exceeding 13 µg/L as a flow-weighted average. The new projects are necessary if these targets are to be met.

4. In summary, the new projects, which have gotten off to a very good start, have the potential to result in very substantial improvements in water quality in the Everglades. They are entirely consistent with the long-term objectives of all levels of government and stakeholders, and, as such, satisfy all requirements related to accountability.

Key Recommendations

1. The new projects that have been started, as well as the plans for additional source controls, should be given high priority as they have the potential for resulting in significant TP loading reductions.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5B

Level of Panel Review: Technical
Reviewers: O. Stein (AA); P. Dillon (A)

WebBoard-Posted: 10/1/13

Technical Questions:

1. Are the findings and conclusions supported by "best available information," or are there gaps or flaws in the information presented in the document?

2. Are there other interpretations of the data and other available information that should be considered by the authors and presented to decision makers? If so, the panel shall identify specific studies that should be addressed or available data to support alternative findings.

The review is divided in to several sections. First are several broad questions and comments regarding the interpretation of reported results followed by relatively minor questions about specific sections, sentences, phrases, etc. This is followed by comments on figures and tables, editorial suggestions and lastly summary and recommendation comments. The broad comments were written after a complete read of the document while many of the specific comments were written "on the fly" as the document was being read. Occasionally a specific comment is qualified in a subsequent one, but left for the authors to respond to, as it is a question that arose as that section was being read.

Broad questions and comments that should be addressed
This chapter is straightforward and very clearly, if dryly, written. There is very little technical information presented, really just two studies, the periphyton and mesocosm studies, discuss technical issues and certainly the first half of the chapter describing the performance of the STAs is not technical. Since Chapter 5 has now been split into several subchapters and appendices, perhaps it is time to divide it once more with one section describing STA performance for the water year relative to the POR which is reviewed at the accountability level and the research summaries which would be evaluated at the technical level.

In general, operation of the STA's seems to have been very successful in WY2013, with record TP removal despite some adverse conditions, e.g. Tropical Storm Isaac. The P output from the STA's was the closest to the target values since this work started, which is promising for the future. To ensure continued improvement in the STA's functioning it is clear that whatever steps necessary to prevent the drying out of individual cells which led to large effluxes of P (and probably N) following re-wetting should be a priority. Alternatively, amelioration of dried sediments with substances that may prevent re-dissolution of P compounds or that may trap P produced by oxic degradation of the organic matter might be considered.

Table 5B-14: This table is a nice way to briefly describe the research activities associated with the STA.

Periphyton study: Potential results of this study are compromised by many operational difficulties, most of which are associated with a lower bed elevation leading to the influx of water and P to the periphyton cell. It appears that a water budget with an acceptable level of error is nearing completion, but a method to close the P budget remains elusive. We suggest one possible way to do this in the specific comments section. Regardless, when any important parameter (in this case the most basic, the P load) must be estimated rather than measured, the potential utility of the ensuing analysis is severely compromised. We fear this study will never lead to a determination of the effectiveness of periphyton relative to other plant and microbial assemblages yet the District seems very committed to the potential use of periphyton. Perhaps the P load issue can be adequately addressed, but the District should also consider smaller, more manageable (and less expensive) studies analogous to the mesocosm study to determine periphyton effectiveness. There is one comment not addressed specifically below. It appears that there has been no attempt to compare the periphyton cell the adjacent SAV cell, yet clearly this was the intent of the original experimental design. Rather comparisons are made to the adjacent cells 3B and 2B. Why?

Mesocosm study: This appears to be a well-conceived study with the potential to shed light on the differences of plant type on performance. Interestingly the preliminary results seem to indicate the SAV communities might not be the best option, but this is confounded by two different essentially SAV treatments having different overall TP removal trends. The results on the types of P in the outflow should shed considerable light on the possible mechanisms at play. However the results are likely compromised by running the system in essentially a continuous flow mode with only by-weekly sampling. It might be useful to run the system for another year or two in batch mode to better assess the temporal dynamics and possibly calibrate removal models. That said, there will also be issue of scale-up from a mesocosm study to a full scale system.

Temporal Dynamics study and Internal Water Quality studies: It is hard to consider these scientific studies analogous to the Periphyton and Mesocosm studies. Rather these are more appropriately termed ongoing monitoring and analysis studies. Nevertheless, they can be useful to the District as tools for optimization of the STAs. These studies appear very similar in nature except that the former includes an assessment of plant community as well as water quality, therefore it is more valuable. In the future it is recommended that all monitoring of internal dynamics consider water quality and vegetation distributions and densities simultaneously, enhancing cause-and-effect analysis.

Specific comments and/or questions by line number

throughout – the calculation of reduction efficiencies based on adjusted effective treatment areas gives a somewhat over-optimistic picture. I understand why the calculations were done but the efficiencies without this “correction” are also important as they represent the “true” efficiencies of the systems.

70 – these spikes can be a major contributor to the total annual flux. It would be useful to see an estimate of the portion of annual export attributable to the drying episodes.

128-129 (and similar sections for individual STAs): Perhaps the details of how flow-weighted means and annual loads are calculated could explain the discrepancy, but how removal rates and inflow and outflow correlations calculated from either parameter could be different is not at all clear. Presumably an annual load is the summation of the product of the FWM concentration and discharge over discrete time periods. It would be instructive to provide the equations used to calculate both parameters (as is done for adjusted effective treatment area on line 169).

145 – why is outflow volume always greater than inflow volume? Is this seepage in> seepage out?

245 – a drought contingency plan is good but perhaps it needs to go further, given that droughts and their impacts remained

319-321 (and similar sections for other STAs): A dedicated call-out description of which cells are in what flow-way is a nice addition which aids in the interpretation of the data.

329-336: The wording here is a bit confusing because data for this water year is mixed with data for POR (even in the same sentence!). The format used in STA-1W (lines 469-473) is much more logical and that should be adopted here as well.

344-345: It is interesting to note that if the first two years of operation (2005 and 2006) are removed, there is likely a correlation between inflow and outflow parameters as there is with every other STA. Clearly performance was unique in the first year and this likely carried over to the second year. Though removal of any data should always be done with caution, it might be enlightening to assess the correlation with the first two years of data removed (especially since it is the first two years where some start-up effect might have been at play).

483 – I think that it would be very useful to have in-place oxygen monitors at several sites to help determine the mechanism for P release from the sediments

498-503: "Substantial" is an insufficient adjective here. While flow diverted prior to treatment in the STA can't be used to assess the efficacy of the STA, how much water of what concentration is certainly germane to the success of the STAs in protecting the EPA region. While the diversion might be unavoidable, some mention of it numerical magnitude is appropriate.

503-509: Why is gate G-307 preferred to G-309? Was it perhaps to minimize potential short-circuiting, or perhaps due to the vegetative clogging?

517:527 (and similar sections): It is unfortunate that bird nesting success is at odds with P removal objectives. While not an expert in avian behavior, it would seem that these conflicts could be minimized if certain small areas within the STAs were dedicated as bird nesting sites. This could be done by construction of bermed areas or perhaps more effectively by construction of small "islands" within the cells. In fact, areas of higher topography would more closely mimic the original landscape of islands and sloughs and have little influence on the hydraulics of the individual flow-ways or cells if oriented properly. It could be an extremely cost-effective way to minimize habitat and water quality conflicts.

543-549 (and similar sections): It is much easier to follow the discussion of effects if changes are grouped in a positive and negative format. Thus rather than describe changes in plant cover by cells in a numerical order, it would be helpful to group them by desired vegetation type, e.g. cells 5A and 5B changing in the same direction (more EAV) would be desirable in 5A and not so in 5B (unless an increase in 5B was due to a planting of berms to minimize short circuiting).

584-588: In other STAs the preferred treatment order appears to be EAV cells followed by SAV cells. Why are most flow-ways in this STA SAV followed by EAV?

594-598: Considering the extensive soil testing that has been done over the years in the STAs it is interesting that (apparently) soil P content and mobility testing to address this hypothesis has not been conducted.

626-635: This partially addresses the comment associated with lines 594-598.

672-679: What is the anticipated time-frame of conversion of cells 5 and 6 from EAV to SAV? Is total conversion even desirable considering the comment from lines 584-588?

635 – agreed. The success of Cell 1, if understood, may give lessons that can be adopted on a wider basis.

754 – same as 635. A careful analysis of why STA 3/4 works best is warranted as this may provide information needed to optimize design in future.

692: Confusion here; according to Figure 5b-12, cell 2 is desired to be SAV not EAV (consistent with the text). But in Figure 3 in Appendix 5B-1, cell 2 is shown to be EAV. Which is correct?

761-763: See the comment for lines 128-129. It is not clear how there could be a correlation for one parameter but not the other.

775-777: See the comment for lines 498-503.

895: Do you mean, Cell 5-1A rehabilitation?

917 – "flow-way 7 ...operational throughout WY2013..." but "..flow-way 7 was offline..." ??

917: If flow-way 3 was operational all year, why are the data not reported in Table 5B-11? (Perhaps there is a typographical error explaining the two comments???)

942-943: Is this criterion used for both SAV and EAV cells? It seems EAV could tolerate a lower stage, (perhaps -0.5 ft) with minimal disturbance (depending on the variation in topography with the cell). Though perhaps a different criterion is appropriate if based on plant survivability versus P release due upon reflooding.

945: Was there a negative impact on the SAV community of cell 5-3B?

