Appendix 1-3: Authors' Responses to Peer Review Panel Comments

During September–November 2015, the peer review panel posted their comments on draft Volume I chapters on the 2016 South Florida Environmental Report (SFER) Web Board (www.sfwmd.gov/webboards); no public comments were received during the review period. This appendix includes authors' responses to panel and public comments. The first response is a global one, pertaining to comments made on several chapters regarding units of measurement. This content was not edited by the SFER production staff and appears verbatim as posted on the web board.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, UNITS OF MEASUREMENT

Garth Redfield

Level of Panel Review: Not applicable
Reviewers:

Comment #1: (Chapter 3) They are consistent with units, using mostly the SI unit system, and provide SI conversion to the US systems when needed. However, in the previous 2015 SFER (Chapter 3A) this reviewer pointed out the inappropriate use of the odd unit for volume (kacre -ft = 1000 acre-ft = 1.233 x 106 m3). Checking Google, this unit is nowhere used or defined. The closest term found is Nacre, which is a shiny mineral excreted by shellfish. The authors and the agency should reconsider using this odd unit. The pitfalls and unreasonableness of this odd unit were pointed out in the last year review of the 3A Chapter in the 2015 SFER. The same comment on inappropriateness of using made-up, not accepted units extends to other chapters of the SFER (e.g., Chapter 2). Generally, agencies should abandon trying to invent their own units and, in today's world, use and provide conversion factors if both unit systems are intermixed or only US units are used.

Comment #2: (Chapter 4) In reference to Tables 4-1 and 4-2 and other follow up information of results throughout the chapter it is evident the authors are reporting the results mixing US and metric units. They provide conversion factors between the unit systems in the tables. Ideally, SI (metric) unit system with conversions to the old US unit system in parentheses should be used. Concentration is defined and measured as mass/volume. Ppb which means one part of something divided by billion of parts of the same something, is the same as µg/L only if water has a temperature of 4oC. The same is true for ppm and mg/L. In any other temperature there is a difference between the two which is pertinent to the Everglades where water temperature may exceed 30oC . Hence they are only approximately equal. Provide also conversion in the text, for example, from acres to km2 and possibly, square miles and use these larger units when describing large watersheds (line 231 and throughout the chapter). In general, acres and hectares should be used for smaller watersheds, i.e., use proper units appropriate to the site. None one would use pounds (kg) to describe loads from the Mississippi River.

Comment #3: (Chapter 4) For all figures in the caption provide conversions for lbs/acre to kg/ha (not verbatim pounds/acre, everybody knows what lbs/acre is). Remove if possible ppb and replace it by µg/L.

For all figures in the caption provide conversions for lbs/acre to kg/ha (not verbatim pounds/acre, everybody knows what lbs/acre is). Remove if possible ppb and replace it by µg/L.

Comment #4: (Chapter 5B, 44–45) Please note that on lines 44 and 45 ppb was used with conversion to µ/L and not to µg/L; ppb is an archaic improper US unit that cannot be used in scientific report.

Comment #5: (Chapter 5B) Table 5B-1 presents the summary of performance of all STAs in WY 2015. While providing lots of useful information, the table uses a mix of SI (mt, g/m2/year, cm/year) and old US (acre-ft and ppb) units without conversion factors for US units. Readers unfamiliar with the SFWMD reporting and using SI system would read mt as milli – ton (one thousandth of a ton). The same problem with the inconsistent use of units is apparent in Figures 5B-2 and 5B-3 where the flow units are acre-ft (without a conversion) which is a unit of volume. Concentration unit ppb for TP must be replaced by µg/L as parts per billion is not a unit of concentration (mass/volume) but a fuzzy unit of parts of something divided by billion parts of the same something. These unit inconsistencies and improperness are ubiquitous throughout the chapter. There is no need in the captions to explain what cm/day (centimeters per day) and g/m2/yr (grams per square meter per year) are, every high school student should know that. The preferable format of the unit phosphorus load is g/(m2 - day) or better, g m-2 day-1. If giving an explanation is deemed necessary perhaps a glossary could be constructed for the report. If the district is compelled to use units such as acre-ft, without conversion to SI units, perhaps the rationale for this could be explained in the text.

This inconsistence and mishmash of units characterize almost every figure in the chapter. For example ppm in Figure 5B-8 should be replaced by mg/L.

Comment #6: (Chapter 5B) Concluding comments on the main deficiency of the chapter.

Overall, the work presented in this chapter is scientifically sound. It is hampered by the misuse of units and some organizational issues. In the last years the reviewers were repeatedly reminding the authors about inconsistent units used in their chapters. The SFER is read and used not only by the employees of the SFWMD but it is put on web, sent to wide audience and interest in this report is worldwide. Consequently, the primary unit system used in report writing ideally should be SI system with conversions to the old US system in parentheses. Authors of some chapters realized this fact and are using generally proper units and if some US units are used (e.g., archaic acre-ft) conversion factors to SI units (1 acre-ft = 1 233 m3) or equivalent values in US units should be provided in parentheses. However, authors of some chapters in this (and last) year SFER even mixed SI prefix with the archaic acre-ft to come up with "kacre ft" (meaning kilo-acre ft) which is nowhere defined on web or scientific conversion tables. The closest found on Google was "nacre" which means a shiny mineral excreted by shellfish. SI units are now used by 100% of scientific reports, books and journals, most government reports, in all US EPA reports and taught now by all major universities as the primary unit system.

We are now fifteen years in the new millennium but Chapter 5b brought the quality of writing back by twenty five years. The chapter is one of the worst mishmashes of units in the SFER whereby archaic US units (ppb, ppm, acre-ft. cfs) are used in the same paragraph of even sentence with SI or (pseudo) SI units (mt for metric ton, g/m2/year,. The authors must completely revise the units in reporting, replace all "ppm" by mg/L (as they done on some pages of the chapter) and "ppb" by µg/L. Ppb and ppm are not proper measures of concentration defined as mass/volume. They have not been used for years and are unacceptable. Concentration in all scientific report has been defined as mass/volume (mg/L or gram/m3) while ppm means parts (of something) per million of

parts (of something). Concentration of 5 mg/L of a pollutants expressed as 5 lbs of the pollutant per one million lbs of water would be the same as mg/L only if water is very pure and has temperature of 4oC (39.2 deg F). In any other temperature or salinity ppm would not be the same which is the case of the EPA. TP unit load in g/m2/year should be reported as g/(m2-year) or more properly g m-2 year-1.

As this is an on-going issue, perhaps the District could craft a "style manual" for these reports that authors could use in the years to come. This might alleviate some of the issues discussed above and lead to a more consistent product that needs less editing. These reports are a huge undertaking that require the contributions of many people, which we understand can be challenging. A style guide might help the authors and the consistency of the final report.

Comment #7: (Chapter 9, 427–429) As is the case in other chapters and other years' reports, a mix of English and metric units is employed throughout which can be confusing to the reader who continually has to do conversions. It is good to at least have an explanation for why the authors use non-SI units in the report. However, it would be even more useful if the conversions were done. Even the USACE must use metric units for parameters such as concentration (i.e. mg/L) so it should not be an impossible jump.

Response to All Above Comments: The South Florida Water Management District (District) has been criticized for using non-standard units of measure and for inconsistent use of units across chapters of the South Florida Environmental Report (SFER). Our agency has worked with authors for over a decade to improve the SFER, including better use of units. As a result, four chapters received no panel comments on units of measure, but we recognize that more work is needed. The 2016 SFER Expert Panel shared these concerns on units, even providing a separate sub-section titled 'Concluding comments on the main deficiency of the chapter' concerning the units in Chapter 5B.

The District takes all these criticisms very seriously. In fact, a meeting with senior management and staff directly involved with producing the SFER was convened on the issue. All panel comments were provided to this group and options for an agency action plan were reviewed. The result of this management meeting was guidance for a coordinated, in-depth analysis and a systematic agency-wide response. Our agency must work through many considerations on the use of units and does not endorse a rushed chapter-specific reaction to the comments on the 2016 draft report. We hope that the panel will carefully consider the process described below and support this deliberate approach leading to modifications to units and data presentations in future SFER reports.

The context of the SFER as an official communication vehicle for 75 agency reports is critical to re-enforce. Each chapter fulfills multiple mandates and does so to a broad audience encompassing technical experts and laypersons alike. Many, if not most users of the information, are not practicing scientists and may have expectations differing from scientific norms. As we begin a process of reviewing and modifying units, we must keep this diverse audience in mind; it includes scientists, engineers, legislators, consultants, farmers, ranchers, attorneys and various stakeholders. Our priority must be providing all these clients with quality assured data and information using the International System of Units (SI) where ever possible without losing portions of the audience. SFER Chapters 4, 8 and 10 are good examples of this blended approach to units, conveying technical information on multiple programs and projects with watershed management as an integrative theme and agricultural interests as a major client.

With our agency responsibility for broad communication in mind, the District will begin a process of reviewing the SFER beginning with mandates for reporting and their associated deliverables, including any specific units of measure. This process is not simple as agency mandates include Consent Orders & Decrees, state and federal legislation, permits and other regulatory requirements, as well as project and program reports. Once requirements are evaluated, authors will communicate with interested parties to better define expectations and recommendations for satisfying diverse clients. Armed with expanded information on communication needs, authors and their management teams will plan appropriate chapter revisions for future reports. District SFER editors and other dedicated staff will help to organize this process and will continue to develop standardized formats and templates as a style guide for figures, tables and other illustrations to improve consistency across chapters.

There are important exceptions to this deliberate revision process. For the near future, acre-feet (ac-ft) must continue to be used as a measure of large volumes. We understand fully the nature of this land-based unit and that it is not SI compliant. However, ac-ft are totally pervasive in District models, flow calculations, stream gaging, technical publications and operational schedules. Likewise, cubic feet per second (CFS) will continue to be used as needed following the same logic as for ac-ft. Here again, this measure of smaller flow volumes and flows is deeply embedded in water management and will be difficult to change.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 3A

Paul Julian II[1], Grover G. Payne[2] and Shi Kui Xue

Level of Panel Review: Accountability
Reviewers: ***Vladimir Novotny*** *(AA)*

Comment #1: "…However, in the previous 2015 SFER (Chapter 3A) this reviewer pointed out the inappropriate use of the odd unit for volume (kacre -ft = 1000 acre-ft = 1.233 x 106 m3)…"

Response #1: Just as in the past, the authors will note the reviewer's comments on units and will attempt to improve our presentation. The authors would like to direct the reviewer to the preface of the South Florida Environmental Report (SFER) where units of measure conversion factors from metric units to U.S. equivalent units are provided. Where possible the authors will include any conversions needed. For an example of the SFER preface, please see the 2016 SFER preface at the following link http://www.sfwmd.gov/portal/page/portal/pg_grp_sfwmd_sfer/portlet_prevreport/2016_sfer_draft/front_matter/front-matter.pdf

Please refer to separate agency response to panel comments regarding SFER reporting on units of measurement. Revisions will be made to the reference of kacre-ft to be consistent with other chapters in the report.

Comment #2: "…it is not clear whether any metal measurements were made in the WY 2015 and the no concern judgement was made based on several zero or no detects measurements during the WY 2015 or measurements were not made and the evaluation was simply postponed to some future SFER…"

Response #2: As stated in the chapter *"Parameters marked with an asterisk (*) were not measured in WY2015"*. These parameters include total selenium, thallium, zinc, cadmium, lead, nickel, silver, antimony, arsenic, beryllium and copper. These parameters have been analyzed and reported in previous SFERs. As discussed in the previous SFER, since WY2007 monitoring of metals entering the Everglades Protection Area (EPA) has been eliminated due to the prevalence of metals being observed below the established water quality standards as outlined in chapter 62.302.530, Florida Administrative Code (FAC) and the lack of new sources. Additional text will be added to the chapter to include this information.

[1] Florida Department of Environmental Protection, Office of Ecosystems Projects, Fort Myers, FL
[2] Florida Department of Environmental Protection, Division of Environmental Assessment and Restoration, Environmental Assessment and Standards Section, Tallahassee, FL

The only metal analyzed as part of the 2016 chapter 3A is total iron. During WY2015 no exceedances of iron were observed within the EPA. The last reported exceedance of the Total Iron, Class III water quality standard for the EPA occurred during WY2001 with exceedances reported at the inflow the Refuge and Interior portions of Water Conservation Area 3 (WCA-3) and Everglades National Park (ENP).

Comment #3:"... Comparison with Florida Class III water quality standards revealed numeric statistical excursions (C, and PC ranking) of four water quality parameters: dissolved oxygen, pH, alkalinity, and specific conductance (Table 3A-1). Whether these numeric excursions represent a quality problem or the problem is due to natural or anthropogenic causes will be subsequently discussed. It should be pointed out that one should differentiate between the water quality problem that is predominantly a consequence of natural/background conditions or the problem is anthropogenic. Section 502-19 of the Clean Water Act defines "pollution" as caused by humans or human activities and not by nature…"

Response #3: A narrative associated with each parameter reported in Table 3A-1 irrespective of the statistical excursion category has been provided in the chapter. Each parameter is discussed relative to background conditions (i.e. Alkalinity and pH) and/or the potential cause of excursions (Specific conductivity, Dissolved Oxygen, etc.).

Comment #4: Dissolved Oxygen (pages 3A-18-19) appears to be the most serious water quality problem. Marshes (wetlands) are generally naturally dystrophic, i.e., exhibiting low dissolved oxygen concentrations. Because of that SFWMD established (and US EPA approved) a site-specific alternative criterion (SSAC) which is a binomial model for all periods that for the time of the year and day estimates DO concentrations that would represent natural conditions. In the previous review this reviewer pointed out that on some days SSAC calculates DO concentrations that in most other water bodies may be harmful to fish and are below the federal DO standards. Starting with the last year (SFER 2015), Florida Class III DO standards, which are more stringent than SSAC and are based on the federal criteria, were applied to the inflow, outflow and rim area, while the SSAC criteria were applied to the interiors. In evaluating the DO criteria and based on the time of fish survival during less than optimal (lethal) concentrations which is limited to few sequential days (1 to 5 days) days not to 10% of the time. Nevertheless, if DO concentrations are violated by more than 10% of samples then, obviously, the situation is very serious (poor-concern).

Response #4: Just as in the past, the authors would like to direct the reviewer to the Everglades Dissolved Oxygen Site-Specific Alterative Criteria (DO SSAC) development document referenced as (Weaver, 2004) and can be found at the following link (http://www.dep.state.fl.us/everglades/files/DOTech_Support_DOC2004.pdf). It should be noted that the development of the Everglades Dissolved Oxygen Site-Specific Alterative Criteria (DO SSAC) was a joint effort between the Florida Department of Environment Protection (FDEP or Department) and the South Florida Water Management District (SFWMD or District). The District collected the large volume of data needed to complete a rigorous valid statistical evaluation of DO conditions within the Everglades ecosystem. As a result of this data collection efforts the Department used the data to develop the current Everglades marsh DO SSAC (Weaver, 2004).

Consistently this reviewer has reported that the DO SSAC is not protective of the ecology of the Everglades ecosystem without substantial data or information to support these claims. The DO SSAC was developed to accurately determine a point at which DO concentrations, based on temperature and time-of-day is protective of the waterbodies flora and fauna. Furthermore the DO SSAC development approach was deemed more appropriate to reduce both type I and II statistical

error. As a result of this approach "to realistically represent the natural background dissolve oxygen regime in the marsh the SSAC must account for these daily fluctuations", therefore a cyclic sinusoidal mathematical model with cross-validation was developed to accurately determine the protective background DO concentrations (Weaver, 2004).

As highlighted in the figure below (**Figure Response 3A-1**) DO follows a diel-curve which during the morning and evening hours DO concentrations are depressed due to the lack of photosynthesis and occurrence of respiration. **Figure Response 3A-1** also demonstrates the influence of nutrient enrichment on DO diel-cycles with the DO curve for highly impacted area (i.e. E1) are characterized by overall lower DO and dampened diel fluctuation relative to minimally impacted (i.e. E5) reference locations. Due to enrichment influence reference sites were used to develop the DO SSAC. Reference sites as discussed by FAC subsection 62-302.800 (2) requires that during the establishment of a SSAC, an affirmative demonstration be made that the "*proposed alternative criteria would exist due to natural background conditions or man-induced conditions which cannot be controlled or abated*". As a result of this criteria one of the initial steps in the development of a SSAC is to define the "*natural background conditions*" with respect to the parameter of concern. Based on the information presented in this response, previous year's peer-review response, and the DO SAAC development document the authors do not understand the concern and criticism by the reviewer on the "protectiveness" DO SSAC.

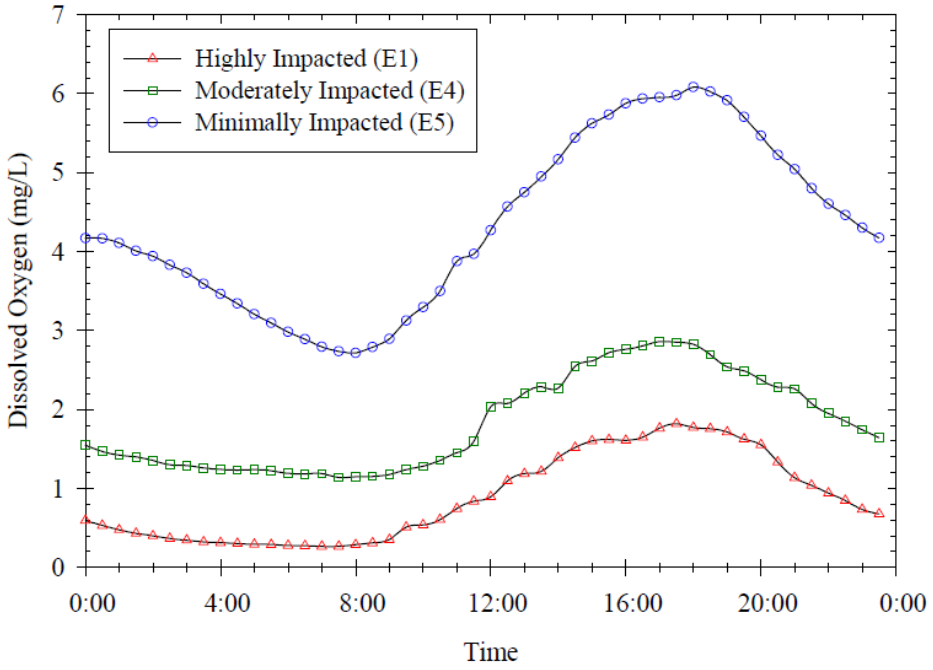

Figure Response 3A-1. Average diel dissolved oxygen curves at three levels of nutrient impact. Impact in this case is defined as the degree of phosphorus enrichment above background conditions (i.e. 10 μg/L). Adapted from Weaver, (2004).

Comment #5: An editorial comment: When describing the DO excursion the wording "exceeding the standard" (line 394) may not be appropriate because the excursions imply that DO concentration is below the criterion, not exceeding.

Response #5: Noted, The review needs to be aware that there is a distinction between criteria and standard. As defined by title 40 Code of Federal Regulations subset section 131.3(b) (The Clean Water Act[3]) water quality criteria (plural of criterion) are "*elements of State water quality standards, expressed as constituent concentrations, levels, or narrative statements, representing a quality of water that supports a particular use*." Furthermore water quality criterion require three components to be a true criterion, these components are magnitude, duration and frequency. Text on lines 393-394 text reads "*During WY2015, eleven interior stations (LOXA104.5, LOXA130, Z1, Z2, FS1, WCA2F1, WCA2F2, CA318, NE1, P33, and P36) exceeded the DO SSAC".* As the sentence currently reads, the use of "exceed" is consistent with the definition of criteria presented above.

Comment #6: "…The EPA systems receive a large portion of water inputs from precipitation. One additional cause of lower pH (increased H+ ion) may also be sulfate discharges (SO4=) from agricultural areas that are more endemic in the upper reaches and most likely were causes of the pH problems in the Refuge and WCA-3 areas. However, one of the consequences of the ongoing significant global, mostly anthropogenic, increases of the atmospheric CO2 content is the decrease of pH (increase of the H+ ion) in the precipitation and subsequent acidification of the oceans and all of water bodies that have a low natural buffering capacity (alkalinity) and receive large portion of water input from the atmosphere… It is also possible that the pH problems may also still be aggravated in the upper reaches (Refuge, WCA 2) by sulfate (SO4=) inputs from the agricultural areas."