1012 and elsewhere – I am concerned about the widespread chemical treatments without comment on the potential or actual effects on organisms other than the target plant species. I haven't been through the appendices so I may have missed something, but information of the toxicity and bioaccumulation of whatever chemical are used should be included somewhere.

1136-1146 (and associated tables and figures): Table 5B-15 indicates that the concentration of TP was 16 ppb and flow was maintained for 365 days. Yet data in Figure 5B-30 indicates a mean inflow TP of about 12 ppb "during the period of flow" (line 1173). These would seem to be incompatible data.

1178: It is stated nowhere in this document (that we could find) that periphyton and algae are the same thing, except that it could be inferred from this title. This should be stated more clearly, or if not correct, just exactly what organisms are included in periphyton should be stated. In general this section is assuming that the reader is more familiar with the terms used than (at least one of) the reviewers are.

1179-1182: Related to the previous comment, it is not clear why there is SAV in the periphyton cell, presumably SAV is a macrophyte and should not be in a periphyton-dominated cell. Note that as one reads on it gradually becomes clear that the periphyton is associated with macrophytes, but then one could assume that periphyton is in all STA cells, whether they be

EAV, SAV or other material. So what is unique about the periphyton cell? There is not enough description to understand what the intent of this plant characterization is or what makes this cell unique from others.

1211 – the P values seem very low. I assumed that they must be expressed on a wet weight basis instead of a dry weight basis, but this apparently isn't the case (l. 1196). They do seem about 10x lower than "normal" however.

1223: The high Ca concentration might be an indication that a significant percentage of the periphyton growing in the cell are diatoms. Is there any way to distinguish between different species of periphyton?

1235: What is APA? It is clear a few lines later, but the acronym should be defined immediately.

1346-1351: It is not had to see why P concentrations at the outflow structure went down during flow (Fig. 5B-44) as influent when down. However it is a bit disconcerting that there was no change in TP during conditions of no flow and that all reduction in outflow TP during flow appears to be due to the decrease in inflow. These data would appear to suggest that the periphyton cells are not working as intended. One would certainly expect P concentrations to go down during periods of no flow as the system would behave as a batch reactor. This of course ignores the possible effects of seepage, but the water budget (and stage) should shed light on this.

1437-1442: Continuing with the above comment, it should be feasible to either a) estimate the P concentration in the seepage water by using the data in figure 5B-44 and the calculated k values and Equation 5, or b) alternatively estimate the k values by making assumptions of the P concentrations in the seepage water. The first approach probably makes more sense, and could help close the P budget.

1481: It would appear that after SAV colonization of the soil treatment it would, for practical purposes, not be any different than the SAV treatment, yet the performance was markedly different in these treatments (Table 5B-19). Is there any possible explanation for this?

1515 – based on the mesocosm study, it seems that the P removal is largely PP removal, i.e. vegetation is trapping particulates in the inflow rather than using P to build biomass and sediments. How far do you think that this applies to the full-scale STA's? Table 5B-19 also demonstrates this.

1753-1754: Is the "TP load" from the canal sediment to the influent to STA2? If so this is not clear. Also it would be good to provide the reader with the size of the canal. How wide is it? How deep?

Figure and table comments

Figure 5B-14: Where vegetation sampling occurred is not clear, though one can surmise it is only the lower portion of cell 2. The left side should contain only cell 2 and the lower area highlighted to emphasize this was the area sampled (if we have surmised correctly).

Figure 5B-25: The panel showing total SAV coverage is very useful. This should be added to the analogous figures displayed for the other STAs.

Editorial comments by line number

345: panels B and *D*

580: should Basin S-2 be repeated, or are there 4 basins that can contribute runoff?

625: …the other two *operational* flow-ways…

1088: …data at all *of* the water-control…

1090: comma after inflow

1359: …exhibited *a* small diel pattern…

WebBoard-Posted: 11/8/13

In the following closure the reviewers will focus on the issues where there were differences of opinion between reviewers and authors or where reviewers make additional recommendations. If a comment is not specifically numbered below, the reviewers believe the authors' response satisfactorily addressed the original comment.

Comment A4: The physical layout of the lower SAV and PSTA strongly suggests that initial design of this experiment would make a direct comparison of these cells. It is disheartening to hear that the construction is such that makes this comparison problematic. Regardless, substituting a portion of an adjacent cell of much larger dimension with no physical boundaries does not seem to improve the ability of direct comparisons. One could argue that retrofitting the culverts to make direct comparisons of the PSTA and SAV cells possible is cost-prohibitive, but an equally valid argument is that results from not only unreplicated (unavoidable at this point) but also physically different configurations does not meet the basic premise of any scientifically valid experimental design. From this perspective, all costs currently incurred in this experiment are potentially wasted, because no scientifically valid conclusions can be drawn.

Comment A5: It is somewhat ironic that the one study reported in this Chapter that does seem to be built on a sound experimental design is the one that is being terminated. The confounding of factors used as reason for termination are exactly the reasons that the PSTA-STA experiment will likely not achieve useful results. In fact it would appear from the limited information provided in the Chapter that the confounding factors mentioned in the authors' response are minor compared to those of the periphyton study. The reviewers are at a loss why this study was chosen for termination when other more poorly conceived (and likely more expensive) studies are maintained.

Comment A6: The same theme as above applies here as well; "opportunistic monitoring", especially at times of systemic problems such as during declines in vegetation health, is not the way to carry out experiments. We wish to emphasize that following a sound scientific approach to experimental design does not preclude collection of data collected opportunistically or that such data might not be useful, but it does seriously compromise the ability to extrapolate results to other conditions and/or locations, making the "bang for buck" relatively quiet. Throughout this Chapter and in the historic reporting of experimental results in previous years, there appears to be a recurring theme of haphazard design of experiments leading to useful, but not fully useful information. This is an easily corrected situation in the future if more effort is expended during the experimental design.

Comment B1: We agree with the response in principle and do not suggest changing how performance is calculated, especially when trying to assess causative relations of performance. That said, from the publics' or management perspective, performance is based on the opportunity cost of the land used to achieve the required reductions in P load. Efficiency of the system is how much land area is needed, not how much is actually in use at any given time. The comment was intended to be "food for thought" in how efficiency in determined.

Comment B12: The reviewers appreciate the management challenges that nesting birds create when P removal is the number one operational goal of the STAs. That said it is again ironic that the compartmentalization of sites and management goals creates a situation that nesting of native birds, including threatened species, must be inhibited in an effort to achieve ecological restoration goals.

Comment B14: We understand that there is a small amount of EAV at the beginning of cells 5 and 6 but this is not the typical arrangement of the entire first cell being EAV. Also it appears that Cel l3 is entirely SAV and receives water from the G333 Structures while Cells 1 and 2 are entirely EAV with no following SAV cells. The initial question was more of a curiosity, but a follow-up question is whether there is a plan to change flow configurations and or planting to make all flow ways EAV followed by SAV?

Comment B20: This explains some our earlier confusion. Comment B14 above may need qualification once corrected maps are presented.

Key Findings and Recommendations (SOW Task 3)

- The POR data for P loads and concentrations to and from the STAs indicates that they are, in general, effectively removing large quantities of phosphorous that would otherwise be entering the EPA. Additional experimentation and operational tweaking is still required to assure that these systems will meet the mandated removal targets consistently over the long term.

- From a scientific perspective the design of experiments intended to optimize these systems is sub-standard. More emphasis on the research methodology should be incorporated at the experiment conception level, rather than the "opportunistic monitoring" approach that is too often currently employed. It is not clear why certain well-conceived relatively cost-effective experiments, such as the mesocosm study, have been terminated while other more poorly conceived and costly experiments continue.

- The current operational objectives of the STAs make these locations the ecological "sacrificial lambs" in order to achieve ecological restoration of downstream locations. The District is encouraged to re-examine some operational guidelines to determine if some ecological function of these locations (such as bird nesting) could be better incorporated without compromising other objectives.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5C

Level of Panel Review: Technical
Reviewers: O. Stein (AA)

WebBoard-Posted: 10/2/13

Accountability Questions:

Does the draft document present a defensible account of data and findings for the areas being addressed that is complete and appropriate?

Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report? Are findings linked to management goals and objectives?

The panel may also provide constructive guidance for the District's large-scale programs, particularly as related to water quality assessment and control across the agency. Importantly, this review should specifically focus on report-related scope and generate critiques only on material presented in the SFER and not expand into high-level agency priorities or policies. For example, do the newly added chapters on the Restoration Strategies Projects (5A) and Science Plan Update (5C) adequately provide a description and status of these required programs/initiatives?

Broad Comments

Does the draft document present a defensible account of data and findings for the areas being addressed that is complete and appropriate?

The chapter clearly articulates the reasons for developing a research plan and the approach (with collaboration between several state and federal agencies and members of scientific/consulting community, and with opportunity for public input) used for formulate the new modified plan. The plan identifies six key questions that research should address and then provides more background and identifies many sub-questions for each of the key broad questions. It then outlines four factors (Testability, Feasibility, Timeliness and Importance) that are/will be used to rank the proposed sub-questions and mentions that eight sub-questions are initially identified for immediate investigation. However, no information is provided as to how these eight came to be ranked highest and/or the ranking of other questions. Perhaps this omission could be considered part of the adaptive process, but clearly several questions were ranked highly but not quite at the "fund now level. Presumably these would still be highly ranked when the next determination is made and it might be instructive to rank sub-questions within certain tiers as proposals to federal funding agencies such as NSF would be ranked. Also no information is provided as to the frequency of re-ranking questions within the five year plan. Will this be done only once every five years, or more frequently?

There follows a section on "other areas of investigation" that seem to be deemed important research areas but not falling into one of the six key questions. This gives the appearance that the District is not using the Strategic Science Plan to identify priorities, but this is odd because both identified investigations, while more practical than scientific, clearly fall with the general category of Key Question 5. More accurate monitoring techniques (the focus of both investigations) are clearly operational issues.

Lastly the chapter describes the development and implementation of nine studies to address the eight questions including a timeline for each study. It is odd that there are nine studies for eight questions, but it is clear that one of the studies focuses on one of the "other investigations". Where the information will be reported in future Reports is not explicitly stated but since these have typically been in what is now Chapter 5B, perhaps that is the assumed location.

Since there is no information as to how the eight sub-questions, nor the nine specific studies, were selected, it is not possible to determine if the selection process was "defensible". That said, the approach and criteria are provided, and other than the aforementioned inconsistency with the "other investigations" the outlined approach appears to appropriate.

Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report?

Since this is a new sub-chapter there is no realistic way to directly compare the consistency with prior Environmental Reports. However many aspects of the science plan were in components of other discontinued chapters or the old (comprehensive) Chapter 5. Separating out the reasons for conducting specific research and providing sub-questions and finally a timeline for nine initial studies is a more logical way to present any research plan compared to the hodge-podge methods previously employed. This is definitely an improved presentation. However, it is difficult to follow this chapter without at least referencing Chapter 5A Restoration Strategies, especially with regard to the first of the six key questions focused on flow equalization basins (FEBs.)

Are findings linked to management goals and objectives?

The Introduction (and the instructions above) clearly limit the focus of the chapter (and this review) to developing an adaptive science plan for P removal in STAs so that they will meet specific water quality objectives. Within that narrow focus the plan is clearly linked management goal and objectives.

Is there any constructive criticism and guidance to offer for the District's large-scale programs?

In the future it would be logical to have Chapter 5A and 5C be reviewed by the same reviewers. Additionally, since these chapters really focus on the "big picture" of STA performance, it makes sense to assign AA and A reviewers to it.

Editorial page and line comments, suggested text changes in *italics*

150: The acronym FEB is introduced here but is not defined until line 178.

WebBoard-Posted: 11/8/13

I have only one additional comment to make on this sub-chapter. The District is encouraged to develop *and make public* the methodology used to rank studies for investigation as all are agreed that it is not feasible to conduct all important studies simultaneously. The methodology need not be purely quantitative, but use quantitative metrics certainly helps make more informed and certainly more open and objective decisions.

Key Findings and Recommendations (SOW Task 3)

- The development of a research plan, and appurtenant reporting of research results in future versions of this Chapter, is a significant improvement to the methodology of how decisions are made to improve performance of the STAs.

- The District is encouraged to develop and make public a methodology of how research initiatives are ranked for implementation.

- Now that a research strategic plan has been developed, more emphasis should be placed on ensuring an appropriate application of the scientific method is employed to reach stated objectives. This includes development of an experimental design including replicated treatments whenever possible and a plan for data analysis and reporting for every initiated research project at the project initiation stage.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 6

Level of Panel Review: Technical
Reviewers: W. Dodds (AA); P. Dillon (A)

WebBoard-Posted: 9/30/13

This chapter details research findings in 4 areas 1) wildlife ecology, (2) plant ecology, (3) ecosystem ecology, and (4) landscape patterns and ecology. The research occurred during a wetter than normal year. The chapter does a good job overall at explaining the experiments and describes some very impressive large-scale manipulative experiments. Most of the comments are aimed toward improving details of the approaches, but the overall approaches for the major sections in this chapter appear to be sound.

The higher than normal precipitation was correlated with a high rate of nest failure, and perhaps led to some food limitations in the wading bird populations. Crayfish, *Procambarus fallax*, movement during desiccation was explored. Exotic fish continued to increase. In general, there are quite a few statements in the Wildlife and the Hydrology sections about factors that could influence nesting success. It seems that there should be some effort building a unified conceptual model to predict nesting success for each of the species. Conceptual diagrams could then be used to assess progress toward understanding the factors that encourage restoration of population sizes.

The crayfish work is solid, some more advanced tracking devices could prove more definitive, but the results would likely stand.

The plant ecology section focused on two areas, species surveys of ridge and slough areas, and detailed sampling of tree species on tree islands.

The LILA experiment has shown some interesting results, and given that there are trends, but that they seem to be ongoing, this experiment should be maintained and periodically evaluated (maybe every 3-5 years).

The ecosystem findings were related to effects of cattail removal by herbicides and soil accretion rates. In the landscape area, a decomposition model was tested, and several approaches to understanding patterns of submerged aquatic vegetation in Florida Bay were explored.

The LILA hydrology work with the artificial islands is a neat experiment, very interesting and nice that it is accomplished at relevant scales.

The cattail removal experiments are promising, but the AMI-1 study is not replicated and difficult to interpret.

The carbon dynamics work is strong, and the results are compelling. To complete the balance, an estimate of dissolved organic carbon loss would be good, and a standard ecosystem flux diagram might help visualize the overall budget.

In the sediment modeling work I cannot help but wonder about the effects of bioturbation on sediment suspension and movement. For example during dry periods, crayfish move in and are concentrated and birds actively feed on them. Could this be a time of increased sediment transport?

The plant-salinity monitoring makes sense, but more years of data are needed here to firm up results.

It is difficult to evaluate the output from the "Simulation Modeling to Evaluate CEPP Alternatives and Effects on Florida Bay Submerged Aquatic Vegetation" with the provided information (citation not in reference list).

Minor points

1. Page 6-2 The third ecosystem project is?
2. Line 188. Better and worse means for wading bird success? Maybe it would be better to use more neutral terms, as they seem like value judgments.
3. Line 209 ' It is believed that…" Should be stated that tree islands may require some dry periods for survival, rather than something is "good" for tree islands.
4. Line 244 something is missing
5. Line 286.. amusing comment but probably should be removed
6. Fig 6-13 which of the sets of mesocosms is this for, or is it a composite of M1-M4?
7. Figure 6-14 Is there any way to get normalize the number of nests with the total population number? This might tell a slightly different story.
8. Line 618. A significant portion statistically or a substantial proportion.. how about a range of biomass?
9. Table 6-4 Italicize species names
10. Table 6-6. With the replication, can't the variance in responses be reported on this table?
11. Line 1264. Maybe say it was marginally significant? $P < 0.05$ is an arbitrary value, or say it was less but not quite at the $p < 0.05$ level.
12. Line 1417. Is one month a year often enough for particulate P collections?
13. Line 1551 Sentence incomplete.
14. Figure 6-35 is pretty difficult to interpret
15. Figure 6-38 Include error bars? Separate graphs if needed.
16. Figure 6-40 These are some nice plots to help visualize what is going on.
17. Line 1775. This is data not shown, right?

P. Dillon, secondary reviewer

General

Like last year's version, this chapter is very well-organized (much more so than in years prior to 2012) and filled with useful and interesting data. Many of the experiments have yielded valuable insights into the functioning of the Everglades ecosystem. I particularly liked crayfish movement experiments and The monitoring has been well-planned and appears to have been well- executed. Almost all of the work is thoroughly documented and justified.

The overview (table 6.1) is very useful. An addition that would help the reader assimilate the large amount of information in this chapter would be a very brief, point-form summary of the key findings/conclusions at the end of each section in addition to, or integrated with, the paragraph on relevance to water management.

Specific

l. 26 – it would useful to provide relative amounts in addition to absolute numbers for figures such as this – without hunting around, I don't know how much of a relative change 12,232 acre-feet is, or even whether it is significant

l. 90 - and the third study is?

l. 138 – this section needs some editing; it is written in a colloquial style unsuitable for a scientific report

l. 187 – this section also needs editing for grammatical errors

l. 247 – it is mentioned frequently that recession rates are a controlling factor in foraging but I can't recall anywhere where the science behind this is presented or referenced. Is this so widely known that it goes without saying?

l. 333 – same comment as line 26

l. 364 – the work in this section is interesting but the analysis is somewhat weaker than that in other sections

l. 511 – I complained about significant figures last year and I will again despite the authors' statements about different error levels in different agency's surveys. It should still be possible to make an educated guess at the accuracy of these counts – are they likely good to the nearest thousand, or to the nearest ten?

l. 1102 and elsewhere – the use of large quantities of herbicide warrant some discussion of the possible (or observed) effects on non-target organisms (other than other vegetation). Are the chemicals bioaccumulating? toxic to non-plant life? This may be covered in other chapter s or appendices and if so should be referred to here.

l. 1487 – I'm glad this was recognized – it's an almost universal problem with sediment traps in my experience, with efficiencies of <10% in high flow areas

l. 1719 – are blue-green algae an issue here at all?

WebBoard-Posted: 11/8/13

Most of the minor points were addressed quite well and the responses clarified many of the major points raised by the reviewers. A bit more thorough editing should fix most of the minor issues.