Response #6: Noted, However the reviewer is in error by suggesting that sulfate inputs through agricultural discharges is a potential cause of the low pH levels observed. It is a common misconception that sulfate causes decreased pH levels. Actually, it is the oxidation of elemental sulfur or sulfide (reduced forms of sulfur) that results in pH reductions according to the redox reaction:

$$S_{(s)} + 4H_2O = SO_4^{2-} + 9H^+ + 6e^-$$

This is why elemental sulfur (not sulfate) is applied to agricultural soils in order to lower pH levels and make nutrients more available. Since sulfate is the oxidized from of sulfur, it has no potential to decrease pH levels. More investigation is needed to determine the cause of the low pH levels observed.

Comment #7: Editorial comment line (lines 453-454). Alkalinity is an equivalent sum of the contents of HCO3-,CO3=, OH- ions sometimes also including some metallic divalent ions (Strontium) if present in larger concentrations. It is expressed as an equivalent CaCO3 content of these cations.

Response #8: Noted, text on lines 444-449 and 453-456 defines alkalinity for the readers.

Comment #9: Pesticides. The current EPA monitoring program consists of 19 sites and is conducted on a biannual basis. In the WY 2015 monitoring 11 pesticides were detected but none above the limits. Even though the pesticide monitoring was initiated in 1976, the WY 2015 was the third consecutive year in which pesticide or pesticide breakdown products were detected at concentrations above their MDLs (Method Detection Limit – please define the acronym) but did not exceed state water quality criteria. However, it could be a warning.

[3] http://www.ecfr.gov/cgi-bin/text-idx?SID=05fb5e1fa0c3a348be0b137d4d9b3f6b&mc=true&node=pt40.22.131&rgn=div5#se40.22.131_13

Response #9: MDL is defined on line 239.

Comment #10: For the total phosphorus load (pages 3A35-42) the chapter presents calculations and mass flow graphs of the TP inputs to each individual basis and north to south flow of TP. The graphs are illustrative but are lacking description of individual sources. It appears that atmospheric deposition seems to be underestimated or not included. The only comprehensive and informative data on atmospheric deposition, both wet and dry, the reviewer was able to find was the article by Ahn and James, (2001) which contains data that are about 18 years old. In thus paper the total TP deposition was estimated for the ENP as 33 mg/m2-yr which is consistent with the information in Redfield, (2002) referenced in the 2016 SFER. If the total atmospheric deposition over the entire EPA is calculated the result is

$$2.5x10^{6}\ (acres) \times 4045\ (m^2/acre) \times 33\ (mg/m^2) \times 10^{-9}\ (t/mg) = 333\ tons\ (metric)$$

which is more than twice that estimated in the report and is 72% greater than the total inputs from the terrestrial sources (193 tons). There is also a puzzling inconsistency between the total TP loads presented in this section. On page 3A-35 the total P load from external sources to EPA would be 193 tons, in Table 3A5 the total load is 65 tons, and on line 904 the total load is 47tons. The authors should reconcile these inconsistencies.

Response #10: Noted, the graphs are intended for demonstrating numerical values of each individual sources and integrated system balance. The description of each sources is beyond scope of this chapter. The detail description of each sources can be found in various Chapters in SFER including Chapter 4: Nutrient Source Control Programs; Chapter 5B: Performance of the Everglades Stormwater Treatment Areas and Chapter 8: Lake Okeechobee Protection Program Annual Update etc. The graphs illustrate flow from surface water, atmospheric deposition is not included as indicated in the Figure description of Figures 3A-11 and 3A-12. The total atmospheric deposition is dependent on the area values and the atmospheric deposition rate used. The deposition rate is highly variable dependent on data sources. Based on the comment, the following are recalculations and clarifications.

80 of 17 µg/L. Another 193 mt of TP are estimated to have entered the EPA through
81 atmospheric deposition. The 65.2 mt TP load in the surface inflows to the EPA

868 In addition to inflow, atmospheric deposition contributes to the TP loading into the EPA. The
869 long-term average range of TP atmospheric deposition to the WCAs is estimated between 107 and
870 143 mt per year. Atmospheric TP deposition rates are highly variable but not routinely monitored
871 due to their high expense. The range [expressed spatially as 20 to 35 milligrams per square meter
872 per year ($mg/m^2/yr$)] is based on data obtained from long-term monitoring evaluated by the District
873 (Redfield, 2002).

903 As detailed in Appendix 3A-5, WY2015 annual TP loads from external surface sources to the
904 Refuge, WCA-2, WCA-3, and ENP were 47.1 mt, with a FWM TP concentration of 21 µg/L.
905 Another 193 mt of TP is estimated to have entered the EPA through atmospheric deposition
906 (Redfield, 2002). Discharges from the EPA account for 8.0 mt of TP for water supply and flood

Areas for EPA=WCAs+ENP=3497+5569=9066 square kilometers (Chapter 1,2005 SFER)

The three Water Conservation Areas (WCA-1, WCA-2, and WCA-3) are major components of the Everglades Protection Area and provide a valued suite of ecological and hydrological functions for the region. WCAs located south of Lake Okeechobee and west of the heavily urbanized Lower East Coast (LEC) comprise an area of about 3,497 square kilometers (1,350 square miles). These remaining Everglades wetlands serve as receiving waters for storm

Everglades National Park encompasses 5,569 square kilometers (2,150 square miles) of freshwater sloughs, sawgrass prairies, marl-forming wet prairies, mangrove forests, and saline tidal areas at the southern end of the Florida peninsula. The Park was formally established by Congress in 1934 to preserve the unique ecology of the Everglades. The Park was designated by the United Nations as a World Heritage Site in 1979. It has also been named a Federal Wilderness Area, an International Biosphere Reserve, and a Wetland of International Significance. Currently, Everglades National Park is the second largest national park in the United States and is one of the nation's 10 most endangered parks (SFWMD, 1992a).

The Park contains three dominant wetland habitat types: sloughs, marl-forming marshes, and mangroves. Sloughs comprise much of the central drainage of the Park. Shark River Slough consists of a broad, southwesterly arc of continuous wetlands, interspersed with sawgrass stands, open water sloughs, wet prairies, and tree islands extending from Tamiami Trail to the mangrove estuaries of Florida Bay. During wet periods, Taylor Slough (also called Taylor River) provides local flow of fresh water from the eastern side of the Park to Florida Bay. Southern marl-forming marshes are characterized by the formation of marl soils (also known as calcitic mud). Marl is

With following data compiled from *Redfield, (2002,*

Atmospheric Deposition Rates (mg/m^2/yr)

Average	STD	95% Upper limit	95% Lower limit	Sample size	Standard Error
33	28	42	24	39	4

$T_{0.025}=2.024$

The total atmospheric deposition over the WCAs is calculated as:
95% Lower limit:$3497x10^6$ (m^2) x 24 (mg/m^2) x 10^{-9} (t/mg) = 82 tons (metric)
95% Upperlimit:$3497x10^6$ (m^2) x 42 (mg/m^2) x 10^{-9} (t/mg) = 146 tons (metric)
The total atmospheric deposition over the EPA is calculated as:
Average: $9066x10^6$ (m^2) x 33 (mg/m^2) x 10^{-9} (t/mg) = 296 tons (metric)

Therefore 193 mt will be modified as 296 mt on lines 80 and 905,
107 mt will be modified as 82 mt on line 869,
143 mt will be modified as 146 mt on line 870,
20 to 35 mg/m^2/yr will be modified as 24 to 42 mg/m^2/yr on line 871,

For the surface water TP loads, 65.2 mt on line 81 is the total TP loads including internal transferring and external TP loads to the EPA.
47.1 mt on line 904 excluded all internal transferring to the EPA (i.e., external TP loads).

Comment #11: Total nitrogen (pages 3A45-52). The nitrogen statistics in WY 2015 are very similar to the previous WY 2014 so the wording herein is similar to that of the last year assessment. The

data on N statistics in Table 3A-9 and Figure 3A7 show significant improvement since the Baseline period. Further improvement can be expected by improving the performance of agricultural BMP, STAs, by improving mileage of automobiles and in the near future even by switching to hybrid and electric cars. Automobile traffic is a significant source of NOx emissions. However, Figure 3A-17 shows that most nitrogen in the EPA system is organic N. Unlike nitrates or ammonium organic nitrogen is not readily available for algal growth.

Response #11: Noted, However the authors would like to point out that Figure 3A-17 presents "Annual geometric mean TN concentrations (mg/L) for inflow and interior areas of the Refuge, WCA-2, WCA-3, ENP from WY1979–WY2015. Bars indicate geometric mean when flow; dashed line indicates geometric mean irrespective of flow. Horizontal lines indicate the mean annual geometric mean TP concentrations for the Baseline (WY1979–WY1993), Phase I (WY1994–WY2004), and Phase II (WY2005–WY2014) periods."

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Redfield, G.W. 2002. Atmospheric Deposition of Phosphorus to the Everglades: Concepts, Constraints, and Published Deposition Rates for Ecosystem Management. *The Scientific World Journal*, 2, 1843–1873.

Weaver, K. 2004. Everglades marsh dissolved oxygen site specific alternative criterion technical support document. Florida Department of Environmental Protection, Tallahassee, FL.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 3B

Edited by Paul Julian II[1], Binhe Gu, Garth Redfield and Ken Weaver[2]

Contributions by Forrest E. Dierberg[3], Mike Jerauld[3], Thomas A. DeBusk[3], Michelle D. Kharbanda[3], Janelle A. Potts[3] and Nichole R. Larson[3]

Level of Panel Review: Accountability
*Reviewers: **Vladimir Novotny** (AA) and **Michael A. Mallin** (A)*

Comment #1: The reason why sulfur and mercury were linked together in one chapter was because of the finding several years ago by the scientists retained by the SFWMD and extensively reported in the 2013 SFER that sulfate may promote mercury methylation in the sediment. These earlier findings led to the efforts to establish a unimodal sulfate standard (1 mg of SO_4^{2-}) for sulfate concentrations throughout the EPA. Later other factors were added and it was found that implementing the unimodal standard would be counterproductive and actually lead to worsening of the problem. These issues were extensively discussed in the past reports and reviews. The bacteria (SRB), the same bacteria that by reducing sulphate to sulfide release immobile iron and aluminum bound phosphorus into pore water as phosphate. This process may also be driven by eutrophication increasing dissolved organic matter, which was found in the 2013 report as one of the factors affecting formation of MeHg. Chapter 3B in the 2016 SFER has introduced other possible factors that will also be discussed herein. It should be prefaced that methyl mercury formation in aquatic systems has been researched for decades, yet, satisfactory models that could be used to develop a scientifically well-founded standard are still not available.

Response #1: Noted, text will be added to provide a sound research statement related to mercury (Hg) and the inability of developing a satisfactory model that could be used to develop a well-founded standard in light of the provide empirical hurdles and inconsistencies.

The authors would also like to point out that the 2013 South Florida Environmental Report (Axelrad et al., 2013) did not establish a sulfate standard nor did it "prove" a unimodal relationship with respect to sulfate and methyl-Hg (MeHg) concentration (previously referred to as the Goldilock theory). Furthermore it is worth noting that increasing evidence suggests that other bacteria beside sulfate reducing bacteria (SRB) are capable of methylation Hg as discussed in the 2015 SFER (Julian et al., 2015b) and others in the scientific literature (Bae et al., 2014; Gilmour et al., 2013; King et al., 2000; Schaefer et al., 2014). The hypothesis that multiple microbial guilds have the ability to methylate Hg could help explain the extreme variability in sulfate concentrations relative to MeHg concentrations.

Comment #2: Page 3B-13 – line 305 – the trend of the THg in the mosquito fish concentration increase from north to south is mentioned and documented. It would be beneficial to the readers of

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[2]Florida Department of Environmental Protection, Division of Ecosystem Assessment and Restoration, Tallahassee, FL
[3]DB Environmental, Inc, Rockledge, FL

this report unfamiliar with the previous SFERs to explain the consistency of this trend over the years.

Response #2: Noted, text will be added to clarify this statement.

Comment #3: The marked variability differences of Figures 3B -4 and 3B-5 between the wet and dry years and the fact that most mercury input (95%) is in wet deposition (Figure 3B-4) warrants more attention. The variability is larger in marshes than in canals (Figure 3B-5). This high degree of variation in marsh habitats was attributed by the authors to relatively dynamic hydrology (i.e. dry-down, dry-out, water level changes, etc.), the dynamics of marsh trophic structure and biogeochemistry associated with dynamic hydrology.

The statistical boxes and limits of the variability on these figures should be identified in the caption (% variance).

Response #3: The authors agree that more analysis is needed to compare and understand fish tissue Hg concentrations between wet versus dry years. Text will be added to figure captions.

Comment #4: Lines 352 to 354 lists several key factors that could influence THg conditions, including water quality conditions (pH, alkalinity, nutrient availability, etc.), trophic position, and habitat structure (Julian and Gu, 2015). This sentence sort of presents a broad array of factors without stating how or why they might impact concentrations. They could be listed in the reference but it would help to the reader of the chapter to have a more specific explanation; hence, some additional material here to clarify it for the reader should be included.

Response #4: Noted, text will be added to clarify these statements.

Comment #5: Figure 3B-9 presents the box plot statistics of the THg contamination of the largemouth bass fish tissue at all stations for the POR from WY1999 toWY2015. It is important to note that the WY 2015 means for 5 stations are much greater than those for the POR, the largest increase being recorded for Station L67F1 in the middle of the ENP which exhibits the smallest SO42- water column concentrations. Throughout the POR, no statistically significant temporal trends in largemouth bass THg tissue concentration were evident (Table 3B-3). This increase is contradicted by generally decreasing atmospheric inputs of Hg over the last10 years presented at the end of the chapter on Figure 3B-23. Probably the longer life span of the largemouth bass causes the delay in the response. Please, provide your explanation.

Response #5: Noted, text will be added to clarify to discuss the decrease in atmospheric Hg and increase in largemouth bass THg tissue concentrations.

Comment #6: Page 3B-17 – line 361 –states that in some years half of the stations approximately exceeded the recommend criterion" Not really – it appears to be 60% at best or more, so please correct and be precise.

Response #6: Noted, text will be revised.

Comment #7: This section/article is focusing on the variability of the Hg contamination of one specific fish species- mosquito fish. The contribution by Dierberg et al, is well written and high level scientific analysis that contributes to the enhancement of knowledge on this difficult topic. It is worth to note that the preceding section has documented spatial variability of the contamination of this specific species which is not as distinct as that for gold fish and largemouth bass so that the statement that "persistent spatial variability in Eastern mosquitofish tissue Hg concentrations across the Everglades has been recognized since at least 1996". The authors of the two sections should reconcile these contradicting findings.

The second possible statement in the first paragraph "there is unexplained variability in the fish Hg/surface water SO_4^{2-} relationship which must be explained if an effective MeHg mitigation policy is to be devised" is not exactly correct. Several literature sources, e.g., Gabriel et al., and the 2014 SFER introduced a possible and credible explanation of the Hg fish tissue contamination. It can be explained by a relatively simple concept of Hg contamination increasing at low SO_4^{2-} concentrations, reaching maximum at concentrations somewhere between 1 to 5 mg/L of SO_4^{2-} and then decreasing and leveling off thereafter, perhaps due to increasing sulfide toxicity effects. This lead to a classic bell shape relation known, for example, from toxicity of metals and other pollutants on biota which at low concentrations stimulate growth and are toxic in high concentrations. Obviously, even in the 2014 SFER other variables plus randomness play a role; hence, the relationship is highly statistical. However, as pointed out in the preceding section the relationship for mosquito fish is as not as pronounced as for the other two species.

Response #7: The reviewer is requiring a false comparison between "goldfish" and "largemouth bass". The difference in the magnitude of variability among different species is not relevant to this claim. Figure 3B-2 in the preceding section does show persistent (i.e. differences in long-term medians) spatial differences in mosquitofish Hg therefore, based on this evidence reconciliation between these two sections are not needed.

Here and elsewhere, the reviewers appear to distinguish the terms "bell-shaped" from "unimodal" in describing the shape of the relationship between (log) surface water sulfate and fish Hg, when the authors used the terms synonymously and interchangeably. The reviewers' issue here seems to arise only from this terminology confusion, since the reviewers echo the authors' position later in their review. The reviewers stated "*bell-shaped relationship may be present…in addition to randomness, an effect of the other constituents present in water…and soil* ." meanwhile in the chapter the authors' indicate "*unexplained variability…which must be explained*" which is restated by the reviewers as "*efforts should be focused on identifying the other variables and separation of the random component*".

The reviewers and authors appear to be in agreement with the need for further exploration into factors other than sulfate affecting the MeHg concentrations in water and biota, even though the reviewers cite Gabriel et al., (2014) as to why fish THg concentrations in the Everglades can be explained primarily by the sulfate concentrations. There are some misconceptions and mis-interpretations surrounding that manuscript that need to be pointed out before a heavy reliance is placed on its findings:

1. While emphasizing the central role that sulfate appears to have on the distribution of THg in three Everglades fish species, the Gabriel et al., (2014) article does tacitly acknowledge the importance of other factors (DOC, bioavailable Hg, redox conditions, other dissolved

ion, pH, temperature, fish feeding patterns, growth rates, and migration) in the very large variations in fish THg.

2. The Gabriel et al., (2014) article is not without controversy. Questions have been raised related to both the methodology and interpretation of the data. See Julian et al., (2015a) and rejoinder by Gabriel et al., (2015).
3. The reviewers attribute the decreasing (receding) limb of the bell-shaped curve to sulfide toxicity effects. We are unaware of that reason being invoked in any publication that provided an explanation as to why the THg concentration in Everglades fish decrease with increasing sulfate concentration. Instead, those publications universally cited the reduction of bioavailable Hg from sulfide complexation and precipitation, and not sulfide toxicity, as being the reason for the receding limb of the bell-shaped curve.

Comment #8: In the long paragraph on page 3B-24 the authors elaborate the difficulties using the unimodal fish THg contamination vs. SO_4^{2-} water column concentration. For example, on page 3B-25, lines 494-500, they reported a puzzle at a sampling site DB-15 that since 2011 had SO42- concentrations below detection limit (0.2 mg/L), yet, the mosquito fish had moderate THg tissue contamination. This indicates that other factors, perhaps iron may play a role. But it has been established several years ago that the relationship is not unimodal but even Figure 3B-12 has an indication that the bell shaped relationship may be present hidden under what at first look appears to be a lot of noise but in addition to randomness an effect of the other constituents present in water (e.g, DOC, dissolved Fe^{2+}) and/or sediment composition may also have an effect. Hence, the efforts should be focused on identifying the other variables and separation of the random component (white noise) that would correlate in a multi regression nonlinear model. This could be characterized as "nonlinear principle component analysis and neural net modeling" and not just linear unimodal straight line regression (see Bedoya et al.,). As shown on Figure 3B-13, the correlation between the water column sulphate and pore water sulfide is fuzzy which introduces a new uncertainty and need to identify other variables that affect the sulfide formation in the sediment (perhaps organic content, temperature).

Response #8: The reviewers don't level any specific criticism, but their general concern should be abated if "unimodal" and "bell-shaped" are read as interchangeable synonyms in the original text (see comment response above). Further, we agree with the reviewer that multi-parameter (likely non-linear) modeling is likely to be required to capture the interacting influences on Hg methylation and uptake. However, a rigorous previous attempt (Pollman, 2012) explained only about 50% of the variability in mosquitofish tissue THg concentrations after significant parameter transformation and data filtering techniques were applied. Based on a review of this modeling effort additional independent variables not available in that modeling effort are needed. We are seeking to identify those variables with this and our ongoing research program.

Comment #9: Figure 2B – 14 is confusing and the selected data from only a few stations cannot be extrapolated to the entire picture. For one, it lacks points from sites where the SO_4^{2-} concentrations are between 1 to 5 mg/L which on other similar plots contained the highest THg fish tissue contamination values. On the other hand, when the same data were replotted on Figure 3B-15 very interesting (exciting) relatively new information was revealed, i.e., (1) in the absence of sulfur at Site DB-15 dissolved iron was present and most likely driving the MeHg absorption while sulfur did the same at Sites F2 and U3, (2) in oligotrophic parts of EPA both iron and sulfur can trigger MeHg formation and cause THg mosquitofish contamination and reducing bacteria are capable to form MeHg, (3) aquatic vegetation can play an important role.