Main Points

- The chapter does a good job overall at explaining the experiments and describes some very impressive large-scale manipulative experiments.
- It may be helpful to include more conceptual diagrams to orient the reader and make clear the successes and missing parts of the research in future documents if space permits.
- Carbon dynamics and flux experiments could be improved by considering dissolved organic carbon fluxes.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 7

Level of Panel Review: Accountability
Reviewer: W. Dodds (AA)

WebBoard-Posted: 9/30/13

This chapter describes the intensive efforts to control exotic and potentially noxious species in the region. The overall effort and level of attention being paid to this issue, as well as the model for cooperation between government entities to allow for control is encouraging in the face of many expanding species that could well never be controlled. It is a bit frustrating that there is not a mechanism in place in Southern Florida that can allow for more rapid reaction to new threats, and can compel private property owner to act more strongly on existing threats. In the face of these challenges the SFWMD appears to be doing the best that they can. The Future Needs in Management and Control section is very nicely written and an excellent brief summary of lessons learned and philosophical roadmap for the future.

Individual Points

1. The Melaleuca results are promising as are some other biocontrol efforts. Hopefully the biocontrol agents will not expand their host range.
2. The vegetation mapping data (and all other data) section should probably contain links to the actual data and statements on compliance with Ecological Metadata Language. In addition, statements on alternative repositories for data would be useful. There is a tremendous amount of ecological information here, and it should be archived for the future in more than just SFWMD data.
3. In figures 7-5 to 7-8 it would be good to note what a “high” or a “low” level of cover actually is, in terms of % of community dominance, stems per square meter or some other measure.
4. Line 485 Is the Florida Invasive Species Partnership also involving tribes?
5. I appreciate the places with the links embedded in this chapter to follow up for further information, and would recommend doing this throughout.
6. Line 735 Be specific that Brazilian pepper is fire positive.
7. Line 770 top 10 according to who?
8. Line 802. Is this published data or a personal communication?
9. Line 994. Citation?
10. Line 1042. Interesting case of conflicting management outcomes.
11. Fig 7-31. Scale?
12. Line 1408 Don’t forget to update number.

WebBoard-Posted: 11/5/13

The authors have adequately dealt with the suggestions on this chapter. Overarching final comments are:

- The district is commended on their continued work and continued collaboration with other agencies to attempt to control non-indigenous species of concern in the area.
- While the accounting is good, these data are very valuable now and into the future. The data should be curated with contemporary standards for ecological data and made widely available. EML compliant data structures as well as considering mirroring through outside ecological or taxonomic data storage efforts will ensure that these data retain maximum usefulness.
- Continued careful work at biocontrol, mechanical removal, and chemical control of non-indigenous problem species is highly recommended as are continued efforts at early detection and where possible eradication or more likely control before the problem gets too difficult.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 8

Level of Panel Review: Technical
Reviewers: P. Dillon (AA); W. Dodds (A)

WebBoard-Posted: 10/2/13

General
As was the case in last year's report, this chapter includes detailed information about, and analysis of, measures implemented in water year 2013 under the Lake Okeechobee Watershed Protection Plan to reduce phosphorus and nitrogen loading to the lake, about the nutrient status of the lake including external loads as well as in-lake chemistry, and about the ecology of the lake with respect to submerged and emergent aquatic vegetation, phytoplankton, macroinvertebrates, fish, wading birds and the Florida apple snail.

The chapter is well-written, clear and concise. Again, the conclusions drawn are well-supported by the data presented. In general, the writers have produced an excellent summary of the ongoing work on Lake Okeechobee and its watershed.

However, as stated last year, the greatest concern related to this section remains the progress, or lack of it, made towards reducing the TP to the target value of 140 metric tons/year. Despite very substantial efforts to reduce loads, many of which have had significant success, and many of which have cost large amounts, the total phosphorus load is in the same ballpark as it has been for the past several years, in fact it increased substantially in the WY2013. This also led to an increase in the in-lake TP concentration. As was indicated in last year's review of this chapter, a path to meeting this target load needs to be described in detail and realistic scenarios for the future need to be developed and presented. The potential for very large internal P loads makes the desired changes in water quality even more difficult; however, on the positive side, notable progress is reported this year in terms of experimentation with methods designed to prevent release of sedimented P.

Again, more intensive efforts to quantify the nutrient load-trophic status response relationships through modeling are encouraged so that expectations with different target loads can be identified.

In terms of specific projects, the following comments are provided.

The Stormwater Treatment Area (STA) projects appear to be proceeding better than in the past. Implementation has and will lead to significant reductions in P leading to the lake. Incorporation of HWTT technology is an excellent idea and clearly (figure 8.7) shows much promise for additional reductions in load. The use of this technology should be expanded wherever possible.
The Dispersed Water Management program is an excellent, and clearly very successful, approach to assist in maintaining water levels within the desired limits. Based on what is reported here, both the public and private land components have been well-received and are working well. The Payment for Environmental Services approach is innovative and clearly successful, with the potential for expansion in coming years.

The Phosphorus Source Control program is really the core of the work that needs to be accomplished. Despite all of the efforts made to date, the loading (and as a consequence the lake concentrations) of P are still far above the target. There are, however, some hopeful signs. The implementation of agricultural BMP's seems to have been bought into thoroughly by everyone.

The SWERP rule will help, as will the controls being implemented on chemical fertilizers in urban areas, and manure and biosolid applications. The use of flocculating agents in source areas (it was excellent to see the evaluation of the different chemical P removal treatments that had been proposed, and even better to see how successful that the 2 tested were) and the use of in-ground chemical barriers to strip P from sub-surface waters are very promising in terms of their potential for widespread use. The next step, which isn't discussed at all in this report, is determining where to go with these findings, i.e. whether to implement them on a much bigger and broader scale. This may be essential if the target load is to be met.

The modeling remains problematic; the hydrology is reasonable but the P modeling, despite considerable "tweaking", is unsatisfactory. It is suggested that the modelers look into the INCA modeling framework developed at U. Reading and Oxford (UK). The original INCA was designed to model N flux through and out of watersheds while INCA-P does the same for P. Although these models were developed for the UK, they have been used in many countries including Canada, and have proven to be extremely useful. My understanding of the data available for the Everglades system leads me to think that INCA is ideal for this situation. It is very good at looking at source areas, evaluating the potential effects of BMP's, quantifying the links between climate, hydrology and nutrient flux. We have modified INCA-P to use in Canada on a large (725 km2) shallow lake, as well as all of its tributaries. There are many papers in the literature on INCA; if there is interest in following up on this, I can direct staff to appropriate papers.

As in the past, the water quality monitoring and the biological monitoring are well-conceived and appear to be well-executed (exception – see comment on line 1357 below). The lack of algal blooms, particularly of cyanobacteria, the increase in SAV with the decline of *Chara*, and the generally good diatom:cyanobacteria ratios are positive signs, although the increase in lake TP concentration is not. It is unfortunate that budget shortages prevented macroinvertebrate sampling in WY2013 as they are excellent indicators of water quality. It is suggested that macroinvertebrate data would have been more useful than detailed periphyton data. The fish and wading bird data seem to indicate continued recovery from the effects of severe weather events in past years.

The apple snail experiments are very interesting, although it is unfortunate that they were somewhat compromised by the escape from the test site. Nevertheless, results seem very promising so far.

It is good to see the thought put into the Strategies For Moving Forward section and the discussion on key activities. The reader is left with the thought that the target load will be very difficult to achieve but perhaps not impossible.

Finally, this is one of the largest and best coordinated sets of environmental monitoring and research experiment networks in the United States and perhaps the world. Making certain these data are available online in a stable source outside SFWMD would help ensure this work has a lasting mark. Links to the actual data in future versions could facilitate the process of making the data more obvious to outside parties. It could be useful for expert workshops to tie all this together from a science point of view. This may require external funding, but the background information is present for so many levels of ecological hierarchy, and the obvious next step is to work on the big picture and any potential emergent properties that might occur in such a large and complex system.

Specific Comments

l. 80 – the same paragraph expresses load in metric tons, followed by areal load in pounds per acre and concentrations in ug/L. It is time to be consistent with units. Since you already calculate loads in mt and concentrations in ug/L or mg/L, I suggest doing away with pounds/acre, acre-feet and other obsolete units

l. 137 - blue green algae is an old fashioned term, cyanobacteria should be used

l. 147 - better indicates a value judgment. Higher or greater? Better food for wading birds?

l. 155 – the summary says mean depth is 9 feet; this says 7.5. In any case use m
Table 8.1 - I would round off these areas – they can't be accurate to 7 or 7 figures; 4 maybe but even that is unlikely
Figures 8.3 and 8.4 are very useful – good

l. 360 – work described in chapter 6 seemed to demonstrate that SAV trapped nutrients better than emergent vegetation; why was the focus here not on establishing SAV?

l. 383 – the HWTT approach seems to be a good one; what coagulants were used here
Figure 8-7 - needs better figure legend, more descriptive and more complete y axis label

l. 1209 – why the switch to TP in mg/L from ug/L; surely accuracy is better than in 10's of ug as indicated by 0.02 mg/L

l. 1357 – figure 8-14; why were there no mid-lake station used for the water clarity measurements, just nearshore stations?

l. 1668 - it could be that diatoms are sensitive to allelopathic compounds produced by components of the SAV. It looks like they were low at the same time that the diatom cyanobacteria ratio was high.

Figure 8-32 - is this not based on mass of SAV? Also, it would be nice to see the plots of some of the ratios of C:N:P

l. 1956 - what about C:P ratios? This could independently suggest the degree of P limitation.
Figure 8-41 - it might be time to see a whole-food web model with SAV, epiphyton, phytoplankton, zooplankton, macroinvertebrates, fish and large avian predators. It seems like there is enough known to do this.
Figure 8-50 - what are the units? Water level?

l. 2318 - given that the apple snail has two species, it would be clearer if the species name or the full common name were used throughout the section on this.

WebBoard-Posted: 11/12/13

Closing Comments

The authors of chapter 8 have provided detailed and reasonable replies to all of the comments and concerns that we have posted. There are no points of disagreement between the authors and the reviewers. We appreciate that some of our suggestions would require additional funding which is not something in the jurisdiction of the authors of the chapter.

Findings and Recommendations

1. The Stormwater Treatment Area (STA) projects are working effectively and are producing better results than in past years. They are contributing significantly to reductions in total phosphorus loading to the lake. Additional work planned for the STAs will lead to more improvements in water quality.

2. The use of HWTT technology is effective and should lead to further reductions in P loading. The technology should be expanded where possible.

3. Despite all of the positive steps taken at significant financial cost, and the projects that have resulted in reduced TP loads, there has been little progress towards reducing the overall TP load to the lake nearer to the target value of 140 metric tons.

4. The phosphorus concentrations in the lake have increased, and the lake remains in a hypereutrophic state. However, algal blooms were infrequent, with cyanobacteria generally inconsequential as determined by the cyanobacteria:diatom ratio. The decrease in Chara along with the increases in SAV is also a positive result.

5. The re-release of sediment phosphorus providing an internal load of TP is of great concern. Notable progress has been made in the past year concerning experimental methods intended to reduce the possibility of this occurring.

6. The Dispersed Water Management program has proven to be an effective means of assisting with maintaining water levels within the desired boundaries. The Payment for Environment Services approach is also excellent and could be expanded in future.

7. The hydrologic modeling has been reasonably successful but the TP modeling has not been effective. It is recommended that alternate approaches to the nutrient modeling be explored. One such approach, the INCA modeling framework developed in the UK, might be suitable.

8. Data pertaining to wading bird foraging and nesting success rates and to the fishery showed improvements from previous years, and were indicative of further recovery following the major storm events of earlier years.

9. The macroinvertebrate monitoring has been cut for budgetary reasons. However, this would likely yield more useful information about the status of the lake than some of the detailed periphyton work, and should be re-instated.

Key Recommendations

1. The lack of progress made towards reducing the TP loading to the target value of 140 metric tons/year remains of great concern. The TP load is similar to what it has been for the past several years. The section on Strategies for Moving Forward is a welcome addition, although a more detailed plan for meeting this target load is needed and should be described in detail in the next year's report.

2. Because of the potential for very large internal P loads occurring in future, efforts to quantify the potential for internal loading and the factors that affect it need to be continued. Additional investigations into the feasibility of in-lake sequestration of phosphorus should be continued and expanded as such treatments may be necessary in future.

3. Improved nutrient load-trophic status response relationships through modeling efforts are needed. FDEP and SFWMD should consider updated trophic status models such as, but not only, the INCA modeling framework developed in the UK. While nitrogen is not the primary management target, efforts to control N, particularly if they help P control, should be considered valuable as increases may alter biotic integrity even in the absence of stimulation of total algal biomass.

PUBLIC COMMENTS ON DRAFT VOLUME I, CHAPTER 8

Audubon FLORIDA

EVERGLADES FOUNDATION CELEBRATING 20 YEARS

November 6, 2013

Lesley Bertolotti
South Florida Water Management District
3301 Gun Club Road
West Palm Beach, FL, 33406

Dear Ms. Bertolotti:

The draft 2014 Lake Okeechobee Protection Plan Three-year Update (2014 LOPP Update) is an important opportunity to improve Lake Okeechobee's water quality.

As in previous updates, the 2014 LOPP Update should be a blueprint of projects and programs to achieve the Lake's water quality goal, with a clear timeline for implementation and funding requirements.

Background

Florida Law requires that every three years, the LOPP update must evaluate any further load reductions necessary, and identify modifications to facilities in the Lake Okeechobee Watershed Construction Project in order to achieve compliance with the Lake Okeechobee Total Maximum Daily Load (TMDL).[1]

The 2000 Lake Okeechobee Protection Act (LOPA) mandated a TMDL of an average of 140 metric tons (mt) of total phosphorus (TP) per year to the Lake be met by January 1, 2015.[2] However, the most recent five-year average phosphorus load was 451 mt (WY2009–WY2013),[3] which is 322% of the water quality goal.

A tremendous amount of nutrients continues to enter the Okeechobee watershed from agricultural and urban sources. An estimated 4,256 metric tons of new phosphorus are added

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[1] 373.4595(3)(b)(3), Florida Statutes.

[2] Lake Okeechobee Protection Plan 2011 Update states, "The TMDL was adopted by the FDEP in 2001 and was established in accordance with Section 403.067, F.S., and consists of 105 mt per year of TP from the watershed and 35 mt per year from atmospheric deposition (e.g., rainfall and dryfall)."

[3] SFWMD South Florida Environmental Report 2014 Draft, pg. 8-27.

to the upstream watershed by human activities each year.[4] Trends show no significant reductions in phosphorus loading in many of the Okeechobee sub-watersheds.[5]

Recommendations

Audubon Florida and the Everglades Foundation offer the following recommendations to refine the vision for Lake Okeechobee's future. Moreover, both organizations are committed to continue working with state agencies and stakeholders to improve water quality north of Lake Okeechobee at the source through agricultural and urban regulatory improvements. Audubon Florida and the Everglades Foundation reserve the right to submit additional comments later.

1. Explain how the listed key activities moving forward will lead to meeting the TMDL.

"Key Activities Moving Forward" on pgs. 8-103 to 8-109 of the 2014 LOPP Update lists a variety of actions, including but not limited to Lake Okeechobee Basin Management Action Plan development, the continued implementation of source control programs, and dispersed water management. However, the 2014 LOPP Update does not detail how these actions will lead to the necessary load reductions to meet the Lake Okeechobee TMDL.[6] The plan should include information on how each of these actions, individually and cumulatively, will lead to meeting the Lake's TMDL.[7]

2. Include a timeline for implementation.

The Northern Everglades and Estuaries Protection Plan authorizes and directs the coordinating agencies to establish priorities and implementation schedules for the achievement of total maximum daily loads.[8] However, a timeline for the implementation of the "Key Activities Moving Forward" does not appear in the 2014 LOPP Update. Using terms such as "current", "near term" and "long term" is inadequate. Including a timeline for implementation is particularly important in light of the upcoming TMDL deadline of January 1, 2015.[9] The 2014 LOPP Update must re-state the commitment of the State agencies to work toward meeting this deadline.

3. Include funding requirements to implement the LOPP.

Most importantly, the 2014 LOPP Update must clearly articulate funding requirements. Florida law states, "The Legislature finds that a continuing source of funding is needed to effectively

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[4] The HDR Team. 2010. *Nutrient budget analysis for the Lake Okeechobee watershed: final comprehensive report.* SFWMD, West Palm Beach.

[5] SFWMD 2011 SFER, Chapter 10-49.

[6] 2014 Draft LOPP, pgs. 8-103 to 8-109.

[7] For a possible template, refer to and update the 2007 SFER Table 10-7, which showed "Ongoing and future phosphorus reduction activities in the Lake Okeechobee watershed", with the estimated percent of total load reduction to meet the TP (TMDL). 2007 South Florida Environmental Report, pg. 10-30. See also Table C-1 in the 2011 Lake Okeechobee Protection Plan Update, Appendix c-2.

[8] 373.4595(3)(h), Florida Statutes.

[9] 2011 Lake Okeechobee Protection Plan Update, pg. 2.

implement the programs developed and approved under this section."[10] Thus, it is incumbent on the drafters of the LOPP 2014 Update to explain what funding is required. In the 2011 LOPP update, the lack of funding was cited numerous times as a reason why programs and projects could not proceed as rapidly as desired.[11] In this draft, there should be an explicit discussion of the funding requirements necessary to complete the plan. Table 8-20 on pg. 8-110 and 8-111 in the Draft 2014 LOPP update should be expanded to include funding needed to complete the plan. Funding requirements for the full implementation of best management practices throughout the watershed should be clearly articulated. In addition, funding for edge-of-farm retention and detention measures as well as needed Stormwater Treatment Areas (Lakeside Ranch - phase II) should be increased.

Conclusion

Charting a course toward a clean Lake Okeechobee will require bold steps implementing the Lake Okeechobee Protection Plan while improving the regulatory programs targeting urban and agricultural sources. Funding, timelines, and clear goals are vital for success. Maximizing agriculture and urban source reductions through updated regulatory frameworks is essential. The clock is ticking on Lake Okeechobee's TMDL. Definitive actions must be taken now before it is too late.

Sincerely,

Jane Graham, Esq.
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[10] 373.4595(1)(k), Florida Statutes.
[11] 2011 LOPP, see pg. 51, 61, and 62.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 9

Level of Panel Review: Technical
Reviewers: W. Dodds (AA); O. Stein (A)

WebBoard-Posted: 9/30/13

Dodds technical review (AA)

This is a fascinating chapter because the Kissimmee River Restoration Project is one of the largest and most ambitious ecological restoration projects in the world, particularly with respect to linked river/ wetland restoration projects. This type of restoration is not expected to yield results immediately, and a major criticism of many restoration efforts is that they do not follow up and fail in the long-term. Thus, the continued accrual of data for years after this restoration has fundamental value with respect to not only managing this system, but also to the field of ecological restoration/ management as a whole. While the project is not complete, and the staggered nature of the restoration presents complexity, the ongoing data still have value.
The authors of this chapter are to be commended for presenting results in a very logical, organized and easy to understand way. This is a very complex document overall, and this section is particularly well put together. It is good that the results from prior reports are referenced there, most of the other chapters that I read could have used more of this. The accompanying table allows visualization of some areas (periphyton, turbidity, macroinvertebrates, shorebirds) that should be considered for updating. This mirrors comments by (A) level reviewer Stein.