Response #9: The data presented in Figure 3B-14 is the actual data collected during year-2 of the referenced study. There is not selection or screening of data applied. The authors agree with the reviewer's conclusions of Figure 3B-15, this information starts to paint a much clearer picture of some of the driver of MeHg dynamics by highlighting the role of iron and potential some degree trophic structure (i.e. aquatic vegetation structure and type). These sorts of comparisons not only relegate sulfate to more of a minor role in accounting for MeHg concentrations than previously thought, but it also can lead to new hypotheses as to factors other than sulfate that may be responsible for the accumulation of MeHg in water and biota.

Comment #10: On pages 3B-28 to 29 the authors expand their finding to suggesting alternative method a of management. On line 335 to 338 they hypothesized that "the physical or ecological structure imposed by different vegetation types (P-enriched monotypic cattail (Typha) versus P-enriched open water with submerged macrophyte Chara versus typical oligotrophic open-water Everglades slough with submerged and sparse emergent macrophytes) was an important factor mediating the uptake of Hg by mosquitofish (and other biota)". This is a reasonable hypothesis but is should be clarified exactly how and why; i.e., what are the reasons the uptake by mosquitofish would be affected – either increased or decreased. Very broad statement as it stands.

Response #10: The authors agree with the reviewers that the "*the physical or ecological structure imposed by different vegetation types (P-enriched monotypic cattail (Typha) versus P-enriched open water with submerged macrophyte Chara versus typical oligotrophic open-water Everglades slough with submerged and sparse emergent macrophytes) was an important factor mediating the uptake of Hg by mosquitofish (and other biota*)" is a very broad statement as it stands. The reviewers acknowledge that it is a reasonable hypothesis but that it should be clarified exactly how and why; i.e., what are the reasons the uptake by mosquitofish would be affected – either increased or decreased. As a result text will be added to the chapter to clarify this statement.

Comment #11: It is hoped that by now all of us realize that a unimodal straight line relationship (i.e., absorption of THg in aquatic organisms increases correlates linearly to the increase of the SO_4^{2-} concentration in the water column) does not exist.

The reviewers suggest it is possible to go beyond this finding and identify other drivers already mentioned in this section (iron) and previous reports (dissolved organic carbon). Hence, because it has now been clearly established that the MeHg formation and absorption into fish tissue may indeed follow the bell shape relationship and this relationship is not unimodal and contains a random component and there is plenty of data now available, time has finally come for developing a statistical multimodal nonlinear model. Ten years ago the reviewer's team working on the EPA STAR project was facing a problem how to find a relationship of the multi-parameter (multimodal) Index of Biotic Integrity to environmental variables (physical –habitat, land use and chemical). "Data mining experts" were a part of the team and the effort was successful and in this particular outcome led to quantitative recognition of the importance of physical parameters (Novotny et al., 2009) . Because the number of driving parameters may be more than one there is a need in the analysis to eliminate cross-correlations. For example, both dissolved iron and sulfide concentrations may be correlated to the dissolved organic carbon.

Response #11: Noted, the authors 100% agree with the reviewers. While the realization that unimodal relationship between sulfate and Hg within the Everglades ecosystem does not exist has been denied in some circles the authors and their respective agencies are continuing research effort to address this topic in an effort to provide more clarity to Hg dynamics within the Everglades ecosystem. This research effort is aimed at understanding other factors that influence MeHg

production within the ecosystem and MeHg uptake and accumulation in consumer indicator species.

Comment #12: ***Mercury sources to EPA*** *are presented on pages 3B-30 – 34. In the lead paragraph, please, list the most important sources of Hg to the atmosphere (i.e. coal-fired power plants, cement plants, etc.). It is important to note here that the State of Florida is one of the largest consumers of coal in the U.S. for power production purposes. Even though the U.S. inputs in general have decreased, Florida situation should be specified.*

Response #12: The authors respectively disagrees with the reviewer comment that "*Florida is one of the largest consumers of coal in the U.S.*" Based on data presented by the U.S. Energy Information Administration (http://www.eia.gov/) Florida is ranked 46th (out of 50) for total energy from petroleum, natural gas and coal consumption per capita (208 million British thermal units per capita; 208x10^{6} kilo-joule per capita) as of 2013 (http://www.eia.gov/state/data.cfm?sid=FL). Additionally during 2013 Florida consumed only 2.3% of the entire amount of coal consumed by the US (21,464x10^{4} short tons; 19,471x10^{4} metric tons) (http://www.nma.org/pdf/c_use_state.pdf). Furthermore as stated in the State of Florida statewide Mercury TMDL (Florida Department of Environmental Protection, 2013), *"The overwhelming majority of the mercury that is deposited from the atmosphere onto Florida's land and waters comes from anthropogenic international sources, outside of North America."*

Comment #13: Atmospheric source represents between 95 to 98 % of mercury inputs to the system. Due to the data release schedules, this assessment only spans WY1996–WY2014. Only 5 monitoring sites were active, only one in ENP. During WY2014, atmospheric loading in the EPA was highest within ENP followed by WCA-3, WCA-1, and WCA-2. However, the differences were very small and it appears that if Student statistic similarity test was performed then, statistically, there would be no difference between the basins. For these reasons no statistical conclusions can be made on the relationship between the magnitude of the Hg deposition and fish tissue mercury contamination. However, if a numeric (pseudo) deterministic model is developed by a consultant, at some university or federal government research center, the magnitude of the mercury input will have to be considered; hence, the current research provides valuable information.

Response #13: The authors would like to clarify the reviewers comment that *"Only 5 monitoring sites were active…",* on line-609 the draft chapter states "*…only three active MDN stations located within the EPA…",* the text continues on to discuss the period of record for each of the stations. The authors will consider the addition of a Student statistic similarity test or a similar statistic depending on the data distribution and conformity to the statistical assumptions of the selected test statistic.

Comment #14: Page 3B-32 – Table 3B-4 – please add period of record in years to the table caption.

Response #14: Noted, text will be added to include the period of record.

Comment #15: The statement on lines 671-672 in page 3B-34 "qualitatively there is a small potential correlation between fish tissue Hg and deposition….limited data and small regional differences limit investment in more detailed data collection and analysis" may be a little misleading. It is recommended that on line 671 the term "correlation" is changed to "relationship" at this point, until the proper statistics can be run. A statement should be included that in addition to the development of a nonlinear multivariate (multimodal) model by advanced "data mining",development of a quantitative deterministic model of the Hg cycle from the deposition to the fish and aquatic biota contamination should be also encouraged as a follow up. This model will

be useful for predicting quantitatively the effects of the expected reductions of emissions from power plants and changing from coal or dirty oil to green energy and natural gas. For this Hg cycle model and quantitative Hg mass balance between the subsystem the knowledge of external inputs and their past and predicted trends is needed.

Response #15: Noted, text on line 671 will be change. The authors will also consider adding some text related to the development of a quantitative model.

Comment #16: Bottom paragraph – on page 3B-36, line 774 states that data were collected on flow events only. Does that mean rain events? It may by unclear to the readers what "flow events" are.

Response #16: Noted, this text will be clarified. Flow events are defined as when flow was recorded at the specific inflow or outflow structure.

Comment #17: Table 3B-6 contains the statistical summaries of the data. At first look there are questionable very high concentration maxima at some sections, especially in ENP, which unrealistically distorts the probability distribution. Typically, monitoring data follow the log – normal probability distribution which is exhibited on a log of data vs. probability of exceedance plot as a straight line. Such plot should be included. At this time, the maximum measured values in ENP and some other sections look like unexplainable outliers because of a great difference between the means and medians. The differences between the arithmetic mean (4.4 mg/L) and geometric mean (0.7 mg/L) seem to be excessively large. Other data mentioned in this chapter indicate that the measured sulfate concentrations in twelve out of fourteen monitored points within ENP had sulfate concentrations less than 1 mg/L. The presence of very high sulfate concentrations at the northern boundary of ENP should be explained. Was it a dryout?

Response #17: As a result of the dynamic hydrology and the dry-down/dry-out and rewetting water column constituent concentration can spike to extremely high values. This phenomenon is highlighted in the sulfate concentrations presented in 3B-6 but are also apparent in other water column measurement as presented in Chapter 3A of this volume (Total Phosphorus: Table 3A-3, Orthophsophate: Table 3A-6 and Total Nitrogen: Table 3A-9 in the draft version). The authors will consider the addition of normal probability distributions, however based on previous experience these figure are not received well from most readers. Although these figures could be potential included in an appendix to this chapter.

The observed difference between arithmetic mean and geometric is presumably due to the distribution of the data and sample size. Similar statistics are reported in Chapter 3A of this volume for Total Phosphorus (TP) however due to the very large sample size (i.e. weekly or biweekly) samples this difference between mean is very low. Sulfate is typically collected quarterly from a limited number of stations relative to the TP monitoring throughout the EPA.

Comment #18: On page 3B-42 line 863-864 the authors state "the very low SO42- concentrations observed for the interior portion of the Refuge indicate that either assimilation of sulfate is occurring and potentially could be limiting". Limiting to what? Plant growth in general? Any particular species?

Response #18: Noted, text will added to clarify this statement.

Comment #19 Page 3B-43 – first sentence – change …"anions have become widely recognized" to "anions have been widely recognized".

Response #19: Noted, revision will be made. Thank you.

Comment #20: Question: Page 3B-44 – lines 975-976 – why is 10 years the sufficient lag time? Is there a reason for this specific period, or a reference one can provide?

Response #20: The text will be reworded. There is a certain degree of lag from implementing controls and an environmental response, the fact that it took approximately 10 years for trends to change is both academically and regulatory interesting.

Comment #21: Line 981 - "Error! Reference source not found" – explain or remove statement.

Response #21: Noted, text will be revised and the error will be removed.

Comment #22: Figure 3B-23 is excellent and gives encouragement to hope that the near and far field deposition of Hg may be reduced so that the system may begin to recover. Is the decrease in the overall atmospheric deposition of Hg in the last ten years unique to Florida, EPA or is it nationwide?

Response #22: Noted, the authors share the reviewer sentiment that "Figure 3B-23 is excellent and gives encouragement to hope that the near and far field deposition…", however clarification is needed Figure 3B-23 depicts the wet and dry deposition of sulfate. For purposes of this report, the analysis was isolated to south Florida using a three stations triangulation to provide a regional estimate of sulfate deposition. It is suspected (but not confirmed) that this decrease in sulfate deposition has also occurred at the state level but the authors wish not to speculate on the nationwide sulfate deposition trends.

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RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 4

Jeffrey Iudicello, Carlos Adorisio, Carmela Bedregal,
Pamela Wade, Jodie Hansing, and Ximena Pernett

Level of Panel Review: Accountability
Reviewers: Vladimir Novotny

Comment #1 (pg. 2): *TN loads were presented only for the St. Lucie and Caloosahatchee Rivers because their estuaries may be both phosphorus and nitrogen limited.*

Response #1: For clarification, TN loads are presented for the Caloosahatchee and St. Lucie Rivers because TN has been identified by statutes as a water quality parameter of concern in the rivers watersheds.

Comment #2 (pg. 2): *Of note is the 79% reduction of the TP loads from the Everglades Agricultural Areas, which is the largest source. This reduction is greater than the goal of 25% TP reduction. However, it is evident that in the other watersheds, C-139 Basin, Non – ECP Basins, and Lake Okeechobee, St. Lucie River, Caloosahatchee River, implementing the source and regional NP pollution programs are still in progress and results may be noticed after the year 2020. Table 4-1 shows some basins in the Lake Okeechobee and St. Lucie watersheds have very high unit and total loads of TP (e.g., Indian Prairie) and also contribute high loads to these water bodies. Please, include a note whether or not these loads impact the EPA basins (most likely they are not).*

Response #2: Basins within the Lake Okeechobee watershed discharge to Lake Okeechobee, which is an upstream tributary to the EPA basins and the river watersheds. Therefore, these basins can directly or indirectly impact TP loads to the EPA basins. In the St. Lucie River Watershed, only loads from the C-44 Basin could impact loads to the EPA Basins. Such an occurrence would be occasional and indirect, as water from the C-44 Canal would first have to be pumped back into Lake Okeechobee. All other basins in the St. Lucie River Watershed discharge to tide.

Comment #3 (pg. 2): *The problems with noncompliance with the TP reduction goals are most visible for the Non-ECP basins (lines 181-187). These basins, although smaller in size, discharge directly into the EPA basins and may have adverse local impact on the quality within the EPA. For example, the Long-Term Plan describes a phosphorus concentration requirement of 50 μg/L in discharges from the North Feeder Canal Sub-basin, yet, the WY2015 TP flow-weighted mean concentration (FWMC) in discharges from the Canal Sub-basin was 228 μg/L. This situation should be addressed more forcefully. Also the discharges from unabated discharges from the Native American Territories (Reservations) should be addressed in more detail.*

Response #3: The District is working with landowners within the North Feeder Canal Sub-basin to meet the phosphorus concentration requirement of 50 μg/L in their discharges (see Lines 1012

through 1016 on Page 4-36). This sub-basin is part of the Western Basins Water Resources Evaluation Project, which seeks to identify management measures that would affect reductions in TP concentrations (see Lines 978 through 994 on Pages 4-36 and 4-37).

The quality of the discharges from the Big Cypress Seminole Reservation does not fall under the District's jurisdiction; however, the Seminole Tribe of Florida has partnered with the Natural Resources Conservation Service (NRCS) to implement the Seminole Water Conservation Plan Project in the L-28 Basin, and both entities are to share the cost (see Lines 1063 through 1074 on Page 4-37).

Comment #4 (pg. 3): *Ppb which means one part of something divided by billion of parts of the same something, is the same as µg/L only if water has a temperature of 4oC. The same is true for ppm and mg/L. In any other temperature there is a difference between the two which is pertinent to the Everglades where water temperature may exceed 30oC . Hence they are only* ***approximately*** *equal. Provide also conversion in the text, for example, from acres to km2 and possibly, square miles and use these larger units when describing large watersheds (line 231 and throughout the chapter).*

Response #4: Please refer to separate agency response to panel comments regarding SFER reporting on units of measurement.

Comment #5 (pg. 3): *This goal for C-139 is confusing. Normally it would be expected that source controls would result in a reduction of the load not keeping status quo. Please, explain. Furthermore, it appears that C-139 just barely keeps the loads below the historic loads. It also seems that some permittees are reluctant to cooperate (lines 355-359). Is this caused by a lack of effective enforcement? Is there a way to obtain full cooperation?*

Response #5: Some reference to the history of the program is necessary to explain the basis for the C-139 Basin goal and performance levels.

In 2002 Chapter 40E-63, F.A.C., was amended to create the C-139 Basin source control program. The impetus for incorporating the C-139 Basin under a regulatory BMP program was a result of the TP in runoff increasing above historic levels (as defined by the EFA) and other factors, such as the impending risk of land use intensification without adequate phosphorus controls in place (see Figure 4-8 depicting increasing load trends starting in WY1998). Additionally, the C-139 Basin was already the second largest contributor of TP in runoff to the Everglades and construction of Stormwater Treatment Area 5 to treat those discharges was underway. The design of the STA relied on the assurance that the regulatory program would consistently control the phosphorus levels within an expected range over the long term. The EFA mandated that "landowners within the C-139 Basin shall not collectively exceed an annual average loading of phosphorus based proportionately on the historical rainfall for the C-139 Basin over the period of October 1, 1978 to September 30, 1988."

Despite the implementation of basic regulatory activities and supplementary projects, TP levels continued to increase in three of the four water years after 2002. A likely reason for the continued increases is that system responses to new practices are not typically observed immediately, but rather take time for positive changes to be observed, sometimes up to two years. As a result, as regulatory requirements became more stringent, TP levels started decreasing in WY2009 and have

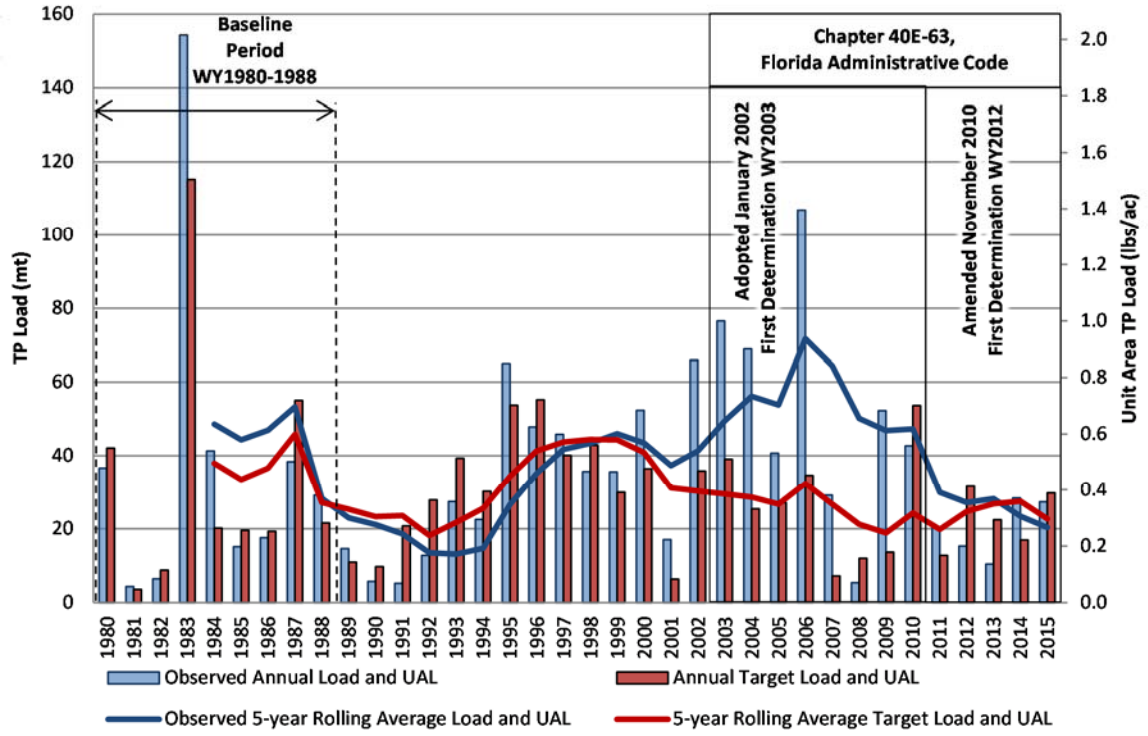

Figure 4-8. WY1980-WY2015 C-139 Basin runoff and target annual TP load and UAL and five-year rolling averages.

Regarding enforcement and lines 355-359 from the SFER ("*However, because permittees in the C-139 Basin have declined to collect water quality and quantity data to characterize their permit-level discharges, a water quality and quantity monitoring network for upstream sub-regional areas throughout the basin would be used by the District to differentiate the relative contribution of the hydrologic sub-basins to support the secondary compliance methodology, if necessary*"), the rule provides permittees an option to conduct individual monitoring at the permit basin level; however, in many cases monitoring is not cost effective or easily accomplished because of the area-specific drainage configurations and technical difficulties. District monitoring encompasses larger hydrologic areas and serves to prioritize efforts and is technically reliable. Thus, permittees opting not to monitor their discharges is not associated with noncompliance.

Comment #6 (pg: 3): The goal of 25% reduction of the TP load have been met consistently since 1992*. This downward trend of TP 4 load reduction is also graphically presented on Figure 4-5. This is interesting. On one side, meeting the goal so early when hardly any BMPs were implemented is great but it leads to an impression that the goal might have been set too low. Please, comment.*

Response #6: There was much deliberation by the scientific community and the stakeholders on establishing an appropriate reduction goal. A wide variety of factors, mainly scientific and economic, were considered when determining the final reduction goal. The 25% reduction goal was selected based on BMPs that could be implemented cost effectively and the need for a target that could be consistently achieved over the long term within the context of a regulatory program

and the initial design of regional downstream treatment projects. As such, the 25% TP load reduction requirement was established in the Everglades Forever Act (1994).

With regard to the early successes, there were water quality improvement activities occurring within the basin prior to adoption of Chapter 40E-63, F.A.C., including research. As reported in the WY99 BMP report (SFWMD, 2000) 18 research, implementation and education BMP programs were initiated as early as 1985 with 12 of those programs dedicated to BMP research in partnerships between the private sector and public agencies (Figure 30 of the WY99 BMP report is presented below for reference).

After 20 years of successful program implementation and meeting the established goals (i.e. Fig. 4-5), we are afforded the hindsight of questioning whether the reduction requirement could have been more stringent. However, at the time the EFA was passed, a reduction goal of 25% was deemed an appropriate number after considering all factors.

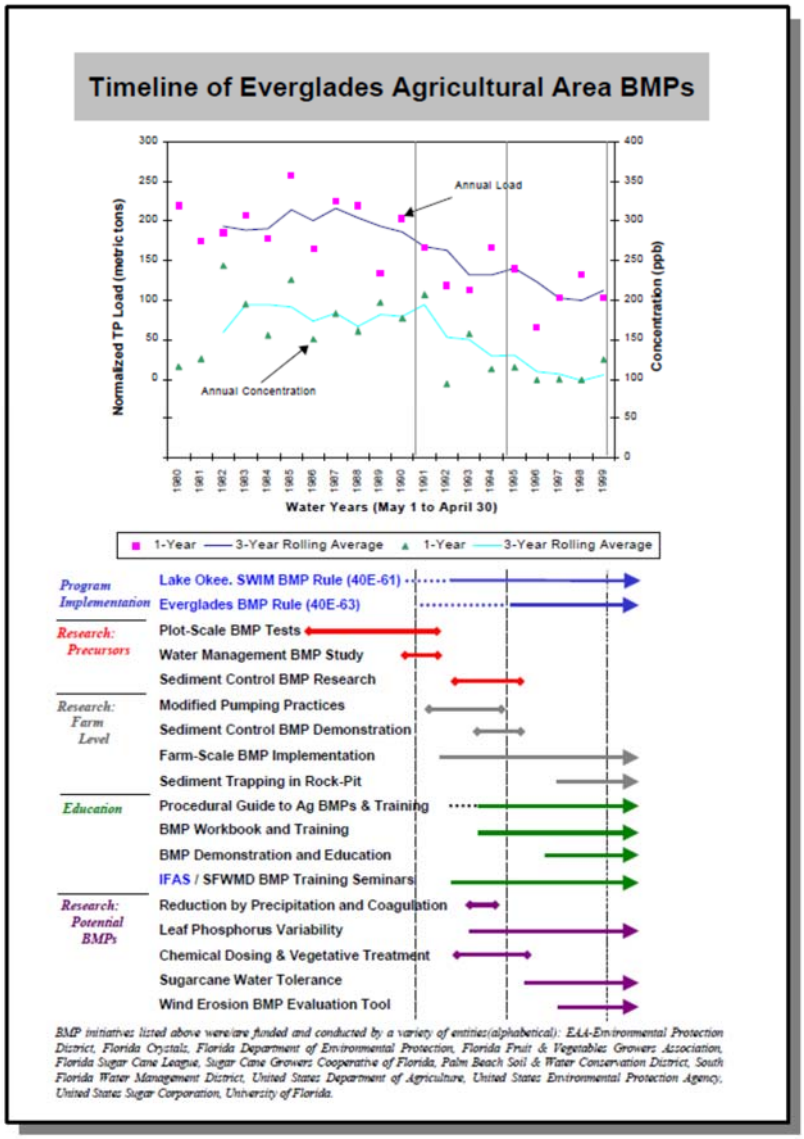

Figure 30 – Timeline Comparison of Rule 40E-63 EAA Phosphorus Levels with BMP Activities

Comment #7 (pg. 4): *Figure 4-6 shows corresponding reduction of unit loads. In the caption provide a conversion from lbs/acre to kg/ha.*

Response #7: Please refer to separate agency response to panel comments regarding SFER reporting on units of measurement.

Comment #8 (pg. 4): *As far as financing the program, the Everglades Forever Act imposes a tax of $10 to $25/acre (there is not a need to include a conversion, the tax is a legal instrument and not a technical or geographical variable) to finance the program (lines 363-373). What is the level of compliance of landowners and how is the tax collected?*

Response #8: This Agricultural Privilege Tax is a non ad valorem assessment collected by the county tax collector through the annual property tax process and sent to the District. Regarding compliance with the payment of the tax, the District target rate is to collect a minimum of 95% of the estimated income based on the statutory tax rate applied to the qualified agricultural acreage, as established by the property appraiser. In the past fiscal year the District collected 101% of the target amount. The actual collected amount depends on when taxes are paid, as fees are collected for late payment and discounts provided for early payment. In the event of nonpayment by April 1, the issuance and sale of tax certificates and tax deeds are coordinated by the tax collector annually in June, as established by statutes.

Comment #9 (pg. 4): *This section, while being descriptive of existing and future programs, is full of acronyms which leave the reviewer and readers to jumping all over the chapter to decipher what they are. This may be also true for some other sections. The authors should edit their writing and if a large number of acronyms and references to various section and paragraphs are needed they should be redefined at the beginning of the section where they first appear. Going back ten or so pages to find a reference for an acronym is difficult.*

Response #9: It is true that the entire chapter includes many acronyms which describe legislation, programs, areas, etc. However, our introduction and use of acronyms in the chapter is consistent with the District's writing convention for acronyms in the SFER. Please note that the Front Matter for the SFER (found at www.sfwmd.gov/sfer) includes a link to "Acronyms & Abbreviations", which will guide the reader to a complete list of acronyms used throughout the SFER.

Comment #10 (pg. 5): *Table 4-7 on page 4-26 is supposed to summarize the actions implemented in the Basin. It appears that about 50% of planned actions have not been implemented and the rest is in the planning stages. From the table and the text, it is not clear what the "points" are for and what they represent; hence this table is very confusing. Also permittees in the C-139 Basin are not required to collect water quality and quantity data to characterize permit-level discharges so it is up to District to step in and establish the water quality and quantity monitoring network and collect the necessary data for the Basin.*

Response #10: All planned actions have been implemented. To clarify, Table 4-7 provides a chronology of BMP requirements, water quality-based performance measure determinations and rule-mandated compliance actions. BMP requirements and compliance actions were implemented in accordance with the schedules. However, the water quality-based performance measures have

been met in only six of the 13 years that the program was in place. These occurrences were prior to the rule being amended in WY2011 to include more stringent comprehensive BMP plans. The table and footnotes will be clarified.

BMP equivalent points are a regulatory tool used by permit reviewers to ensure a level playing field between growers. Each Everglades Works of the District (EWOD) permit approves a BMP plan. The BMP plan includes operational programs or infrastructure enhancements designed to reduce phosphorus levels in discharges. The District is responsible for ensuring that a base level of BMPs is established for each permit area and that BMP plans between different permittees are consistent and comparable. To accomplish this, a system of BMP "equivalents" was developed by assigning points to BMPs within four basic categories consisting of water management practices, nutrient management practices, control of sediment and particulate matter, and pasture management (if applicable). Points were originally based on the review of reports and publications produced by University of Florida Institute for Food and Agricultural Services (UF/IFAS), on the best professional judgment of District staff, and extensive cooperative workshops conducted among affected landowners, consultants, and other interested stakeholders. At the time that the literature was reviewed, information suggested that the certain practices could reduce P in discharges, thus providing the basis for their inclusion. Relatively speaking, the level of points within each of the categories, gives an indication of expected performance. This approach was developed considering that both flow and concentration are targeted through a comprehensive plan. With these objectives in mind, the number of points assigned to each BMP was developed as a negotiated solution in a regulatory context. The BMP points system has proven successful in ensuring implementation of a consistent level of BMPs among permittees with different site conditions.

The concept of points will be clarified in the SFER.

It is correct that the permittees implementing BMPs under the C-139 Basin regulatory source control program are not required to monitor water quality in discharges. Also see Response #5.

Comment #11 (pg. 5): *This ambiguity of the C-139 program is reflected in the results. Table 4-8 shows results for WY 2015 which indicate the annual load from the basin was about 10 % better than the target predicted load. This "good" (actually "marginal") result for WY 2015 is contradicted in Table 4-9 and Figure 4-8 in which 6 out of the last years (including WY 2014) had TP loads greater than the target value. Luckily, the TP load removal performance of STA 5/6 (Chapter 5B) is very good, about 85%, but this does not give an excuse for low outcome of the source control in the C-139 Basin. Please comment.*

Response #11: The annual C-139 Basin compliance determination for phosphorus loads is based on achieving a Target Load, which is the predicted pre-BMP baseline period load adjusted for hydrologic variability. Moreover, as explained in lines 784-798 of the chapter, noncompliance for the C-139 Basin is a two-step evaluation: 1) if the Target Loads are exceeded for three consecutive years, or 2) if a single year's load exceeds the Limit Load, which is calculated as the 90^{th} percentile confidence level of the Target Load. This two-part test is designed to meet regulatory goals for identifying increased TP loads while statistically accounting for error in the annual load prediction.

As discussed in Response #5, implementation of the C-139 Basin regulatory source control program has reversed the increasing trends that were observed during the WY2002 – WY2009. Continued implementation will confirm whether the amended regulatory compliance strategy resulting from the 2010 rule amendment will be sufficient to consistently reduce the levels to those

observed during WY1978 – WY1988. Note that the rule includes requirements for additional water quality improvement activities with the option to focus on priority areas, if noncompliance occurs.

Comment #12 (pg. 5): *Figure 4-10 shows that the Feeder canal and L-28 basin bring most of the TP load from the Non-ECP basins directly into EPA. C-111 Basin discharge goes directly into the ENP. The programs in the Non-ECP struggle to meet the 50µg/L outflow concentration goals, as a matter of fact the current concentrations are far above this goal (page 4-35) in the Feeder Canal The District is working with the landowners and two Native American Tribal territories. Apparently, work on tribal lands is stalled (page 4-37). As stated in the chapter, the 2003 Long-Term Plan recommended modification of the plan to convert WRA-7 (little abatement-reviewer's comment) to an STA by 2010 as of August 2015, had not been authorized. Please, provide the reasons for the delay or an update if there is a change. Also, replace word "Indian" by "Native American" and "Reservation" by 'Territory" or "Land" in this Chapter and, hopefully, in the entire SFER).*

Response #12: For discussion on the Feeder Canal Basin and L-28 Basin non-ECP topics, please see Response #3 above. C-111 Basin annual TP FWMC in discharges to ENP have been 8 µg/L or less the last four water years.

The District has consulted with tribe representatives and their requested names are "Seminole Tribe of Florida" (http://www.semtribe.com/) and "Miccosukee Tribe of Indians of Florida" (http://www.miccosukee.com/tribe/). The requested names for their reservations are "Big Cypress Seminole Reservation" and "Miccosukee Federal Indian Reservation (Alligator Alley Reservation)." The final 2016 SFER will ensure consistency with the naming requested by tribe representatives.

Comment #13 (pg. 6): Northern Everglades Programs. *Overview of the Northern Everglades Source Control Programs is presented on* ***pages 4-39 to4-45****. Northern Everglades include watersheds of Lake Okeechobee, Caloosahatchee River, and St. Lucie River. These water bodies and watersheds were in the past heavily modified. The NEEPP includes a phased, comprehensive, and innovative protection program composed of integrated approaches such as source control programs, construction projects, and research and water quality monitoring programs. Hydrological modifications have caused diversions of significant portions of the flow from Lake Okeechobee into the two rivers and, hence, to the Gulf of Mexico and Atlantic Ocean. The pollutants of concern in the Northern Everglades are phosphorus in the Lake Okeechobee watersheds and both phosphorus and nitrogen in the river watersheds and in the flow into the Water conservation Areas of EPA. Lake Okeechobee was suffering years ago from cyanobacteria growth, today this important lake still may be eutrophic. One reason may be (please comment) that the NP Source controls in the Lake Okeechobee watershed have not yet been fully implemented. Table 4-10 lists the nutrient control plans and actions in the Northern Everglades. Besides the listing the table could also include the status of the programs (planned, ongoing, complete).*

Response #13: For clarification, TN is a pollutant of concern in the Caloosahatchee and St. Lucie River watersheds as mandated by the NEEPP, but not in the Water Conservation Areas.

It is correct that the NP source controls in the Lake Okeechobee watershed, such as Chapter 40E-61, F.A.C., have not yet been fully implemented, and for several reasons. Moreover, while the

BMAP has been approved by the FDEP, it is widely acknowledged that it will likely take several years before the BMAP realizes complete implementation and improved water quality.

To address Table 4-10, the following sentence was added to the text around Line 1178: "All programs listed are ongoing except for the dairy remediation projects and Best Available Technologies Project, which were both completed in 2008".

Comment #14 (pg. 6): *On lines 1191 and 1192 the District reported the maximum cost of the action to remove TP as $5/lbs. How was this limiting cost determined, in the plan or by monitoring the performance? Does it mean that if the cost is greater than the limit there would be no removal or cost can be shifted between less expensive and more costly programs?*

Response #14: The cost reported is not a limiting nor a maximum cost of the action to remove one pound of TP. It is the actual average cost to the District for 20 years of implementation of its Regulatory Source Control Program in the EAA and C-139 basins. This number was calculated by adding the total annual program costs (i.e., staff resources in the Everglades Regulation Bureau, water quality monitoring, technical investigations, and demonstration projects) and dividing it over the total TP load reduction observed during the 20 years of program implementation.

Comment #15 (pg. 6): *Figures 4-12 to 4-14 show the source control implementation areas. The scale of the maps does not allow to read, identify and locate the projects.*

Response #15: Figures 4-12 to 4-14 identify the source control program implementation area and the monitoring network established to track progress/trends from these areas. There are no projects identified in these figures, nor discussions in the chapter that reference depicted projects.

Comment #16 (pg. 7): *Understandingly, the authors of this chapter focused on the NP source controls but the effects of physical modifications may play also an important role in increasing or attenuating the nutrient loads to Lake Okeechobee, the Caloosahatchee River, and St. Lucie River, and potentially to the Everglades. Ideally, one page information dealing with physical restoration of the rivers and reclamation of floodplains that can attenuate nutrient loads would be nice to complete the entire picture on nutrient abatement in the Northern Everglades.*

Response #16: Descriptions of the "physical restoration of the rivers and reclamation of floodplains that can attenuate nutrient loads" is out of the scope of Chapter 4. Instead, the reader is referred to SFER Chapter 9, "Kissimmee River Restoration and Basin Initiatives", for a comprehensive discussion of the Kissimmee River. Additionally, it can be seen in Figure 26 of Appendix 4-3 that nutrient loads coming from the floodplains of the Kissimmee River are not as high of a water quality concern as other areas within the Lake Okeechobee Watershed.

Comment #17 (pg. 7): *For all figures in the caption provide conversions for lbs/acre to kg/ha (not verbatim pounds/acre, everybody knows what lbs/acre is). Remove if possible ppb and replace it by μg/L.*

Response #17: Please refer to separate agency response to panel comments regarding SFER reporting on units of measurement.

Comment #18 (pg. 8): *The authors did not comment nor attempted to analyze the variability of the histograms on Figures 4-15 to 4-38. One could assume that because no source controls have been implemented and actually working in the watersheds the plots could be more or less steady reflecting variation of meteorological and hydrological factors. Nevertheless in some watersheds land uses changes have occurred over the last twenty years and also one or two (or more) hurricanes could have an impact. Land use changes can generate temporary heavy pollution or even permanent (e.g., change from natural land to agriculture) heavy NP loads.*

Response #18: For clarification, the data plots included for the Northern Everglades are bar charts showing values of annual TP load (and TN for the Rivers).

The authors' intent was to present the data and available information on activities that may impact water quality in relationship to a timeline; not to hypothesize as to the cause and effect. Certain source controls, projects, and other activities have been implemented in some basins, and when comparing the timeline of activities to water quality trends, there may appear to be a water quality response in some basins. In those cases, more detailed investigation is warranted. For example, the S-191 Basin has shown improvements in water quality (see Figure 4-25 on page 4-62). Additionally, it is agreed that land use plays a major role in the water quality exiting a basin, which is why the historical comparison of land use is provided for the Northern Everglades basins.

Comment #19 (pg. 9): *Summary Point #2: Implementation of NP source controls in the Northern Everglades watersheds is in the permitting phases of implementation and at this time do not have an impact on the TP (or TN) loads.*

Response #19: BMPs in the Northern Everglades are in various phases of implementation. In the future, the FDEP will assess the impact of the BMPs and permitting programs through the BMAP.

Comment #20 (pg. 9): *Summary Point #3: The largest land uses are in the Northern Everglades watershed are natural land and pasture that will not be affected by implementation of NP pollution controls.*

Response #20: Implementation of BMPs on pasture lands in the Northern Everglades has been shown to successfully improve water quality in basins with large pasture areas. For example, the S-191 basin land use is over 50% pasture land, both historically and currently, and the basin has realized a decreasing trend in TP runoff load (Figure 4-25).

Comment #21 (pg. 9): *Summary Point #6: A reader of the chapter is overwhelmed by acronyms and section and paragraph numbers of the regulation to the point of being lost and spending a lot of time looking for the definitions. Some reediting may be necessary.*

Response #21: See Response #9 above.

Comment #22 (pg. 9): *Summary Point #7: There is still a need to reedit the definition and use of units as advised in the review.*

Response #22: Please refer to separate agency response to panel comments regarding SFER reporting on units of measurement.

REFERENCES

SFWMD, 2000. Everglades Best Management Practices Program- 5th Annual Report, Water Year 1999.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 5A

Jennifer Leeds

Level of Panel Review: Accountability
Reviewers: **Siobhan Fennessy**

Comment #1:

Overall this is a clear and useful overview and synthesis of the status of the WY2015 projects designed to improve water quality and meet the water quality standards necessary for the restoration projects in the SFQMD. Table 5A-1 is particularly helpful in outlining how projects relate to management goals. It would be beneficial to expand this table to include all the Consent Orders to demonstrate where the district is in the over scope of the projects (if possible). The expansion of STA-1 and establishing the FEBs should provide valuable returns in meeting water storage and water quality goals. A few specific comments are offered below to help improve clarity of the projects described in this section of the report.

Response #1:

Table 5A-1 – During previous SFER chapter preparations and reviews by FDEP, FDEP has requested the District to only report on those milestones that were completed for the SFER reporting year.

Comment #2:

Page 5A-2 (line 28) mentions that the Consent Orders recognize that the WQBEL won't be met until all the Consent Order activities are complete – when is that deadline?

Response #2:

All Consent Order activities will be complete by 12/31/2025

Comment #3:

Figure 5A – this map clearly shows the bounds of the L-8 FEB, but it isn't clear what the L-8 basin (shown in yellow) is, or how the two areas are related. Is this a catchment area? An explanation in the legend indicating what the different points are about would also be helpful, for example, points that start with "S" mean something, and points that start with "G" mean something else (I assume). There are a lot of these types of named structures in the reports overall and they can be difficult to track for those of us not working on the projects day to day,

Response #3:

Agree, the map legend will be updated to reflect the structure designations.

Comment #4:

Page 5A-11 – the bottom photo, labeled 5A-4, is out of order in the report; should be 5A-9, and on the following page 5A-10 (instead of 5A-5).

Response #4:

Thank you, the labels on the photos have been fixed.

Comment #5:

A-1 FEB - For the FEBs that are shallow and meant to hold stormwater, did the hydrological planning account for water losses due to evapotranspiration? This may be detailed elsewhere, and not appropriate for this chapter, but it seems that evaporative losses could be quite high.

Response #5:

Yes, the hydrologic planning including the use of the DMSTA model. One of DMSTA's input parameters is daily evapotranspiration (ET). So for each RS facility, water losses due to ET were included and thus are part of the overall water budget.

For the A-1 FEB, simulated average annual rainfall was 50.7 inches and average annual ET was 52.5 inches. This equates to a rainfall volume of 57,000 acre-feet/year and an ET volume of 59,000 acre-feet/year of ET. For perspective, A-1 FEB inflows were approximately 365,000 ac-ft per year.