Minor points.
1. Line 345 How certain is it that bouts of anoxia were not common historically before the hydrology was altered?
2. Figure 9.7 usually rainfall is represented as daily bars as opposed to a line connecting points.
3. Figure 9.8 B, what is the frequency of dissolved oxygen measurements? These are daily means according to the text, but what is the measurement frequency? Also the words "daily mean" should also be included in the figure legend. Also Dodds seconds the call by Stein (A) reviewer to work out the mechanisms of low DO.
4. Line 447. At least one of the chapter is using SI units consistently! Except figure 9-16
5. Line 723. It is also worth noting that the sub-basins most contributing to Okeechobee nutrients are those the closest to the lakes, implying some degree of retention is occurring.
6. Line 726. I would like to see some measures of denitrification rates associated with various sediment types. The total nitrogen concentrations at the river sites are pretty high.
7. Line 847. Interesting observation. Was the arial herbicide to control invasive species?
8. Figure 9-24 Given the variance over the years, it is pretty clear that the two baseline years were not near enough to establish changes over time from the restoration.
9. Line 1073. Good point.

Comments by Otto Stein "A" reviewer
Note that Appendix II indicated that the level of review is at the "Technical" level.
The review is divided in to several sections. First are several broad questions and comments regarding the interpretation of reported results followed by questions about specific sections, sentences, phrases, etc. This is followed by comments on figures and tables and lastly, editorial suggestions.

Broad questions and comments that should be addressed

The chapter provides an overview of the current year's efforts to assess the success of activities designed to restore the ecological health of the Kissimmee River to a pre-channelization condition. The premise is that the ecological health of the river basin was severely compromised by channelization of the river into the C38 canal but that obliteration of the middle section of this canal and restoration of the original river channel will reconnect the restored channel to its associated floodplain in that section. This will in turn allow the associated assemblages of flora and fauna to revert to something similar to the pre-channelized basin. In addition to activities associated with this major restoration project, other topics such as the restoration and overall health of some of the upper lakes region of the basin are provided.

In general the chapter provides an appropriate summary of the current year's restoration construction and monitoring activities. As in previous years, only some of the activities associated with the KRREP monitoring are discussed, presumably because other activities were not conducted this year. As mentioned in last year's review, Table 9-3 provides the reader with a "road map" of what activities were conducted this year and what activities are presented in previous Environmental Reports and is thus very useful. This reviewer does question the protocol used to decide what monitoring activities are conducted in any given year. As mentioned on lines 188-189 successful restoration depends restoring prechannelization hydrologic conditions. Significant construction activities to achieve that, especially removal of the S65C control structure in the middle of the restoration section of the canal, as well as implementation of the Headwaters Regulation Schedule are still in the future. Considering that only a small portion of the restoration section can be considered complete at this time, it is not clear why some monitoring activities are conducted at all. The hydrologic activities make sense and a case can be made that monitoring of water and chemical properties including DO, N and P are prudent, but the evidence collected to date on bird abundance and reproduction rate clearly indicates that there is little measurable success of these expectations. This is probably to be expected. Unless these data are collected for other reasons it would make more sense to focus energies on construction and baseline monitoring of parameters that are likely to respond more rapidly to the restoration activities.

There is a clear correlation between higher discharge and longer floodplain inundation hydroperiods to lower DO concentrations in the resorted channel and there also a correlation, those weaker between the same hydrologic conditions and elevated N and P concentrations and/or loadings. While the same trends, at least for DO, are apparent in baseline and reference reaches, the potential for a conflict between meeting stated hydrologic and chemical expectations should be of concern to the District. Does the District have a plan if further monitoring and research concludes that these stated expectations (desired metrics) are incompatible? Is there a hierarchy that could be employed? Perhaps construction completion will minimize these observed incompatibilities, but the data collected so far is not encouraging.

Specific questions and comments by line number

345-352: Clearly the causes of hypoxia are not understood, nevertheless the data collected to date suggest that even when restoration is complete meeting the expectations for DO will be a challenge. It is not clear why more research is not being directed to this concern considering that fish assemblages are dependent on sufficient oxygen and the return of wading birds and perhaps waterfowl is likely dependent on sufficient fish prey. In these lines four hypotheses are provided for reduced DSO concentrations and these should be used as a starting point for research. That said, some comments on them are warranted. If the decrease is due to an influx of BOD from the floodplain, I would expect there to be a lag phase of a few days before DO started to decline as heterotrophic microbes responded to the increase in food source. If this is the cause, increased wetland coverage of the floodplain might exacerbate the problem by increasing primary productivity of the floodplain. If it is due to upstream dilution, tracer studies and/or volumetric balances could be uses to determine its magnitude. If it is due to photosynthetic inhibition, a comparison of diurnal cycles at high and low stages could be compared. Groundwater influx seems the least likely cause as elevated river stage probably minimizes groundwater efflux, but this could be evaluated by comparing the chemical signature of the groundwater to the surface water at times of high and low DO.

466-472: I do not understand what data are being compared here. The coefficient of variation C_v is defined as the standard deviation divided by the mean of the analyzed data. If the data consists of mean monthly flows, I understand a C_v between months over an annual cycle (one value) but do not understand how it could be different for different months. Is a varying C_v for different months used on data for daily flows *within* the month?

478-479 (and subsequent discussion): Interpretation of this Expectation #3 is quite difficult. What is meant by "average ground elevation"? Does this mean a cross-section? If so, how far from the river does it extend? What is the depth of the channel at the cross section? How is an appropriate cross section determined? The expectation should be more explicit and the methodology used to between cross-section to measure compliance should be discussed.

542-593: It is clear from Figure 9-13 that "baseline" reaches not only have a lower average DO in either season (compared to "reference" blackwater reaches) but also vary more between the wet and dry seasons, generally due to even lower concentrations in the wet season. Could the Kissimmee River have a different response than the selected reference streams or is this observation due to more disturbance?

542-593: Additionally the discussion here seems to conflict with the earlier discussion of DO in lines 309-344. Here it is suggested that restored reaches have improved DO levels but the earlier discussion talk of two hypoxic events and DO level low enough for fish kills. While the locations might be different or other non-addressed factors not fully explained but the reader comes away with a very different conclusion on DO concentration in these two sections.

Figure and Table comments

The panels of Figure 9-11 are very small and layout results in too white space. Enlarge and square the layout

The legends on figures 9-16 through 9-20 are too small to see which bars represent which parameters. Part of the issue might be the little white bar at the top of the bars which carries over to the legend.

Editorial page and line comments

39-44: A very long sentence, try replacing "and" on line 42 "with in addition to"

65: for "a"

WebBoard-Posted: 11/6/13

The reviewers feel that the authors have satisfactorily answered most of our major points.

AA review response to author responses to comments #5 and #6. These comments were specifically directed toward assessing the potential for instream retention of both N and P. Research indicates potential for such removal from small scales to large (e.g. Alexander et al. 2000, Mulholland et al. 2008). This is a potential area for future research or monitoring that should be considered.

Alexander, R. B., R. A. Smith, and G. E. Schwarz. 2000. Effect of stream channel size on the delivery of nitrogen to the Gulf of Mexico. Nature **403**:758-761.

Mulholland, P. J et al. 2008. Stream denitrification across biomes and its response to anthropogenic nitrate loading. Nature **452**:202-U246.

The A reviewer responds to the issues on Comment #10:
The authors have provided a very detailed response to this comment which was intended more as an overview of the entire approach to monitoring the KRREP. The level of detail in the response is appreciated but suggests the authors might have missed the broad overview intent of the comment. No one is questioning the need for interim monitoring or that there has been movement toward meeting several, if not most, expectations, but this reviewer did question the implicit hierarchy and/or emphasis of what is being monitored. For example, it could be premature to monitor "higher level" ecological indices such as bird and vegetation surveys, which must respond slowly to hydrologic changes, when other indices such as hydro-period and water quality parameters would likely have a more immediate response. That said, there may be other reasons for such monitoring such bird species being high visible and important to the public and/or monitoring baseline conditions at locations that will be subsequently remediated.

As for the comment that some expectations are likely in conflict, the authors' response "We anticipate that through adaptive management these metrics will not be found to mutually exclusive…" might be paraphrased as "we hope this is not a problem". Perhaps that is the only response possible at this point in an incomplete restoration project, but prudent management requires that an adaptive approach consider the available data and at least recognize that, unless the currently monitored data trends change, a potential problem is looming on the horizon.