Comment #6:

Line 234, Additional Components – it is not clear what is meant by "conceptual projects" and where they might be found on Figure 5A-1 (as indicated). Conceptual seems very different than 'planned.' Perhaps the terminology should be changed, and the map modified accordingly?

Response #6:

The sentence will be changed to read, "Potential sub-regional source control projects in the S-5A basin, location depicted on Figure 5A-1, were considered…"

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 5B

Edited by Kathleen Pietro

Contributing Authors: Tom DeBusk[1], Brian Garrett, Larry Gerry, Delia Ivanoff, Michelle Kharbanda[1], Jill King, Tracey Piccone, Larry Schwartz, Lou Toth, Shi Kui Xue, Yaoyang Yan, Manuel Zamorano and Hongying Zhao

Level of Panel Review: Technical or Accountability
Reviewers: Siobhan Fennessy (AA), Vladimir Novotny (A)

Comment #1: Panel Comment: Table 5B-1 presents the summary of performance of all STAs in WY 2015. While providing lots of useful information, the table uses a mix of SI (mt, g/m2/year, cm/year) and old US (acre-ft and ppb) units without conversion factors for US units. Readers unfamiliar with the SFWMD reporting and using SI system would read mt as milli – ton (one thousandth of a ton). The same problem with the inconsistent use of units is apparent in Figures 5B-2 and 5B-3 where the flow units are acre-ft (without a conversion) which is a unit of volume. Concentration unit ppb for TP must be replaced by µg/L as parts per billion is not a unit of concentration (mass/volume) but a fuzzy unit of parts of something divided by billion parts of the same something. These unit inconsistencies and improperness are ubiquitous throughout the chapter. There is no need in the captions to explain what cm/day (centimeters per day) and g/m2/yr (grams per square meter per year) are, every high school student should know that. The preferable format of the unit phosphorus load is g/(m2 - day) or better, g m-2 day-1. If giving an explanation is deemed necessary perhaps a glossary could be constructed for the report. If the district is compelled to use units such as acre-ft, without conversion to SI units, perhaps the rationale for this could be explained in the text.

This inconsistence and mishmash of units characterize almost every figure in the chapter. For example ppm in Figure 5B-8 should be replaced by mg/L.

Figure 5B-3 – The hydraulic loading reported of approximately 2 cm/day is similar to natural or free surface wetlands used for stormwater treatment1. TP loading is very low when compared to design parameters for free surface wetlands (WPCF 1990 1). The WPCF manual recommends TP loading in kg/ha-day that, after conversion to g/m2-year would be order of magnitude greater that those listed in presented on the Figure and throughout the chapter. However, it should be pointed out that typical constructed storm and waste treatment wetlands do not provide degree of treatment that would be as good as P removal in STAs.

Overall, the work presented in this chapter is scientifically sound. It is hampered by the misuse of units and some organizational issues. In the last years the reviewers were repeatedly reminding the authors about inconsistent units used in their chapters. The SFER is read and used not only by

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the employees of the SFWMD but it is put on web, sent to wide audience and interest in this report is worldwide. Consequently, the primary unit system used in report writing ideally should be SI system with conversions to the old US system in parentheses. Authors of some chapters realized this fact and are using generally proper units and if some US units are used (e.g., archaic acre-ft) conversion factors to SI units (1 acre-ft = 1 233 m3) or equivalent values in US units should be provided in parentheses. However, authors of some chapters in this (and last) year SFER even mixed SI prefix with the archaic acre-ft to come up with "kacre ft" (meaning kilo-acre ft) which is nowhere defined on web or scientific conversion tables. The closest found on Google was "nacre" which means a shiny mineral excreted by shellfish. SI units are now used by 100% of scientific reports, books and journals, most government reports, in all US EPA reports and taught now by all major universities as the primary unit system.

We are now fifteen years in the new millennium but Chapter 5b brought the quality of writing back by twenty five years. The chapter is one of the worst mishmashes of units in the SFER whereby archaic US units (ppb, ppm, acre-ft. cfs) are used in the same paragraph of even sentence with SI or (pseudo) SI units (mt for metric ton, g/m2/year,. The authors must completely revise the units in reporting, replace all "ppm" by mg/L (as they done on some pages of the chapter) and "ppb" by µg/L. Ppb and ppm are not proper measures of concentration defined as mass/volume. They have not been used for years and are unacceptable. Concentration in all scientific report has been defined as mass/volume (mg/L or gram/m3) while ppm means parts (of something) per million of parts (of something). Concentration of 5 mg/L of a pollutants expressed as 5 lbs of the pollutant per one million lbs of water would be the same as mg/L only if water is very pure and has temperature of 4oC (39.2 deg F). In any other temperature or salinity ppm would not be the same which is the case of the EPA. TP unit load in g/m2/year should be reported as g/(m2-year) or more properly g m-2 year-1.

As this is an on-going issue, perhaps the District could craft a "style manual" for these reports that authors could use in the years to come. This might alleviate some of the issues discussed above and lead to a more consistent product that needs less editing. These reports are a huge undertaking that require the contributions of many people, which we understand can be challenging. A style guide might help the authors and the consistency of the final report.

Response #1: Author Response: Please refer to separate agency response to panel comments regarding SFER reporting on units of measurement.

Comment #2: Panel Comment: Line 186 –It is not clear what is mean by vegetation condition; does this refer to community composition and cover data? An assessment of condition connotes things that aren't presented here. This term is also used in subsequent headings, such as at line 378. Please change to reflect what was actually measured. In contrast, the section starting at line 1399 does present information on vegetation condition (in this case, the result of stressors that affect the 'health' of a given plant).

Response #2: Author Response: Text will be revised to indicate that the vegetation condition assessment pertains to coverage and qualitative observations.

Comment #3: Panel Comment: Line 225 - This paragraph has text that is identical to text in the paragraph at line 217.

Response #:3 Author Response: The repeated text will be removed.

Comment #4: Panel Comment: Figure 5B–5: This figure is lacking the legend for the colors used (this is found in other figures, for example at line 500).

Response #4: Author Response: The legend will be added to the figure.

Comment #5: Panel Comment: In Figure 5B-9b it is not clear why net inflows are described as negative; this figure is meant to show inflow and outflow volumes as separate lines.

Response #5: Author Response: Although unintentional and infrequent, negative (reverse) flows can occur at gated culverts. In lieu of plotting a negative inflow volume for January 2015, a note was added below the figure and the flow was graphed as "zero ac-ft". The note will be revised to clarify this issue.

Comment #6: Panel Comment: Line 584 – It is reported here that turbidity at S5-A was 20 ppm. It would be helpful to explain what S5-A is and where it can be found? On Figure 5B-1 it looks that it might be an inlet from an unidentified canal.

Response #6: Author Response: S-5A Pump Station provides flood protection to an upstream agricultural basin and serves as the primary inflow structure for the overall STA-1 inflow basin that coveys water to STA-1W and STA-1E. S-5A Pump Station can also be used to deliver releases from Lake Okeechobee to the STAs. We will add further description and a link to one of the maps.

Comment #7: Panel Comment: Line 584 –Also turbidity is not measured in ppm (mg/L) which must be a typo. The unit of turbidity is NTU, which has no mass value.

Response #7: Author Response: Yes, the units shown were incorrect and will be changed to NTU.

Comment #8: Panel Comment: Figure 5A-20 is another representation of SAV coverage of a cell which shows shortcutting channel along the side though which shortcutting may occur. If these areas support EAV or other vegetation types please clarify.

Response #8: Author Response: Good comment. Yes, all our SAV cells also have EAV in them, mostly in the form of vegetation strips or sporadic throughout the cell to help protect the SAV from strong winds or strong flows. We will add clarifying statements in the revised write-up.

Comment #9: Panel Comment: Figure 5B-24 – The text states that starting in WY2009, the southern end of Cell 2 was converted from cattail to SAV to improve treatment performance, however Figure 24 doesn't indicate improvement in the STA's performance overall. Is there an explanation for this?

Response #9: Author Response: Outflow TP concentrations from Cell 2 have generally improved over time, however the impact of this performance may not be apparent in the overall STA performance because multiple factors affect overall STA performance, including the fact that in WY2012 new flow-ways were undergoing startup and the vegetation grow-in phase. In more recent years, the overall STA-2 performance trend has improved as the new flow-ways have stabilized and are providing additional effective treatment area.

Comment #10: Panel Comment: Figure 5B-25 is missing legend on vertical axes A, B, and D. Are they the same as those on C?

Response #10: Author Response: The legend is not missing for the vertical axes and all 4 plots share the same axis labels. We will center the axis labels between plots B & C to avoid confusion.

Comment #11: Panel Comment: Figure 5B-26 - PLR (phosphorus loading rate?) should be defined in the caption. It might have been defined pages before in the text but a reader might have a hard time to find it. On subsequent figures replace ppm by mg/L.

Response #11: Author Response: PLR will be described in the figure caption. Please see Response to Comment 1 above regarding the units used for PLR in subsequent figures.

Comment #12: Panel Comment: Line 787 – It was surprising to see such a lengthy discussion of calcium here since it was not mentioned above. Why is it a focus for STA 2 and not the other STAs? Some rationale for its inclusion here would be useful. Most readers at this point in the report are expecting a parallel structure to be used for the findings on each STA and their flow ways. As it is presented this seems like an outlier. It is also introduced very abruptly. The first sentence in the section states that calcium decreased by approximately 38 percent at the STA-2 inflow; 38% of what? Obviously the chemistry of calcium and phosphate are intertwined (as influenced by pH), but this section needs to be introduced properly and set in context.

Response #12: Author Response: We will add clarification that the rationale for discussing calcium extensively for STA-2 is because we are evaluating the effects of Lake Okeechobee releases in the STAs and STA-2 was chosen for the following reasons: 1) TP and SRP concentrations increased in Flow-way 1 during the period of Lake releases, 2) STA-2 received ~50% of total Lake releases into the STAs in WY2015, 3) STA-2 provides a parallel comparison among the different vegetation configurations (Flow-way 1- emergent vegetation, Flow-way 2 – mixed emergent + submerged, Flow-way 3 – submerged, and Flow-way 4 – sequential emergent then submerged), and 4) there is an ongoing Restoration Strategies Science Plan study in STA-2 which also aims at determining the influence of calcium on P removal and cycling.

Comment #13: Panel Comment: Line 817 –This paragraph doesn't synthesize the Ca and SRP data, which we were expecting. The flow of logic would also benefit by re-ordering the paragraphs in this section to more clearly explain the goals and results. As an example, the paragraph starting at line 827, which gives the objectives of the study, could be presented first.

Response #13: Author Response: We will re-organize the paragraphs according to Panel's suggestion. The period in which the potential effects of lower calcium were observed was very short and there was not enough data to make any solid conclusions. We are continuing to gather additional data to investigate this further.

Comment #14: Panel Comment: Line 836, - Why was Ca measured only in 2 months? Is this enough data to make sound conclusions?

Response #14: Author Response: The discussion starting on Line 836 pertains to internal transect sampling which was conducted infrequently (1-2 times per year). The introduction to this subsection will be revised to clarify the objectives (and frequency) of these internal sampling events. Coupled with high temporal resolution inflow/outflow data, these less frequent, high-spatial resolution internal data offer useful insights. However, we are also continuing to gather additional data to investigate this further. As part of a Restoration Strategies Science Plan study, calcium sampling and analysis is conducted weekly during periods of controlled flow events at six interior sampling stations in Flow-way 1 representing the inflow to outflow gradient, and at the inflow and outflow structures on a bi-weekly or monthly basis. A separate controlled microcosm experiment specific to calcium and its effects on phosphorus concentrations in the water column is also underway.

Comment #15: Panel Comment: Line 845 - Results and discussion – this goes back to a discussion of P only with maps of the spatial distribution of P. What happened to the Ca profiles (line 827)? Why not plot P and Ca? The discussion of the links between Ca and P are not clear. This is an interesting topic and, if it is retained, should be presented in a clear and logical manner. As it stands, this section is very disorganized and extremely difficult to follow. It almost reads as if parts of a draft manuscript was copied and pasted into the report.

Response #15: Author Response: This section is not a subsection under the preceding calcium discussion, so the interaction of P and Ca is not the only analysis of interest for these internal data. The analysis and discussion of P and Ca will be enhanced and better integrated with the results and discussion of the preceding section. It should be noted that this investigation is still in the preliminary phase, therefore, the frequency of sampling for these parameters within these transects is very sparse and limits our interpretation of the data.

Comment #16: Panel Comment: Line 927-8 - What insights have been provided? This is vague.

Response #16: Author Response: We will revise the concluding paragraph to clarify the findings of the internal water quality monitoring. Specifically, our analysis showed the changes in spatial distribution of the different species of P over the three sampling periods, representing before and after the receipt of a large volume of Lake releases. It also showed that the calcium spatial profile was different before and after the receipt of a large volume of Lake releases.

Comment #17: Panel Comment: Line 1336 and 1337 and Figure 5B-44. As a note, the TP unit load of 0.7 g/m2-year is an order of magnitude smaller than that typical for stormwater treating wetlands1.

Response #17: Author Response: We will include a statement that the inflow phosphorus loading rate is lower than what is typically received by domestic wastewater treatment systems.

Comment #18: Panel Comment: Figure 5B-44 - It would be helpful to identify what the abbreviations WFW, CEW, and EFW stand for (as in Figure 5B-46).

Response #18: Author Response: Clarifying text will be added to the figure caption.

Comment #19: Panel Comment: Line 1362 – Here the same general points as above apply for the Ca- P discussion. In addition, Ca is presented in units of mg/L (as it should be) while above the units were ppm.

Response #19: Author Response: See response to Comment #12. We will change the units to be consistent with the rest of the document.

Comment #20: Panel Comment: Figures 5B-45 and 5B-46. PWM and FWM (respectively) should be identified in the captions.

Response #20: Author Response: The figures do not show PWM. Rather, all figures show FWM. The abbreviation FWM will be removed from the axis label and replaced with the text "flow-weighted mean".

Comment #21: Panel Comment: Line 1400 – Why is this a question of interest here and not in the other STAs? What were the findings?

Response #21: Author Response: Vegetation health is important for all the STA cells. Cattail condition is being more closely monitored in STA-3/4 Cell 3A because it received continuous Lake releases in WY2015 and continued to receive Lake releases in WY2016. Since we cannot closely monitor all areas due to limited resources, we chose a flow-way that received a high volume of Lake releases. Cell 2A of STA-3/4 is being monitored for comparison with Cell 3A because this cell received a relatively small volume of Lake releases in WY2015.

Comment #22: Panel Comment: Page 5B-68 - The authors report that herbicides were used to eliminate cattail vegetation. Why? Cattail herbicide treatment was also mentioned for other STAs. This contradicts with the finding in Chapter 3B dealing with excessive mercury contamination of fish tissue in the EPA which observed that cattail vegetation cover has had significantly more beneficial effect on reducing THg fish tissue contamination than SAV or absence of vegetation.

Response #22: Author Response: Targeted herbicide application is used to aid in maintaining the preferred vegetation communities for optimal P removal treatment. For example, in SAV cells, cattail is not entirely eliminated, but is maintained in the form of emergent vegetation strips to help protect the SAV during high winds and/or flow events.

The effect of vegetation on fish mercury levels is complex and linked to changes in food web structure beginning with the influence of canopy density and periphyton abundance. Mercury levels in STAs are typically low possibly due to high sulfate levels which under some conditions may limit mercury methylation and due to high plant growth supporting food webs with less opportunity for bioaccumulation. The dense vegetation canopies in the STAs can act as refuge for small fish encouraging predatory fish to feed at lower trophic levels and from sources based more on detritus. With all these factors in balance, the lower mercury in fish from STAs is not driven solely by cattail abundance, but has been observed across diverse plant communities in the STAs. The selective removal of cattail is not anticipated to create elevated mercury in fish inhabiting the STAs and research continues in the WCAs to understand more about the role of plant canopies and food web structure.

Comment #23: Panel Comment: STA 5/6. The overall efficiency for TP retention (67%) was less than that for the other STAs but still relatively good. Can the cause of differences in performance be discussed?

Response #23: Author Response: Text will be added explaining that STA-5/6 inflows are irregular due to the nature of the tributary basin's water management practices (i.e., the farmers try to hold onto their water as long as possible but during the wet season, when their canals and impoundments are full, they have to start releasing water), is subjected to dryout and subsequent temporary increases in TP upon rehydration, and has been expanded by the addition of Cell 6-2 then Compartment C buildout. We will also add that Cell 5-1A underwent rehabilitation to alleviate short-circuiting, higher ground elevation areas on the west end were leveled, and numerous vegetation enhancements have been implemented in the SAV cells. As a result, outflow TP concentrations have notably improved starting in WY2009.

Comment #24: Panel Comment: Line 1677 – There are no maps of vegetation for this last section as there were for the other STAs. The maps were quite well designed, conveyed a lot of information and few well into the discussion.

Response #24: Author Response: There were no vegetation surveys conducted in STA-5/6 in WY2015.

Comment #25: Panel Comment: Overall the chapter ends quite abruptly. The authors might address the overall conclusions, issues, and possible lessons learned from all of the above data on the STAs that can feed into adaptive management decisions.

Response #25: Author Response: The key WY2015 results and highlights are presented in the beginning of the chapter, however, a short summary/conclusion section will be added at the end as well so the chapter ending is not so abrupt.

Comment #26: Panel Comment: Table 5B-8 presents the vegetation controls. It mentions repeated herbicide controls of hyacinths. While hyacinths are invasive species there are known to be highly effective for removing nutrients and other pollutants and can be harvested for producing biofuel. Has this been considered? Does the state prohibit them for this use?

Response #26: Author Response: Control of floating aquatic vegetation (FAV), such as water lettuce or hyacinth, is mainly conducted in the vicinity of water control structures (including pump stations) to prevent flow obstruction. FAV is also controlled when it negatively impacts coverage of other desired vegetation, such as submerged aquatic vegetation. Harvesting of vegetation in the STAs is not conducted for several reasons, including: (1) mechanical removal is very expensive and would be disruptive to the STA ecosystem if done on a large scale, (2) the lack of biomass disposal locations and high associated costs, and (3) a viable market for plant byproducts, such as conversion to biofuel, has not materialized in South Florida.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 5C

Larry Schwartz, Michael Chimney, Raul Novoa,
Tracey Piccone and Peter Rawlik

Level of Panel Review: Accountability
Reviewer: P. Dillon (AA)

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

Peer Reviewer Comment: *This sub-chapter presents a brief overview and update of eight studies that are in progress and that form the core of the Science Plan for the Everglades Stormwater Treatment Areas. It provides a well-written overview of this topic that is easy to read (with one exception) and understand. The Science Plan and the studies all focus on methods to reduce the output of phosphorus from the STAs. All 8 of the studies are in an early phase; thus, many understandably have few findings to present at this time while some others that started earlier have more results. All of the projects appear to be on schedule. The study schedules for future work are realistic and the proposed future activities are consistent with the detailed study plans. One minor point – the number of acronyms and capitalized phrases makes the introduction hard to read.*

SFWMD Response #1: Every attempt will be made to reduce the number of acronyms and capitalized phrases in the Introduction. Commonly used acronyms/abbreviations are also provided as part of the SFER Front Matter on the SFER webpage.

Peer Reviewer Comment: *The first project, on the use of soil amendments to control P flux project is just finishing phase 1 with a draft report prepared but no results included in this chapter.*

SFWMD Response #2: The draft report has just been completed. Preliminary results will be added to this SFER chapter as follows:

1. The literature review indicates that more than 100 materials that sorb P have been tested as a soil amendment; however, only a few of these products would be suitable for use in the STAs which discharge water into protected natural marshes. To determine the long-term usefulness and feasibility of application of any of the amendments in a treatment wetland, these products will have to be tested in the field.
2. No published data were found that document the long-term treatment efficacy of soil amendments or soil management techniques to reduce outflow total P concentrations in operating free water surface treatment wetlands.
3. Considering the uncertainties in treatment efficacy, potential impacts to STA operations and the economics associated with implementing any of these technologies at full-scale in the STAs, the Restoration Strategies (RS) Science Plan Management Team recommended that (a) the study move forward with planning for a field-trial of soil inversion in the STA-1W Expansion Area, and (b) not to proceed with study Phases II and III for testing soil amendments or any other soil management technique elsewhere in the STAs. The District's management team, the RS Steering Group, concurred with these STOP/GO recommendations.

Peer Reviewer Comment: *The second project, on development of operational guidance for FEB and STA regional operation plans, is in its third year and demonstrating good progress. It is nice to see some of the high quality research that is being undertaken appear in the peer-reviewed literature rather than just the grey literature. Again, it is good to see the modeling making significant progress. One criticism – I have no idea what the first point under progress (l. 126-128) means.*

SFWMD Response #3: The text in lines 126 to 128 will be modified as follows: An information gathering document, as prescribed in the Detailed Study Plan, will be prepared.

Peer Reviewer Comment: *The third project on evaluation of P sources, forms, flux and transformations is going well. Fig.5C-4 is very helpful. It indicates that work on P speciation should have begun about a year ago. I would be interested to hear more about the results of this task but there is no mention of this in the results section. Also, I remain dubious about the use of auto-analyzer technology for P sample collection, although the relatively high concentrations here make it less likely to fail.*

SFWMD Response #4: The contractor for the organic P speciation study was changed and that resulted in a delay in the study. Preliminary results will be presented in the 2017 SFER. Figure 5C-4 will be revised in the final chapter to reflect the current planned schedule.

SFWMD Response #5: The study entitled Evaluation of Sampling methods for TP includes an analysis of the use of auto-analyzer technology for P sample collection. The results will be presented in the 2017 SFER.

Peer Reviewer Comment: *The fourth project which focuses on periphyton-based stormwater treatment shows marked success in reducing P concentrations although this is a site where TP concentration of the inflow is relatively low to begin with. I am not clear on why the volume of water out is much greater than that coming in (Table 5C-1) – is this unmeasured leakage in that is included in the outflow? is it rainfall? Is the drop in TP a dilution effect?*

SFWMD Response #6: The volume of seepage entering the PSTA Cell from the surrounding water bodies (i.e., Upper SAV Cell, Lower SAV Cell, and Discharge Canal) was not measured but was assumed to be significant, as evidenced by much higher PSTA Cell outflow than inflow volumes, even after accounting for rainfall on the cell. Efforts are under way to develop improved estimates of seepage volumes and TP concentrations.

Peer Reviewer Comment: *The project on the influence of canal conveyance features on inflow and outflow P concentrations seems designed to tie up the loose end addressing whether the inflow and outflow canals themselves contribute or remove P. This is still in its early phase and few results are discussed. The report that one canal acted as a P source certainly requires further investigation.*

SFWMD Response #7: Agreed. Further investigation of the remaining STA canals is ongoing.

Peer Reviewer Comment: *The evaluation of inundation depth and duration for cattail sustainability began this year and already much progress has been made in terms of setting up the study site plots.*

Comment appreciated, no SFWMD Response needed.

Peer Reviewer Comment: *The seventh project focusing on improvements in estimating the water and P budgets has made substantial progress as well. Improvements in water budget estimates will translate directly into better P budget estimates.*

Comment appreciated, no SFWMD Response needed.

Peer Reviewer Comment: *The final project on evaluation of sampling methods for P is critically important. The project's results are only as good as the data collected. My experience with autosamplers is opposite to that reported here. I expected and saw lower TP values, largely because a portion of the P ends up in bacteria and phytoplankton attached to the sample bottle walls. My solution has been to use containers in the autosamplers that the sample can be digested in with no transfer to another vessel.*

SFWMD Response #8: Historically, the trend in TP water quality data indicates that grab samples generally have lower concentrations than in the autosampler flow-proportional composite samples. It is suspected that because the concentrations are close to the detection limit (2 µg/L) the data distribution is skewed resulting in composite samples being higher through simple random probability. Studies of microbial communities in South Florida canals report very low densities of algae in the water column as compared to periphyton colonies which dominate the ecosystem. Additionally, the District pre-preserves sampling bottles with sulfuric acid preventing microbial activity in the sample.

Question for Peer Reviewer b. Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report?

Peer Reviewer Comment: *The material is presented in a very clear and logical manner and is totally consistent with earlier versions. I like the charts provided with the third and fourth projects that show the timelines for the different activities very clearly.*

SFWMD Response #9: Graphical timelines for all studies in Chapter 5C are planned for the 2017 SFER.

Question for Peer Reviewer: Are findings linked to management goals and objectives?

Peer Reviewer Comment: *All of the studies contribute very directly to the management goals and objectives in that they all focus on ways of reducing P output from the STAs.*

Comment appreciated, no SFWMD Response needed.

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

Peer Reviewer Comment: *I suggest careful evaluation of the autoanalyzer sampling methodology is warranted. The question of P speciation hasn't been addressed in any detail in this or previous reports but needs to be.*

SFWMD Response #10: These comments are appreciated. As indicated above the study entitled Evaluation of Sampling Methods for TP includes an analysis of the use of field-deployed (remote) auto-analyzer technology for P sample collection, and the results will be presented in the 2017 SFER. As mentioned in SFWMD Response #4 results for organic P speciation will be presented in the 2017 SFER.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 6

Edited by Fred Sklar and Thomas Dreschel

Level of Panel Review: Technical
Reviewers: Dr. Siobhan Fennessy (AA), Dr. Michael A. Mallin (A)

Comment #1: Overall this chapter is quite strong, with most sections stating clearly articulated goals with clear links to restoration management objectives. The chapter is well organized, with most sections having an introduction, methods, results, and discussion, then a conclusion on the relevance of that topic to water management. This makes the text clear and easy to follow, and allows the science to take center stage. Some of the research presented is quite strong and will benefit our basic ecological understanding of the Everglades ecosystem, and its application will strengthen, and make more efficient, the various restoration activities. It is encouraging to see the positive direction that many of these projects are moving in.

Response #1: Every year we make a concerted effort to bring the science out of the well of jargon and into the light of English so that a broad audience can have a refreshing drink of knowledge. We greatly appreciate these reviews and enjoy hearing that our work is important.

Comment #2: Table 1 is very useful in summarizing the Everglades studies and demonstrating the links between mandates and the results of monitoring and research to support those mandates. However, comments such as projects are "providing interesting information", for example with the Cape Sable Seaside Sparrow, are vague and should be revised.

Response #2: Good comment. The sentence has been modified to read: "The Cape Sable seaside sparrow subpopulation D birds are surviving, do not seem to be affected by the C-111 Spreader Canal Western Project so far, and indicators of population health include observations of multiple clutches, success of multiple clutches, and on migration between subpopulations."

Comment #3: I found the discussion on page 6-9 regarding the relationship between the wading bird abundance and habitat very interesting. Line 195 makes the statement that nesting close to the ground in cattail is rarely successful due to mammal predation, yet there were 9,000 ibises nesting within. I have read (Dorcas et al. 2012) that there have been severe declines in everglades mammal populations from Burmese python predation – could this be a factor in improved nesting success?

Dorcas, M.E., J.D. Willson, R.N. Reed, R.W. Snow, M.R. Rochford, M.A. Miller, W.E. Meshaka, Jr., P.T. Andreadis, F.J. Mazotti, C.M. Romagosa and K.M. Hart. 2012. Severe mammal declines coincide with proliferation of invasive Burmese pythons in Everglades National Park. Proceedings of the National Academy of Sciences 109(7): 2418-2422.

Response #3: This is a valid hypothesis given the known effects of pythons on mammals in Everglades National Park (ENP) but it's not clear that it currently applies at this particular colony in the northern Everglades (WCA-3A-N). Python abundance is thought to be much lower in the northern than in the southern Everglades (ENP), and if this holds, then predatory mammal populations may not have been affected at this particular colony. Instead, I think our observation of 2 or 3 alligators, which are important deterrents of raccoons and other mammalian predators, probably kept any mammalian predators at bay. What we do not understand is why these alligators, which we have shown can cause considerable damage to ground nesting birds, did not have more of a disruptive influence.

Comment 4: Regarding the fires – I would think that burning would reduce the amount of overlying cover the prey items would normally have protecting them, and the wading birds were simply taking advantage of this. I would further speculate that after a burn, new growth would be nitrogen-rich, and invertebrate grazers as well as herbivorous fish would avidly feed on it, thus increasing their exposure to avian predators.

Response 4: In this case, a fire burned a large, moderately enriched area of dense cattail/sawgrass in the early wet season of 2014. We have previously shown from our Cattail Habitat Improvement Project that such areas typically support a relatively large biomass of crayfish but birds are unable to access this prey resource due to the density of the vegetation (Hagerthey et al. 2014). The fire clearly removed that structural constraint but water levels rose rapidly thereafter and became too deep for foraging until the following dry season (February 2015). By this time, the emergent vegetation had regrown and seemed to be far too dense for the birds to feed in. Surprisingly, this dense vegetation supported tens of thousands of foraging individuals for a number of months, which is orders of magnitude more foraging than a similar area of Everglades ridge and slough habitat can support. Evidently, foraging wading birds can tolerate a much greater density of emergent vegetation than we expected, and it is possible that the removal of the dead vegetation by fire was sufficient to allow birds access to the crayfish prey resource. We are currently testing this hypothesis and its potential as a management strategy in a current Active Marsh Improvement project.

Hagerthey, S.H., M.I. Cook, R.M. Kobza and S. Newman. 2014. Aquatic faunal responses to an induced regime shift in the phosphorus impacted Everglades. *Freshwater Biology*, 59: 1389-1405.

Comment #5: Page 6-19 – line 325 – please state (briefly) the tenets of the Trophic Hypothesis here.

Response #5: I believe that we have already done this by stating that wading bird reproduction, foraging, prey availability and hydrology are all tightly interrelated and that these relationships form the basis of the trophic hypothesis. The actual details of how they are related are referenced in the hyperlink.

Comment #6: Page 6-20 lists the performance measures, and they are ordered beginning with nesting bird numbers. However, the discussion on restoration targets is back on page 6-24, along with a nice table (6-3) that should be the lead for the section following the listing of performance measures. State what occurred, then follow that with the various explanations that are on pages 6-21 to 6-23.

Response #6: This section is set up to provide a brief summary of this year's wading bird nesting responses and their relationships to hydrology, as well as what these responses mean in terms of performance measures (e.g. location of nesting, timing of nesting, and nest success 3-year averages). The nesting responses and performance measures are integrated, the order seems relatively logical; however, we will consider this suggest for the next report.

Comment #7: Fish Distribution

This is an excellent study based on an effective experimental design with well-designed figures. The data show quite clearly the rapid response of fish to hydropatterns.

Response #7: Thank you.

Comment #8: Page 6-25 – line 550 – please state enclosure dimensions here.

Response #8: "(12 m x 4 m)" was added to the sentence.

Comment #9: Page 6-31 – line 636 – please remove the word "hopefully". Also, making the text more active is encouraged. For instance, this sentence might read "Next year we hope to experimentally examine the relative roles of recession rate and …. "

Response #9: Good point, the sentence has been modified as suggested.

Comment #10: Page 6-31 – lines 637-644 – the rapid use of newly flooded habitat is proscribed to habitat profitability. I assume that means the availability of terrestrial insects in the newly flooded habitats? This can be easily tested by sampling for prey items of newly flooded habitat compared with habitat that has been flooded for weeks. Something to add into next year's study perhaps.

Response #10: We did indeed examine insect prey abundance this year but we are still analyzing the samples. We shall prove these data in next year's report.

Comment #11: Page 6-34, top – The text states that two organizations monitor the sparrows, but only the NPS staff is mentioned.

Response #11: Text has been added to the paragraph to clarify. The paragraph now reads, "Both the SFWMD and the National Park Service monitor the sparrows and both support field surveys.

In addition, the National Park Service staff from the ENP conducts surveys via helicopter at predetermined sites in all six subpopulations, indicated in Subpopulation D by the regularly spaced circles in **Figure 6-20**. The helicopter surveys are conducted once during the breeding season to locate calling males. This survey, repeated annually, provides a standard protocol to address questions of population estimates and changes over time. In contrast to the annual helicopter surveys, field crews contracted by both SFWMD and the Park conduct up to daily ground surveys within the subpopulations (in 2015, surveying Subpopulations A, B, and D), visiting several times a week and more frequently when active nests are located. In Subpopulation D, field crews monitor the previously identified breeding grounds shown in **Figure 6-20** where three stage gauges, CSSD1, CSSSD2, and SWEVER4, mark the perimeter of the area where recent breeding has occurred. Field crews determine the number of males and their territories, females in the area, and the nests with eggs, nestlings, and fledglings. Nests are monitored frequently to assure that nesting was completed and chicks were banded or until the nest failed. The scientists follow the progress of courting, nesting, and other behaviors. Because individual birds are banded as adults or as chicks, demographic data can be associated with each bird. Through identification of banded birds, they can determine whether a sparrow is local or has immigrated from another subpopulation, its gender, age, and other information."

Comment #12: Page 6-35, bottom – please explain what "degradation of two roads" means (removal? destruction?). Are there results of the vegetation removal efforts to present?

Response #12: Text has been added to the paragraph to clarify. The sentences now reads, "Hydrologic improvement in 2014 included cutting three 40-foot sections three feet deep across a dirt road that had impeded southward flow of water, causing local ponding (SFWMD, 2014b). Flow across the roads is occurring and ponding has been reduced."

Comment # 13: Page 6-35, bottom – Are there results of the vegetation removal efforts to present?

Response #13: None have been formally documented. We will try to include them in a future report.

Comment #14: Page 6-41-line 885-9 – monitoring the "health" of the islands in order to determine how the structure and function relate to restoration is a lofty but vague goal. How is ecosystem health defined? Using the term 'ecological condition' seems more appropriate and relates more directly to the goal of restoring ecological integrity. In light of that, what are the goals for the three measurements presented here? The data for ET is not presented, so this should be removed from the specific objectives presented here, or an explanation offered as to why it isn't included.

Response #14: I changed the term healthy for ecological condition as the reviewer suggested. It is important to remember that the data presented in the 2016 SFER is part of on-going tree island project in which some data have been presented before (RECOVER 2014), therefore are not presented in the current report (e.g., evapotranspiration, hydrology, and nutrients) but they are implicitly discussed by presenting Table 6.6.

RECOVER. 2014 System Status Report. Restoration, Coordination and Verification Team, c\o United States Army Corps of Engineers, Jacksonville, FL, and South Florida Water

Management District, West Palm Beach, FL. Available online at http://141.232.10.32/pm/ssr_2014/ssr_main_2014.aspx.

Comment #15: Page 6-41 – The last few lines on this page discuss the variations in 13C values – please explain what high or low 13C means, physiologically to plants. What is the target for determining 'health'? What insight do these data give?

Response #15: Text to explain the importance of measuring stable isotope, including ^{13}C and ^{15}N has been added, "In general, stable isotopes indicate that tree island plant communities can reflect P availability and hydrologic status in ^{15}N and ^{13}C signatures. Our results on patterns of isotopic signatures suggest that higher ^{15}N concentrations correspond with greater N demand and higher P availability while higher ^{13}C concentrations correspond with greater C demand."

At this point we do not have a specific target, the importance of isotopic signatures is their association with phosphorus availability and nitrogen cycling. These data will be further discussed in the final report of this project.

Comment #16: Page 6-41 – A figure, perhaps a drawing, of a healthy tree island vs a "ghost' island would be a great help here for the reader to visualize the difference, as would a description of what a ghost island is. Label the key areas on the drawing. I am sure there are such figures available within the SFWMD.

Response #16: This is a good suggestion and we will work on developing such a figure for the next report. To better visualize the differences on tree islands, we included pictures of the actual tree islands where this project is being carried out.

Comment #17: Page 6-42, Figure 6-23- this is not an easy figure to interpret. Please clarify. A key to the different colors would be helpful.

Response #17: Thank you for this good idea. Text has been added describing the colors, "The blue color in the image illustrates wetter hydrologic conditions and the yellow illustrate drier conditions in WCA 3A. The green color illustrate the presence of tree islands (tear-drop shape) and cattail distributed mostly along canal and levees."

Comment #18: Page 6-44 – line 916 - The stable isotope data are not mentioned here, is it meant to be the basis for a metric?

Response #18: Text has been added to explain the basis of using stable isotope as part of the metric (P availability), "In particular, isotopic signature results indicate that tree island plant community reflect P availability and hydrologic status in ^{15}N and ^{13}C. This evidence corresponds with observations of high TP soil and TDP soil water concentrations. Thus, ion composition and water quality samplings illustrated key findings that were useful in the development of metrics to assess tree island condition (Table 6.6)."

Comment #19: Table 6-6 – It seems that a single reference site (one intact tree island) is the basis for defining the target for metric performance. Relying on a single site (n=1) to set expectations is risky as natural variability is not accounted for. If this is the case the performance targets will be difficult to defend. If the targets were drawn from earlier data sets then citations should be included.

Response #19: We agree; using one intact tree island is not enough to set reliable metric performance. In an ideal world, we would use n=3 (to say a number) but resource limitations restrict the work effort. On the other hand, it is important to note that the tree islands under study were choosing to follow a hydrologic gradient going from wet (3A) to dry conditions (3B). In this sense, we are using these tree islands to evaluate a response surface across a hydrologic gradient.

Comment #20: Ecosystem Ecology – The discussion of the results of the cattail herbicide and burning attack was very interesting. Wading birds came in rapidly and their use persisted for many weeks. It is interesting that on Page 6-49, line 1060, the authors note that "As expected, this management approach produced habitat highly attractive to foraging wading birds'. This seems to contrast with page 6-10, where on line 201 the feeding in the burned areas is described as 'unusual behavior". So it is unusual or expected?

Response #20: We thought it unusual for the birds to feed in the naturally burned area because our observations occurred 8 months after the fire when the emergent vegetation had fully regrown and it was far denser than any vegetation we had previously seen the birds forage in. By contrast, we expected birds to forage in the AMI plots because our management technique removed the emergent vegetation and created typical slough-like foraging-habitat.

Comment #21: Page 6-46 – line 1010 - There is an extra word here.

Response #21: The text was modified to correct this, "An examination of hydrologic conditions in the area indicate that while the western plot experienced an extensive dry out, May–June 2011, any water inputs during the WY2012 came from Stormwater Treatment Area (STA-2) at the west-northwest side of WCA-2A."

Comment #22: Page 6-47 – The east-west difference in spatial patterning is not described, yet is cited as an important factor in the results. Please add where appropriate.

Response #22: Our hypothesis in CHIP and AMI was that the creation of the slough habitat and its associated algal and SAV community, would result in a change in the food web, which in turn would create greater wading bird foraging. Perhaps the inconsistency is that birds foraged immediately after the burn, prior to the evolution of a new habitat, thus showing they are able to access prey species made vulnerable due to removal of emergent vegetation.

An examination of hydrologic conditions in the area indicate that while the western plot experienced an extensive dry out, May–June 2011, with and any water inputs during the WY2012 coming came from STA-2 at the west-northwest side of WCA-2A.

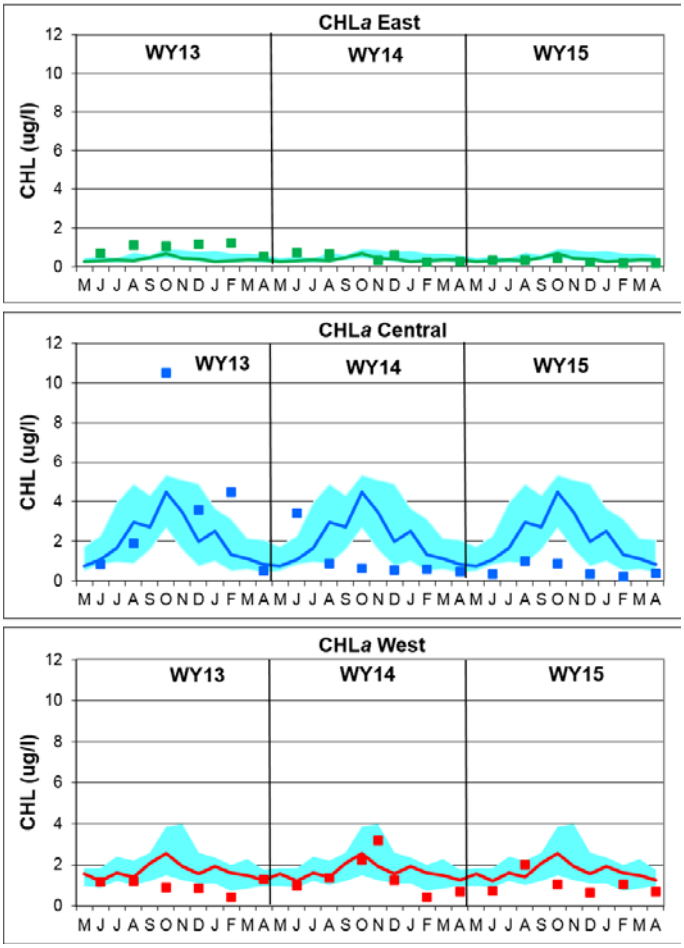
The primary difference we hypothesized between the east and west – was a hydrologic gradient- however, as noted on lines 1013 and 1014 the east- west hydrologic gradient was generally negated. However, we did note there were still differences in vegetation patterning. See lines 1038 to 1041 and photos in Figure 6-25.