In general the overarching points on this chapter are:

- This is a very impressive large restoration project and the SFWMD continues to make progress in assessing the effectiveness of this restoration.
- The low dissolved oxygen, its exact causes, and potential for restoration remain a problematic issue. This is a key issue with regard to both biotic integrity and the potential for P deposition and release. The district should continue to focus on and refine their understanding of this issue.
- The potential for instream nutrient removal is one potentially important area of emphasis. Given all the downstream issues related to nitrogen and phosphorus transport, research in this area (either empirical or literature based modeling) could be useful.
- While it is understood that the restoration is still in progress, an adaptive management approach could be useful if some management approaches appear not to be fruitful or even harmful. However, some caution is warranted with this view because the system is variable enough (e.g. pre-treatment variance) to make short term assessment of responses to management decisions difficult.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 10

Level of Panel Review: Accountability
Reviewer: V. Novotny (AA)

WebBoard-Posted: 10/4/13

Accountability Review *(for chapters that are of a more routine nature)*
Questions addressed by the review:
– Does the draft document present a defensible account of data and findings for the areas being addressed that is complete and appropriate?
- Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report?
- Are findings linked to management goals and objectives?

General Comments
The chapter is focusing on two important estuaries, the St. Lucie (SLE) and Caloosahatchee River (CRE) Estuaries and investigates the impact of the (intermittent) freshwater releases from Lake Okeechobee where these rivers/estuaries originate and it is the largest freshwater contributor. Annual reporting on the status of both estuaries is mandated by the Northern Everglades and Estuarine Protection Plan (NEEPP). The lake outflows have a profound influence on circulation and transport, water quality, and biotic resources of the two estuaries. The first part of the chapter describes the construction projects, the middle part assesses the current physical (water quality and hydrology) and ecological nekton, oysters and submerged aquatic vegetation (SAV), status of the two water bodies. The final part describes the results of several scientific cruises studying the effect of lake release on circulation, water quality and biota that lead to the development of Adaptive Protocols guiding the lake release to protect the ecological value of the estuaries.

Hydrologically, the rivers are not a part of the Everglades system and the key issues are different from the Everglades where the main problem appears to be excessive, mostly nonpoint loads (surface and atmospheric) of nutrients, mercury and sulfates which was covered in Chapter 3A, 3B and 4. Consequently, NP abatement was not included in Chapter 10 and almost all projects described in this chapter are structural for manipulating the flows which have no effect on the Everglades. However, both estuaries have very high ecological and commercial value for fish and shellfish development and habitation. Because Lake Okeechobee releases have not been continuous, salinity of the estuaries varies which may have adverse effects. Furthermore, the flow contributions from the lake have decreased and most of the flow originates from the watersheds. The reduction of flows resulting in increasing the extent of and magnitude of salinity was beneficial for the oysters' survival.

In addition to salinity increase and stabilization, the Chapter 4 review showed that participation of land owners in the two watersheds in Total P load reduction programs was still low and this chapter did not report any increases of participation. As it was pointed out in the last WY 2012 review, the reason for reduced concentrations of P and N in the estuaries was mostly due to reduced release of nutrient rich water from the lake. This has positively impacted the oyster densities because the fresh water discharges are important as the section of the estuary affected by them is apparently the most productive part of the estuary. However, the chapter pointed out that concentrations of N and P increased in the WY 2013 over those in the previous two years. The SLE and CRE portions of Chapter 10 provide summary information on watershed rainfall, freshwater inflow to the estuaries, salinity patterns, nitrogen (TN) and total phosphorus (TP) loads, estuarine water column concentrations, patterns of submerged aquatic vegetation (SAV), community composition, and status of oyster reef habitat. Monitoring data from both estuaries was summarized by water year.

The SFWMD continues to develop Adaptive Protocols on the effects of low releases and reasons for them and connectivity to the Everglades system which are also fed by Lake Okeechobee discharges. In addition to the Protocols the district is developing the Watershed Protection Plans for the estuary watersheds. Each plan will have three components: (1) Pollutant source control program; (2) The construction projects, and (3) Research and water quality monitoring program. These programs appear to be similar to the other watersheds which drain into the Everglades. *Pages 10-7 to 10-15* describe mostly construction projects in both watersheds conducted on regional , subregional and local levels addressing both water quality and storage and aimed to improve hydrology, water quality, and aquatic habitats. The largest project, and probably the one with the greatest effect, will be the Allapattath Natural Water Storage in the St. Lucie watershed encompassing 42,000 acres (170 km2) identified for use as alternative storage, rehydration, habitat restoration, and incidental water quality treatment. About ½ of the area has already been acquired. On the local level the Watershed Dispersed Projects identifies landowners with who the District will work cooperatively to implement storage and treatment.

In the Caloosahatchee River watershed, the West Basin Storage Reservoir project is even larger (170,000 acres = 70 km2) aimed at storing excess Lake Okeechobee releases and storm water needed to restore and maintain the estuary. The reservoir will also store peak flows during the wet season and provide flows during the dry season to maintain optimal salinity on the estuary. The local disperse projects are similar to those in the St. Lucie watershed. Many other projects are planned and being implemented in the two watersheds, almost all for flow manipulation. In addition to the flow manipulation project, several small nutrient reduction projects were also described in the chapter.

Pages 10-15 to 10-30 describe extensive ongoing environmental monitoring in the St. Lucie watershed. The first impression one may obtain is that monitoring is yielding important information on the status of the estuary. The focus of the monitoring activities was to study the hydrology effects on salinity that affects the oysters and submerged aquatic vegetation. *Figure 10-5 on page 10.21* shows tremendous salinity variations that certainly will have an impact of biota that requires certain salinity ranges. It was also found that TP and TN loadings are closely related to hydrology (Figure 10-7) that can be impacted by the releases from Lake Okeechobee.

Apparently, in WY 2013 the TP and TN concentrations and loads were elevated and increased in the last two years but the authors stated that they were still “below the long term averages”. This is not a good comparison because the long term average reflects high and unacceptable nutrient concentrations. Developing 5 year moving averages, as it was done in some other chapters of the Florida ER, gives a better picture about the progress towards improvement. It is interesting to note that the effect of Lake Okeechobee which is eutrophic, is diminishing and the load from the watershed is increasing, to the point that the SLE watershed contributed relatively more TN and TP to the SLE than Lake Okeechobee in WY2013 and 100% in WY 2012. Figure 10-8 shows the osculation of the Total N and Total P and, clearly, the Total P is above the target concentration. Maximum TP concentrations were observed with increased freshwater inflow.

What is missing in this discussion on otherwise excellent reporting on monitoring, is what is the limiting concentration that could affect algal development and the trophic status of the estuaries. Marine waters are generally nitrogen limited. The monitoring also found that salinity levels were generally favorable to the oyster population (*Figure 10-9 on page 10-28*).

In the summary, the monitoring found that in WY 2013, contribution of Lake Okeechobee decreased to very low levels, yet TP and TN loads are correlated to fresh water inflows which today are mostly from the watershed. This gives an incentive to developing TMDL and watershed NP pollution reduction programs which is lagging in the estuarial watersheds behind Everglades' watershed south of Lake Okeechobee. The fact that the Total N and Total P are exceeding the target concentrations makes this urgent. The monitoring also focused to a great extent on the development of sea grasses (part of SAV) which seem to be healthy but this would be beyond the reviewers understanding of marine biology.

Pages 10-31 to 10-46 describe monitoring of the Caloosahatchee River, using the same format, same figures and same number of pages as that of the St. Lucie River. Hence, at first look, the monitoring results are very similar. Again, the monitoring focus was on sea grasses and oysters.

Figure 10-15 on page 10-38 shows great variation of salinity at Fort Meyers whereby, similarly to SLE, salinity is inversely proportional to fresh water inflow. Similarly on Figure 10-17, Total P and N concentration follow the magnitudes of the total flow. Figures 10-18 and 10-19 show relatively steady TP and TN concentrations that are on a lower side which may also reflect on relatively low Chlorophyll a concentrations.

The summary findings generally express average conditions for the estuary, somewhat short of freshwater flow so that flow interventions were necessary to maintain salinity favorable to the oyster population. Similarly to SLE waters, the Total N and P were elevated over the WY 11 and WY12.

It looked to the reviewer like there were no major problems with the CRE except with the low focus on reducing TP and TN loads form the watershed which may become a problem in the future. Some signs of the problem were already revealed in the scientific surveys (Adaptive Protocols) which will be subsequently pointed out.

Adaptive Protocols Research Study (*Pages 10-47 to 10-60*)

This is a very important and to some degree unique estuarine study designed to reveal the effect of freshwater input on the ecological health of the water body. The estuaries depend on the upstream fresh water reaches that bring nutrients. As a result, it is expected the phytoplankton develop in the upper reaches and the overproduction can stimulate algal blooms, but rapid flushing by the tides moves the phytoplankton downstream and reduces its settling. The goal of the study was to collect enough data so that an optimum between the nutrients inputs, growth of phytoplankton and grazing, and trophic transfer to higher trophic level can be found. The fresh water flows from Lake Okeechobee are now manipulated.

There were nine flow cruises conducted between January and May in the roughly 50 km section of the CRE between the S 79 sections near the outlet from Lake Okeechobee and Sanibel Island. The lake outflows have an effect on the salinity, circulation, nutrient levels, water quality and the health of the biota in the estuaries. The lower sections are impacted heavily by the Gulf of Mexico (CRE). In this study, nine pulse releases were made that were followed by the research boat collecting data. The flow through system of collecting data was unique and made it possible to collect data as the research vessel was travelling downstream. Stops were made to collect vertical profile data on temperature, turbidity, salinity, pH DO, and Chlorophyll a. Information on nekton composition, sea grasses density and fauna parameters were also collected.