Comment #23: Overall, the active marsh improvement projects seem to be generating a lot of excellent and practical data that can be applied.

Response #23: Thank you for your comment. We agree that the active marsh improvement projects are providing important data to guide restoration of the ridge and slough landscape.

Comment #24: Florida Bay Water Quality – This on-going study builds on a comprehensive and extensive data set that is invaluable in monitoring restoration success. Only one comment on this section is on the Figures (6-28-30), which have different ranges of data on the y-axes. It would make comparisons easier if the y-axes covered the same range of values, or if the differences were noted in the legends to alert the reader.

Response #24: Graphs have been modified according to the suggestion. For an example, see the figure below:



Comment #25: Coastal Lakes – Please add some information about these coastal lakes, such as size, average depth, trophic state (if available). These systems are rarely mentioned as part of the everglades to those not intimately involved in local research.

Response #25: A description of the central lakes was added, "The coastal lakes area of northern central Florida Bay is currently of intense interest due to the newly initiated C-111 Spreader Canal Western Project, which will establish more natural flows to Taylor Slough by creating a hydrologic ridge along the eastern margin of Taylor Slough. The project is designed to reduce seepage and retain more water in the slough to improve the timing, distribution and quantity of water in central Florida Bay. The lakes region is also expected to benefit from the additional fresh water as a result of C-111 project operations. The West Lake chain, including West Lake, Long Lake, and Cuthbert Lake, lies on the western edge of Taylor Slough. The Seven Palm Lake chain in central Taylor Slough includes Seven Palm Lake, Middle Lake, Monroe Lake, and Terrapin Bay. These water bodies are of great importance to Florida Bay because they support large seagrass meadows and mangrove forests, provide critical nekton and bird habitat and they supply fresh water to central Florida Bay. The shallow (1-m depth), connected lakes are expected to benefit from C-111 Spreader Canal Western Project operations due to increased freshwater flow that will reduce dry season salinities and increase water transparency, improving habitat conditions for fauna such as birds and fish. The lakes are also often high in nutrients and algal biomass that can exceed 100 μg L^{-1} chlorophyll. Increases in freshwater flow may alter nutrient dynamics in as yet unknown ways that could impact phytoplankton and benthic vegetation in the lakes and that could be transported downstream to sensitive Florida Bay waters. The District, in collaboration with Florida International University initiated a study in September 2013, with the objectives of understanding water and nutrient flows into the lakes, developing a hydrologic budget and determining downstream impacts on Florida Bay."

Comment #26: Page 6-60, Methods – It appears that each group involved in the monitoring is using a different method for the vegetation monitoring. The reference to earlier studies is appreciated, but adding a sentence or two here justifying why these data are comparable is warranted.

Response #26: Each group is using a slightly different method. Therefore, each group's data is only compared to itself. We make no judgement about how the changes at sites from one group relate in magnitude or importance to changes at sites from another group.

Comment #27: Page 6-60 – Paragraph line 1249 – the frequency of H. wrightii in 2015 looks to be within the range of variability for Twin Key and Rabbit Key. Can these be tested for significance?

Response #27: They can, and they were. 2015 may not have been significantly different from the long-term mean and variability in all the basins, but it was significantly different from 2014. Thus, the statements about significance in the text were kept to a "significant decline" or "significant decrease."

Comment #28: Please reconcile the two Florida Bay SAV chapters. On page 6-60, bottom, it is stated that in WY2015 the overall benthic vegetation community is stressed. On page 6-62, the combined indicator score for 2014 is largely rated "good". The two chapters share one of the co-

authors, so please come up with some sort of compromise (I realize the two may be referring to two adjoining years, but it is still conflicting as written).

Response #28: The two statements are slightly different and are referring to two different years as stated in the introduction on page 6-62. The statement about the community being stressed refers to the directional changes between years (the fact that benthic vegetation decreased between WY2014 sampling and WY2015 sampling). The statement about the combined indicator score for 2014 being largely good refers to the static snapshot of conditions in early WY2014, which had abundant SAV throughout most of the bay. Having the reference or explanation of the metrics would clarify this as suggested in the next comment.

The new text reads, "An indicator was developed that expresses and tracks the status and trends of SAV for Florida Bay (Madden et al. 2009). The indicator uses monitoring data to assess four metrics capturing the status of abundance (areal extent and density), and species diversity (species dominance and target species). The four indicators are combined to give an overall score for the bay each year. For WY2014, a bay-wide composite score of yellow (fair) summarizes the overall system status, unchanged from the previous assessment. Broken down by individual zones, the combined indicator remained good in the Northeast, Central, and Western zones, and fair in the Southern zone for both 2013 and 2014 (Figure 6-35). The SAV indicator for the Transition Zone (the mangrove ecotone, embayments, creeks, and lakes in the southern Everglades wetland) improved to good in WY2014 from fair. Data for the unusually hypersaline WY2015 will be evaluated as part of the 2016 assessment and is expected to show a seagrass community under stress.

Comment #29: Page 6-62, first paragraph – Please include citations or a short explanation of the metrics used.

Response #29: The reference was already included in the reference list, but was not cited until the *Methods* section:

Madden, C.J., D.T. Rudnick, A.A. McDonald, K.M. Cunniff and J.W. Fourqurean. 2009. 1683 Ecological indicators for assessing and communicating seagrass status and trends in Florida 1684 Bay. Ecological Indicators, 9S(2009):S68–S82.

A text edit for line 1275 added in the citation, "Florida Bay (Madden et al., 2009). The indicator uses monitoring data to assess four metrics capturing the status of…"

Comment #30: Peat Collapse – This section discusses the potential impacts of sea level rise on the chemical and physical breakdown of the peat soils in the shoreline marshes. It is all bad news, but it is important that the SFWMD continue to support such efforts for planning purposes.

Response #30:

Comment #31: Page 6-65, first paragraph – If CO2 and CH4 emissions were measured as a function of salinity, we expect that methane emission rates will decrease as sulfate increases. See for example:

Poffenbarger, H.J., B. A. Needleman, and J. P. Megonigal. 2011. Salinity influence on methane emissions from tidal marshes. Wetlands. 31:831-842.

Response #31: Page 6-65, first paragraph – If CO_2 and CH_4 emissions were measured as a function of salinity, we expect that methane emission rates will decrease as sulfate increases.

Comment #32: Given that, it isn't clear why the marshes were bigger C sources with increased salinity. The means by which these two C fluxes were combined into an overall C flux isn't provided. Please explain.

Response #32: Although we are collecting methane samples, this data is not presented or incorporated into Figure 6-38. As a result, we have revised the figure caption to read, "Net ecosystem exchange was measured as carbon (CO_2) flux in freshwater (FW) and brackish water (BW) marsh sites. Both sites were a source of C to the atmosphere (positive CO_2 flux) in the dry season and this effect was amplified with increased salinity in the brackish marsh." We intend to include a discussion of the methane data in future reports.

Comment #33: Decomp Physical Model – First – change the name of this chapter. It is an atrocious title and tells the reader absolutely nothing. Change it to "Experimental restoration of sheet low to improve habitat" or something like that. It contains important restoration information so make it something someone who is not a modeler will want to read.

Response #33: We have changed the title of the section, but left Decomp Physical Model in the title. The DPM name indicates this is a physical experiment, not a math model. Also since DPM is a well-known project, omitting the name will add confusion and might sacrifice reader interest. The new title of the section reads, "Experimental Effects of Sheetflow on Sediment Redistribution in the Ridge-and-Slough and Canal Backfilling – Interim Findings of the Decomp Physical Model."

Comment #34: Page 6-76 – last line of the report – it says this process could potentially alter P cycling in the canal. You need to be less vague. How would it be altered? What are the implications?

Response #34: Changed the last paragraph to read, "Canal velocities roughly doubled under high flow, reaching 7–8 cm s^{-1}, above critical erosion thresholds for Everglades sediments. Therefore, the widespread changes in canal sediment sources, as evidenced by molecular biomarkers, may be caused by velocity changes in the canal itself. Given the high TP of canal sediments, this process could potentially alter P cycling in the canal. Specific impacts include (1) increased water column TP in both canals and the marshes downstream of the levee gap, (2) changes in the vegetation growth and vegetation types within canals (e.g., exotics that further reduce through-flow of natural (low-TP) sediments, promote anoxic conditions, or produce high-TP sediments), and (3) changes in marsh vegetation downstream of canal/levee gaps (e.g., cattails). To address these questions, subsequent flow events will place greater emphasis on quantifying the sources (biomarkers) of advected sediments entering canals from adjacent ridges and sloughs; evaluating biomarker signatures and nutrient contents of benthic sediments within the canal; recording vegetation changes within and adjacent to canal treatments; and conducting more focused analysis of water

column TP (including sediment TP) in canal and downstream marshes. The extent to which partial or complete backfilling interacts with these ecosystem responses will also be evaluated."

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 7

LeRoy Rodgers

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

Comment #1: Does the draft document present a defensible account of data and findings for the areas being addressed that is complete and appropriate?

This chapter provides a very thorough account of the current status of non-indigenous plant and animal species and of management activities related to eliminating or at least reducing their effects on the environment of South Florida. Although eradication is always the preferred solution, this is often impractical or impossible, and controlling the spread of invasive species may be the most that can be achieved. The report acknowledges that there are many non-indigenous species for which there are no data, but it is clear that attention is given to those with the greatest potential to damage the ecosystem. The 3-level scoring (red/yellow/green) approach is an effective way of presenting the level of risk for each species.

The chapter is very thorough in its approach, and I appreciated particularly the species by species discussion on pages 13-42. The authors are open about the challenges, indicating clearly species for which they expect to have trouble implementing a successful control programme. One small addition would be helpful for the reader – in a few cases the year of introduction of the species is reported but usually it is not given. This would give the reader an idea of how fast new species are spreading.

There are some notable successes to date. The melaleuca story is an excellent example of what can be accomplished with adequate funding and a co-ordinated approach with buy-in from all partners. It appears that the programmes in place have reduced the extent and impacts of several key invaders, although the region is clearly under great pressure from a continuous influx of new species.

Response #1: The authors thank the reviewer for the positive comments and appreciate acknowledgeent of the unique challenges faced by South Florida land managers, restorationists, and resource managers with regard to invasive species management. The authors will improve consistency of reporting year and mode of species introduction in the chapter. This information is not always available, but we will attempt to include when it is.

Comment #2: Is the synthesis of this information presented in a logical manner, consistent with earlier versions of the report?

This chapter is very well-written and organized and very informative. The current status of the most important non-indigenous species is outlined and any control activities that are in place are updated. I would like to see a little more detail on control methods, specifically which herbicide is being used with which species.

Note that in my copy Table 7-1 is blank.

Response #2: Again, the Authors appreciate the reviewer's constructive comments. We concur with the suggestion to include information on types of herbicides used for different invasive species. We will incorporate a small section in the invasive plant management overview section (probably a table) in the final draft. Also, note that Table 7-1 was intentionally left blank in the first draft. This was necessary because total fiscal year expenditures were not completed at the time we submitted the review draft. This table will be complete in the final draft.

Comment #3: Are findings linked to management goals and objectives?

The linkages between the control programmes, the monitoring results and management objectives are very clear. The amount spent on control programmes is substantial but it is clear that increases are needed make headway against the increasing number of potential serious threats. Prioritizing based on risk, as is being done, is essential.

Response #3: As the reviewer indicates, prioritization of resource allocation based on risk is essential, particularly when fiscal resources are limited and management challenges are at times overwhelming. Resource managers and invasive species scientists continue to refine methods to assess risks on non-indigenous species. The authors hope to provide updates on progress in this area in the 2017 SFER.

Comment #4: Is there any constructive criticism and guidance to offer for the District's large-scale programs?

Biological controls for melaleuca, water hyacinth and old world climbing fern have been introduced and others are being considered and worked on. As mentioned last year, the scientific literature is full of examples where biological controls have had unexpected adverse effects. It is critical that extensive studies be carried out before any biological control agent is introduced.

Response #4: Thank you for this comment. The reviewer indicates that there are many examples of unexpected adverse effects of biological control. The vast majority of such cases involve either inadvertent biological control releases or deliberate releases that lacked sufficient host-specificity testing. All current biological control efforts conducted for natural weed management in Florida utilize methods of "classical biological control," which involves intensive risk assessment protocols (including stringent host-specificity testing). This approach has yet to result in unexpected impacts to native species. However, a philosophy of extreme caution is advised, as the reviewer suggests. The District's biological control collaborator, USDA-ARS, is required to exercise extreme caution in its evaluation of potential agents through years of overseas and quarantine host-specificity and environmental harm testing. An interagency technical advisory group, USDA-APHIS, and USFWS must all concur that the agent will only complete its life cycle by feeding on the target weed species and will not act as a pest to humans or agricultural interests before it can be introduced as an agent. This concurrence is based on years of research to evaluate numerous facts of the level of the plant insect interaction including taxonomy, natural geographic range, pathogens, parasites, hyperparasitoides, closely related species in introduced range, and possible direct and indirect impacts of agent on T&E species.

Comment #5: l. 218 – "a new insect was developed" – what do you mean, not created, surely.

Response #5: "developed" is in reference to the years of research needed to receive approval to release from quarantine as a biological control agent. We will re-word to "*a new insect was approved for release*"

Comment #6: l. 318 – it is good to see the CISMAs developed but it is not clear whether or not different CISMAs overlap spatially, e.g. is there more than one CISMA concerned with the Kissimmee watershed? the Everglades? A figure like 7-6 for each CISMA mentioned would be useful, or a single figure with the different CISMAs showing.

Response #6: CISMA boundaries are generally defined by county or management area boundaries. Creating separate maps for each CISMA will likely exceed space limitations, but it may be feasible to increase the size of the current map and zoom into the South Florida Ecosystem (District boundary). The Authors will evaluate options and discuss with the Chief Editor.

Comment #7: l. 1190 – a good example where regulatory tools are inadequate but should be easily fixed

Response #7: The authors concur. Regulatory driven prevention appears to be a very cost-effective invasive species management tool. This tool should be utilized more frequently to reduce expenditure of public treasury for long term invasive species control and minimize impacts to native ecosystems.

RESPONSES TO COMMENTS ON THE DRAFT 2015 SFER – VOLUME I, CHAPTER 8

Joyce Zhang, Bruce Sharfstein, Lesley Bertolotti, Thomas James, Brian Tilles and Odi Villapando

Level of Panel Review: Technical
Reviewers: AA Reviewer: Dr. Michael A. Mallin (AA), Dr. Peter Dillon (A)

Comment #1: The writers have produced an excellent summary of the ongoing work on Lake Okeechobee and its watershed, and have provided a very clear picture of the status of the lake. They have demonstrated more progress towards environmental targets than in other years, although some targets remain elusive. There still has to be great concern regarding achieving the TP target value of 140 metric tons/year. Although the total phosphorus load decreased in WY2015 compared with the previous year, the in-lake TP concentration did not improve, and the frequency of cyanobacteria blooms increased.

This chapter is well-written, concise and easy to follow. The conclusions are supported by the data presented. That having been said, some of the sections left the reviewers hanging, and need more explanatory material needed. Some re-arrangement of data presentation could help as well.

Response #1: Thank you. Comments are appreciated.

Comment #2: Regarding the lakeside ranch and the pilot-scale stormwater treatment areas – please add the N removal results as well as the P removal results in these brief summaries.

Response #2: The District routinely calculates and reports total phosphorus load reductions in accordance with related permit requirements. Although there is currently no permit-related requirement to report on total nitrogen load reductions, it is recognized that this is relevant information for the Lake Okeechobee Watershed monitoring and restoration efforts associated with the BMAP/TMDL program and related needs of other regional stakeholders. Therefore, total nitrogen load results for the Lakeside Ranch and Taylor Creek STAs will be included in the final report.

Comment #3: Page 8-4, line 158 – please add the names of the other exotic invasive species to this paragraph.

Response #3: The authors will add the names of the exotic invasive species of primary concern to this paragraph in the final report. These include torpedograss (*Panicum repens*) and exotic water grass (*Luziola subintegra*).

Comment #4: Page 8-4, lines 165-170 – It would be useful to add a bit more material here on microcystin levels in the lake, and what they mean.

Response #4: Beyond noting that microcystin levels increase or decrease from year to year, there is little that can be stated about conditions on Lake Okeechobee as the District routinely collects samples at only six sites each month, as presented in the *Algal Bloom Monitoring* sub-section of

this chapter. The authors will revise the final report to indicate that there was an increase in microcystin levels in WY2015, although the concentrations measured tended to be relatively low at the six monitored locations.

Comment #5: Page 8-5, top – it would be worth mentioning is there were or were not any fish kills in the lake during this period.

Response #5: The large size of Lake Okeechobee makes it impractical to routinely survey for fish kills. As such, the District only investigates such situations when the public or another agency indicates that there may be a problem. Consequently, our data on fish kills is incomplete, largely anecdotal and, hence, of little value in assessing the real extent of fish kills on the lake.

Comment #6: Table 8-1 – Lakeside Ranch Stormwater Treatment Area – under estimated water quality benefits, it says an average annual load reduction of 19 mt/yr, but it does not say of what. Probably TP, so please add. For WY2015 status update, please add the realized TN reduction as well.

Response #6: Yes, it is TP and we will reword the sentence in the final report. As noted in response to comment #2, the TN load reduction results also will be added.

Comment #7: Taylor Creek STA – under 2015 status update – again please add TN removal.

Response #7: As noted in response to comment #2, the TN load reduction results will be added in the final report.

Comment #8: Regarding P removal - the Lakeside Ranch STA is working well based on better than expected P removal, while the Taylor Creek STA is not as effective – any comments on why the difference in P retention?

Response #8: The two STAs were designed for different removal efficiencies. The Taylor Creek STA was designed for a 38% TP removal efficiency, while Lakeside Ranch was designed for a 43% efficiency. Although Taylor Creek did not achieve the annual 2 mt per year TP removal target, it did remove 55% of the TP it received in WY2015, which is better than the design TP removal efficiency of 38%. The Taylor Creek STA has been operational since 2006. Initially, Taylor Creek performed better, but has declined over time. One reason for this could be the accumulation of P in the sediment from years of P loading to the STA. Vegetation enhancement has been implemented to help improve the long-term treatment efficiency of this STA. While the TP removal efficiency of the Lakeside Ranch STA is an impressive 85% in WY2015, it has only been operational for two years. The accumulation of P in sediment from years of high P loading to the STA is unknown in terms of how this will affect future performance.

Comment #9: Regarding the Hybrid Wetland treatment technology – please expand on the general description in reference to what chemicals are added – is it to precipitate P?

Response #9: The chemical added is alum. In addition to alum, a polyaluminum chloride compound was used at some facilities as a supplemental coagulant to improve flocculation and reduce total P and total Al export from the Hybrid Wetland Treatment Technology (HWTT) facilities. The HWTT combines the strengths of both wetland and chemical treatments to maximize P removal and minimize chemical use. Chemical coagulants are added, either continuously or intermittently, to the front end of the treatment system, which contains one or more deep zones to capture the resulting floc material. A fundamental concept of the HWTT is that the floc resulting from coagulant addition generally remains active and has the capability of additional P sorption.