Page 10-52, The biological data were evaluated by and synthetized using the Shannon Wienner diversity index. This index is relatively old and new more representative biotic indices were developed for fresh water bodies by James Karr and followers. Obviously Karr's Index of Biotic Integrity requires fish shocking and similar indices collecting benthic organisms are directly not applicable but there is some subjectivity in using the Shannon – Wienner index. That is why this index is now rarely used for fresh waters. Furthermore the parameters p and R were not defined.

The results summarized in *Table 10-11 on Page 10-52* revealed a zone of higher turbidity and Chlorophyll a (a cause of higher turbidity) around the E Km (kilometer) 18. This was confirmed on Figure 10-25 on Page 10-54 which shows a zone of high turbidity and Chlorophyll a between Km 10 and 25. At its peak one could classify the conditions as eutrophic. This also coincides with the zone of the significant change of salinity and emergence during some cruises of density currents. The chapter does not go beyond reporting the data but the readers would appreciate presenting some hypotheses why this has occurred. Could it be linked to the nutrient levels or to the change of salinity which stressed mostly freshwater phytoplankton species? The reader can only guess.

In the Summary on *Page 10-6*0 the authors state that chlorophyll a concentrations were low. Actually, they were quite high during some cruises in the zone of transition from fresh water to higher salinity marine conditions. However, the authors emphasized in the chapter several times that marine life depends on the productivity in the upper fresh water section. The authors could express their opinion as to whether or not having a higher productivity eutrophic upstream fresh water reach is good .

Some specific editorial comments

Some lettering on Figures 10-2 and 10-3 are difficult to read.
Figure 10-4 needs a conversion from inches/day to mm/day. Also "inches/day" appears in the caption twice
Table 10-4 needs a conversion factor to mm or cm.
Figure 10-6 needs unit identification on X-axis (cfs)
Figure 10-14 needs conversion factor to mm. Also remove (d-1), it is redundant.
Table 10-6 needs conversion factor to mm or cm.
Table 10-7. Be consistent. Include conversion from acre-ft to m3 as it was done in the other tables
Figure 10-15 Conversions from cfs to m3
/sec is needed in the caption. Same for Figure 10-16
Table 10-9 Conversion from acre-ft is missing.
Figure 10-27 Conversion from cfs to m3/sec
Figure 10-29 Identify stations also by Km

Overall Assessment

As in the last year SFER, this chapter is well written and the flow of writing is logical and relatively easy to follow. It could be ranked as the best of the chapters this reviewer ranked of this year's report. It is rich on substance and the Adaptive Protocol Research Study is a high level scientific document. Probably in this report the authors did not venture much from simply describing the data to interpretation and synthesis, which most likely is coming in peer reviewed articles they should write or are already writing.

One strategic problem (not attributed to the authors) is the lack of implementation of BMPs in the watershed that would reduce the potential eutrophication salinity transition zone. Unlike in the other South Florida Zones discharging into the Everglades, participation of dischargers in BMP phosphorus and nitrogen (the estuaries are most likely nitrogen limited) load reducing programs is very low and most practices reported in this chapter were structural, many focusing on flow manipulation and not on load reduction.

Hence, in conclusion this document represents a defensible account of data that appears to be complete and appropriate. The data presenting is thorough and comprehensive. This report is very strong on presenting data and information on the actions undertaken during the WY 2013. However, because of the cruises, being done just recently, a full synthesis of the collected data and forming hypotheses was not done. One may look forward to peer reviewed articles and special reports by SFWMD that will significantly contribute to the understanding of the two important Florida estuaries and their interactions with Lake Okeechobee. The findings of the Adaptive Protocol research will definitely lead to management decisions but this was not done in this report.

WebBoard-Posted: 11/4/13

The chapter focused on the St. Lucie (SLE) and Caloosahatchee River (CRE) Estuaries and investigated the impact of the (intermittent) freshwater releases from Lake Okeechobee where these rivers/estuaries originate. The lake is the largest freshwater contributor. It also addressed the effects of nutrients on the eutrophication status of these important water bodies. Annual reporting on the status of both estuaries is mandated by the Northern Everglades and Estuarine Protection Plan (NEEPP). The chapter also described in detail the data obtained by scientific boat cruises on the Caloosahatchee River.

The reviewer found the chapter as being very strong on presenting data and information describing the actions undertaken during the WY 2013. However, because of the cruises being conducted just recently, a full synthesis of the collected data and forming hypotheses was not included in the chapter. Nonetheless, the chapter was well written and could be ranked as the best of the chapters of this year's reports ranked by the reviewer. It is rich on substance and the Adaptive Protocol Research Study was a high level scientific endeavor.

The authors provided qualified responses to all comments and promised to incorporate in the chapter proposed modifications and editorials. There is no need to review and argue all comments and responses. The key discussion bullet points follow below.

Discussion of responses and findings

Hydrologically, the rivers are not a part of the Everglades system and the key issues are different from the Everglades where the main problem appears to be excessive, mostly nonpoint loads (surface and atmospheric) of nutrients (primarily phosphorus), mercury and sulfates which were covered in Chapter 3A, 3B and 4. The nutrient problem for the two estuaries is different from that of the inland freshwater bodies in the Everglades system that includes Lake Okeechobee and its tributaries, Everglades Conservation Areas and the Everglades National Park which are strictly phosphorus limited. As the chapter documented and the authors in their response to Comment 2 by the reviewer pointed out, ascertaining the limiting nutrient for these estuaries is not straightforward. Most estuarine water bodies are N limited and the bioassays confirmed this fact also for the two estuaries. Nevertheless, N and P cycling in these water bodies and the ratios of bioavailable dissolved nutrients, DIN/DIP, are different from the ratio of total concentrations, TN/TP. Hence the review and the authors' responses concluded that both N and P should be controlled and restricted.

Chapter 10 documented that during the WY 2013, the flow and nutrient inputs from Lake Okeechobee were relatively small and most of the nutrient input originated from the watersheds of the estuaries. However, in the Comments 3 and 7 the reviewer stated that the watershed nonpoint pollution reduction programs in the two estuaries are lagging behind the programs in the Everglades watershed south of Lake Okeechobee. As a matter of fact, the review of Chapter 4 of the Volume of the SFER also revealed, based on the information provided by the authors of Chapter 4, that participation of the dischargers in the NP programs managed by the SFWMD was very low. This was explained by the fact that the NEEPP act was enacted by the legislation relatively recently (in 2007) and the NP source control programs are still in the initial phases and recruiting participants is continuing. The fact that the Total N and Total P are exceeding the target concentrations puts urgency on expedited inclusion of the nonpoint source areas into the programs. Also, the difference in participation between the SFWMD programs (low participation) and permitting and those by the FDACS (high participation) is great and SFWMD needs to catch up.

The authors conducted and participated in nine cruises conducted between January and May 2013 in the roughly 50 km section of the CRE between the S 79 sections near the outlet from Lake Okeechobee and Sanibel Island as a part of the Adaptive Protocols Research Study. These cruises generated a large data base on the hydraulic (stratification, tidal exchanges), chemical and ecological dynamics. This data base after synthesis will be invaluable for developing the management protocols for releases of water from Lake Okeechobee and for potential developing of TMDLs for the estuaries. While the synthesis of the data presented in the chapter is brief, the authors pointed out and quoted several technical reports and publications already published in peer review journals that are relevant to the chapter.

Key Findings

- It is apparent that in the WY 2013, the impact of the Lake Okeechobee releases into estuaries represented only a small portion of the nutrient input. Most of the N and P loads originated from the watershed of the estuaries.
- The estuaries are generally divided into three sections: (1) the freshwater section dominated by the releases from Lake Okeechobee, (2) the tidal portion with a steep salinity gradient, and (3) the coastal estuary section. The transitional zone between the freshwater and tidal sections appears to be the most productive but the scientists could not state whether or not it was beneficial. Under normal conditions the productivity of this section is beneficial to the downstream estuarine and marine life, but recently in the St. Lucie estuary and also in the Caloosahatchee River harmful algal blooms have developed. High chlorophyll- *a* concentrations were also measured in the transitional productive zone of the Caloosahatchee River estuary during some cruises. This situation gives a warning to the authorities that the nutrient inputs, not just flow hydrology, have to be controlled and reduced.
- Most of the control measures implemented in the last years included construction measures for flow manipulation and storage. The NEEPP legislation calling for developing regulatory and voluntary nutrient source controls was passed relatively recently. Unlike the programs for the Everglades areas south of Lake Okeechobee depending mostly on relatively successful regulatory measures to reduce the phosphorus input, NEEPP considers a mix of regulatory and voluntary participation which has not yet fully materialized.
- As a result of the insufficient participation and initial phase of the source control programs, implementation, reduction of flow weighted median congregations in the

estuarine waters were not observed and loads from some subwatersheds are actually increasing.

- The Adaptive Protocols Research Study cruises presented a unique opportunity to evaluate the effects of short-term inflow strategies on water quality and plankton abundances during the dry season. This study focused on the synoptic timescale to assess effects of short-term pulses of Lake Okeechobee derived fresh water on water column ecological attributes along the length of the CRE and contributed significantly to the understanding of hydrological, chemical and ecological dynamics of the estuary. This study resulted in a number of quality technical reports and peer reviewed articles published in impact journals that advanced the state-of-the art knowledge on the subtropical estuaries impacted by anthropogenic flow manipulation and elevated nutrient inputs.