Comment #10: Page 8-22 – good to see some innovative economically-based treatment projects underway. The buy-in on private land program seems like a very good partial solution to the water fluctuation issues. Regarding the West Waterhole Project, on line 468 it says the marsh filters the nutrients out. Of course filtration is a physical process, and this statement is oversimplified. Please state in a couple sentences how P is removed in the wetlands (plant uptake? Soil sequestration?). Is the N removal via denitrification? Plant uptake?

Response #10: Research at the site has not been conducted to determine the exact mechanism or processes of nutrient removal. It is correct that it could be any number of physical and/or biological processes not solely filtration. This language will be modified and the reference to filtration removed in the final report.

Comment #11: Line 485 – what is the estimated P removal by the 8 funded projects? There is some concern that nutrient retention in the marsh could be short-term unless the biomass is harvested; decomposition of the biomass made release a large portion of the nutrients back into the water eventually.

Response #11: While there is an anticipated ancillary nutrient removal benefit from water retention projects, the primary focus of these eight projects is the storage of excess water and reduction of harmful flows to downstream water bodies. Therefore, nutrient removal estimates are not provided.

Comment #12: The Payment of Environmental Services program is innovative, and other areas of the country may wish to adapt such a program. Please state where the funds come from; i.e. state government, federal government, private donations, etc. Additionally, please state the going rate(s) that are paid to ranchers – is it based on the amount of N or P removed? By acre of ranchland converted to wetland? This puts a monetary value on wetlands that is sorely lacking in economic discussions of wetlands protection elsewhere in the country.

Response #12: Thank you. The District meets with other entities interested in implementing similar payment for services programs to share our experiences with the program. To date, Northern Everglades Payment for Environmental Services (NE-PES) projects have been state funded. Also, the initial funding for the pilot program (Florida Ranchlands Environmental Services Program) that lead to the PES program included grant funds that were obtained by World Wildlife Fund. Going forward, other sources of funding will be investigated and evaluated. PES payments are based on estimated volumes stored for retention projects and estimates of nutrients removed for nutrient removal projects. Calculated costs for all projects in our PES program (including construction costs, operation and maintenance, and service payments) range from $99 to $157 per acre-foot of average annual storage.

Comment #13: Page 8-23, line 516 – the watershed (and lake) monitoring programs have been very effective in scope and implementation. Past overlap in effort is now removed and some gaps have been filled.

Response #13: The authors agree.

Comment #14: Page 8-25, line 538 – the data, as in previous years, show how dependent variations in loads are on changes in hydrology. The TP load declined by 159 mt despite a small increase in flow. This is promising since it indicates that the average concentration of the inputs dropped. Again, this has a lot to do with hydrology because it seems that the flow increased in the inflows with lower TP and not in those that typically have higher TP. This another example why volume-weighted inflow concentration is generally better than loading when assessing progress as it factors out hydrologic effects reasonably well.

Response #14: The authors agree that there is a benefit to showing volume-weighted (also called flow-weighted mean concentrations).

Comment #15: Page 8-28, line 614 – the N/P load ratio looks to be a little over 12, indicating P limitation, which is no surprise but is probably worth mentioning.

Response #15: While the five-year average of TN:TP ratio is 11.8:1, this is still far below the goal of 22:1, and the ratio slightly declined from WY2014 to WY2015 (see Table 8-12). Similarly, the five-year average DIN:SRP ratio (4.5:1) is also well below the goal of 10:1 and also declined from WY2014 to WY2015. Because of the abundance of SRP and low light levels in the lake, it is unlikely that the phytoplankton are P limited.

Comment #16: Page 8-29, line 629 – the volume-weighted TP concentration of all inflows declined, is now lower than the lake concentration, but the lake concentration increased; this deserves more explanation. It may be partly due to the residence time of the lake being >1 year, partly due to internal loads?

Response #16: It is best to focus on the five-year moving averages rather than the year to year variations because (1) the residence time (based on surface outflow) for Lake Okeechobee averages around three years; (2) the variation in flow and load in the watershed due primarily to yearly variations in rainfall; and (3) the variation of water column nutrients are strongly affected by sediment water interactions, which vary primarily on wind. Evaluating the five-year moving averages show that lake concentrations have rarely been greater than inflow concentrations, the exception occurring after the WY2005 and WY2006 hurricane events (see Figure 8-9).

Comment #17. Page 8-30 – Table 8-5 – These drainages are ordered by subwatershed, which themselves do not seem to have any particular logic in their placement in the table. If P loading is the critical factor in this table, why not have the table rank the drainages (or even subwatersheds) in descending order from highest to lowest in terms of unit area load, or even TP load. That way the reader can focus on the most problematic areas (as noted in the discussion) more easily. The ranking comment goes for the following tables (Tables 8-5 to 8-8) as well.

Response #17. For sub-watersheds with drainage basins, the sub-watershed is listed alphabetically in the table. For sub-watersheds with no drainage basins included (e.g., Upper Kissimmee), it is listed at the bottom of the table in alphabetic order. Even though P loading is the critical factor in this table, the table is ordered in this fashion to facilitate year-to-year comparisons. As TP parameters vary year by year at the basin and the sub-watershed levels, ordering them as suggested would make year-to-year comparisons difficult.

Comment #18: What this report section needs is more description of what landscape factors within these troublesome subwatersheds and drainages account for the highest loading of P and N. As the section reads now, it merely reports data with no analysis of the meaning. This is a critical point.

Response #18: This year's chapter is the annual update of the lake and its watershed, which has specific requirements outlined in the Northern Everglades and Estuaries Protection Program (NEEPP) legislation (Section 373.4595, Florida Statutes). In-depth analysis including trend analysis and identification of areas of concern has been presented in previous Lake Okeechobee Watershed Protection Plan Updates, with the most recent analysis provided in the 2011 update. We will consider updating the trend analysis in the next three year update of the plan.

Comment #19: Table 8-11 - Overall, an impressive collection of projects. Good to see the WAM sensitivity and uncertainty analysis – badly needed.

Response #19: The authors agree. It is anticipated that the WAM sensitivity and uncertainty analysis will be completed by September 2016.

Comment #20: The water quality monitoring and the biological monitoring are well-conceived and appear to be well-executed.

Response #20: Thank you.

Comment #21: Lake Okeechobee has some impressive performance standards to shoot for.

Response #21: The authors agree.

Comment #22: Page 8-39, line 808 – please state how the diatom to cyanobacterial ratio is computed. Is this by biovolume?

Response #22: Yes, the ratios are based on biovolumes using quarterly data from four pelagic sites.

Comment #23: Page 8-39, line 810 – any explanation for the increased frequency of cyanobacteria blooms? Perhaps a combination of temperature, hydrology? Possibly related to burning of cattail? There is considerable evidence that blooms are linked to the Fe cycle in at least some locations; I presume that there are no iron data available?

Response #23: We do not have an explanation for the increased frequency of cyanobacterial blooms other than that on a statistical basis, blooms are more likely to occur on the lake during warmer months and when the lake stage in spring is 14 ft (4.3 m) NGVD or higher. The reviewers' suggestions regarding iron are intriguing as most blooms appear to originate in areas of the lake adjacent to major tributary inflows. Although the District does measure Fe because it is part of the Florida Class 1 drinking water standards due to potential laundry staining, the temporal and spatial distribution of these samples is probably insufficient to investigate this hypothesis. Most recent cattail burns have occurred in regions of the lake distant from the typical locations where bloom conditions are encountered, so the potential linkage of these two events would seem to be tenuous at best.

Comment #24: Page 8-40, table 8-12 – Please clarify that the N/P ratios are by weight (if so). Readers will compare your values to the Redfield ratios, which are molar not weight ratios.

Response #24: Good point. In the final report, we will add a footnote to Table 8-12 indicating that the reported ratios are by weight and not based on molar equivalents like the Redfield ratio.

Comment #25: Page 8-42, line 877 – until the last few years there is a fairly consistent increase in TP in the lake despite, if anything, a slight decrease in volume-weighted inflow concentration. Does this suggest that other processes, e.g. internal load, have an increasing role?

Response #25: After the hurricanes in 2004 and 2005, the in-lake TP concentrations were very high but declined to pre-2004 levels around 2010. Since then, the values have ranged from 123 µg/L in 2010 to 96 µg/L in 2012. This is within the range of annual values observed from the early 1990s. It is likely that the year-to-year variation can be attributed to sediment water interactions, but their role appears not to have changed over time.

Comment #26: Page 8-43, line 885 – says a low sedimentation coefficient indicates that the lake adsorbed less excess TP load from the watershed; does this mean it sequestered less P in the

sediments? Or is this uptake by phytoplankton? Please clarify. It has ranged from 0.16 to 1.10 over the years. Is there a target sedimentation coefficient for the lake that would be desirable to exceed?

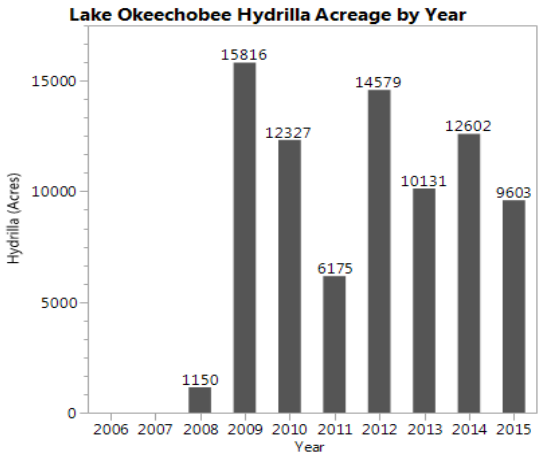
Response #26: The sedimentation coefficient is based on the estimated removal of total phosphorus from the water column to the sediments divided by the average water column mass in the year. Lower coefficients indicate smaller portions of the mass are removed. The total phosphorus budget for Lake Okeechobee estimates the removal of phosphorus from the water column by subtracting the net change in lake mass (estimated from the beginning of the water year to the beginning of the next water year) from net loads (atmospheric deposition + external loads – discharge from the lake). Note that TP analyses includes P in phytoplankton, bacteria, and organic and inorganic particles and does not differentiate among them. Thus, the net sedimentation is an estimate of the sum of settling, resuspension, diffusion, and adsorption to the sediments and the uptake by phytoplankton, and settling out cannot be distinguished based on this analysis. There is no target sedimentation coefficient other than a positive number (e.g., there is net removal from the water column to the sediments). Clarification language will be included in the final report.

Comment #27: Section LAKE OKEECHOBEE MONITORING RESULTS/SUBMERGED VEGETATION: Overall – very impressive data collection on SAV. Some of the signs (e.g. Potamogeton data) are encouraging, however.....

Response #27: The authors agree.

Comment #28: Page 8-49 – Hydrilla is one of the most problematic invasive macrophytes nationwide. Please present some data on its increase or decrease in this lake over a several year time period.

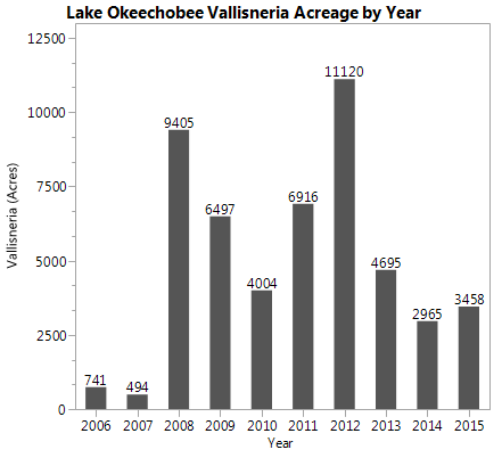
Response #28: Per the attached graph, it appears that during the recovery from the hurricane years of the mid-2000s hydrilla coverage increased rapidly, occupying nearly 16,000 acres of the nearshore zone in 2009. Since then, the coverage appears to have been relatively stable.



Interestingly, hydrilla has become an important resource in the lake due to its association with the exotic invasive apple snail (*Pomacea maculata*), which may be a key element in the recent nesting suceess of the federally endangered snail kite (*Rostrahamus sociabilis plumbeus*) on Lake Okeechobee.

Comment #29: The loss of Vallisneria coverage is troubling. Are there any hypotheses as to what is causing this decrease?

Response #29: Looking at the post mid-2000s hurricanes data for *Vallisneria*, it is not clear that there is a consistent decreasing trend in coverage. However, our field experience suggests that there are many locations where *Vallisneria* beds are being extirpated by expanding emergent and floating leaved vegetation, possibly as a result of the generally lower lake stages that have characterized recent years. It is expected that our next littoral zone aerial mapping effort, scheduled for this coming water year, will help to quantify some of the changes that have been observed in the field.



Comment #30: Page 8-57, line 1104-1106 – any speculation as to why cattail is aggressively expanding?

Response #30: There are several possible and non-mutually exclusive reasons that may explain why cattail is expanding: (1) treatment efforts have been relatively limited over the last several years; (2) generally, lower lake levels are creating more hydrologically suitable cattail habitat; (3) the hurricanes and associated elevated water levels of the mid-2000s pushed large volumes of nutrient-rich pelagic zone water back into portions of the previously pristine, rain-driven western marsh stimulating rapid expansion of cattail (note that this is the area where much of the cattail expansion has occurred in recent years); and (4) the relative infrequency of higher lake stages coupled with storm-generated wind and wave activity over the recent past. A combination of conditions appears to be capable of uprooting large areas of cattail along the nearshore-littoral edge when they occur. This information will be included in the final report.

Comment #31: Excellent photograph (Figure 8-20) of an alligator sunning itself among herbicide-sprayed cattail. Are the herbicides known to have any adverse effects to wildlife? Please address this question.

Response #31: All herbicides are applied according to label directions and none are classified as having direct adverse effects on wildlife. It is possible that herbicide application activities may have localized indirect temporary negative impacts on wildlife as a result of abrupt habitat shifts. However, many years of experience on Lake Okeechobee indicate that vegetation control activities produce overwhelming positive impacts on the ecosystem as a whole. The LOOP permit requires that both herbicides and pesticides are monitoring and reported in annual permit report. Therefore, this information will be reported in the final 2016 SFER – Volume III, Appendix 4-1.

Comment #32: Line 1112-1115 – Clearly burning the dead cattail is done to reduce BOD and SOD but it will release all the P in highly available form. It would be ideal, but probably impossible, to remove the dead biomass from the system.

Response #32: Yes, this is correct on both counts. The authors recognize that the physical removal of dead plant matter would be far better ecologically than burning (which in turn is better than allowing the vegetation to decay in place). However, the cost and logistics of attempting physical removal of large acreages of dead plant material in this sizeable lake, coupled with the difficulties of finding suitable nearby disposal areas where the material could be stockpiled in a way that would prevent it from returning nutrients to the lake via runoff, renders the process prohibitively

expensive. The last time this type of harvesting was attempted, costs were approximately $55,000 per acre, as compared to regular herbicide treatment followed by burning, which typically costs less than $100 per acre.

Comment #33: Please make a list of the exotic plant species threatening the lake environment (species and common name) in order of most problematic.

Response #33: Rooted Species: (1) torpedograss (*Panicum repens*); 2) exotic water grass (*Luziola subintegra*); (3) cattail (*Typha sp*.) – native species, nuisance status; (4) alligator weed (*Alternanthera philoxeroides*) – native species nuisance status; (5) melaleuca (*Melaleuca quinquenervia*); and (6) West Indian marsh grass (*Hymenachne amplexicaulis*).

Floating Species: (1) water lettuce (*Pistia stratiotes*) and (2) water hyacinth *(Eichhornia crassipes*). Further information on exotic plants can be found in Chapter 7 of this volume.

Comment #34: Line 1140 – it is encouraging to see the control program for torpedo grass working well.

Response #34: The authors agree, although there is a need to treat additional acres of torpedograss each year to achieve the desired lake-wide control levels. Please also refer to Chapter 7 of this volume on torpedo grass control program.

Comment #35: Figure 8-26 – please add a second bar next to each species bar that shows the estimated total acreage covered by each species, so the reader can compare acres treated with total acres covered.

Response #35: A complete aerial mapping of the marsh has not been done since 2011-2012 and, given the rapid changes that can occur in the areal coverage of emergent plant species over relatively short periods of time, there is not adequate data to develop the suggested graphic at this time although the authors agree that it would add much value to Figure 8-26.

Comment #36: It is good to see the microcystin sampling taking place. A couple of points – the levels are not extremely high but may be of concern. Dr. Dillon notes that the Canadian drinking water standard is 1 µg/L – what is the Florida and US EPA drinking water standard? Also, a colleague of Dr. Dillon's who works on microcystin told me that there is good evidence that fish reproduction is affected at 0.5 µg/L. Finally, there is now evidence that microcystin bioaccumulates in fish (in tropical lakes in Africa), sometimes to dangerous levels with respect to human consumption. It would be worth measuring levels in some sport and commercial fish species.

Response #36: Neither the USEPA nor the FDEP has an established drinking water standard for microcystin. The FDEP does have a contact recreation standard threshold (10 µg/L) and a non-contact recreation standard threshold (20 µg/L). With regard to human consumption of fish, this falls under the prevue of the Health Department.

Comment #37: Several biological indicators including fish seem to be showing improvement which is very positive. It would be instructive to note whether or not there were any fish kills in the lake environs during the water year.

Response #37: As noted above, because of the magnitude of the resource, the District does not routinely monitor for fish kills but only responds when potential kills are brought to our agency's attention by other agencies or stakeholders. As a consequence, we do not have comprehensive data on fish kills. However, based on anecdotal evidence, it does not appear that large kills are common.

This is not surprising since despite chronically low dissolved oxygen values in heavily vegetated portions of the littoral zone, most of the nearshore and pelagic zone tends to be well mixed and adequately oxygenated due to the lake's shallow depth and long fetch.

Comment #38: Page 8-77, line 1547 – it is good to see bird species other than wading birds included; gives a much more well-rounded ecosystem view.

Response #38: The authors agree, and this was the motivation for expanding our monitoring to include secretive marsh birds, particularly in light of the differences in habitat requirements between this group and wading birds.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 9

David Anderson, Steve Bousquin, Hongjun Chen,
Michael Cheek and Zach Welch

Level of Panel Review: Technical
Reviewers: P. Dillon (AA), S. Fennessy (A)

Hydrologic Conditions and Water Management

Comment #1: This section includes a great deal of data with much detailed discussion. This section could be improved in terms of readability. The writing is dense with long sentences and passive voice, making it difficult to follow in some sections, e.g. the sentences on lines 359-362, and 488-491. Editing for clarity and directness will help readers follow the discussion more readily.

Response #1: We will review the section in light of this comment and make changes as needed.

Comment #2: l. 427 - 429 – as is the case in other chapters and other years' reports, a mix of English and metric units is employed throughout which can be confusing to the reader who continually has to do conversions. It is good to at least have an explanation for why the authors use non-SI units in the report. However, it would be even more useful if the conversions were done. Even the USACE must use metric units for parameters such as concentration (i.e. mg/L) so it should not be an impossible jump.

Response #2: The District is considering changing its conventions for units and will likely develop new guidance for authors for next year.

Comment #3: l. 468 – the number of significant figures used in several places in the chapter is unwarranted. It is unlikely that rainfall measurements can or were made to the hundredth of an inch.

Response #3: Number of significant digits was reduced to tenths of an inch for the one value that was inconsistent to match the rest of the paragraph. The final report will be revised accordingly.

Comment #4: l. 510 – are these recommendations from the district for management in the year that has just passed? Not clear how these fit.

Response #4: Yes, for the wet and dry seasons of WY2015, which ended April 30 2015. A brief new paragraph was added to explain that the first subsection lists water management recommendations for wet season 2014 from the District's KRREP for the Kissimmee River, and that wet season lake requests from other stakeholder agencies are in the second subsection titled "FWS/FWC Requests for Wet Season 2014". These sections are followed by a discussion of outcomes for the wet season. Recommendations, requests, and outcomes for dry season follow in a separate section. The final report will be revised accordingly.

Comment #5: l. 530 - the suggestion here about alternating years in which the lake or river objectives are met is a good one, particularly given that the hydro-period targets have not been met for the Kissimmee since 2001 (line 508). Alternating management priorities could provide data, and perhaps more insight, into the trade–offs that are made by these management decisions.

This could help with the tension that exists between conflicting management goals and perhaps improve the hydrological conditions for the river-floodplain.

Response #5: That is an interesting observation but we tend to disagree. It was recommended as a temporary stopgap measure to address lack of consensus. However, over the long term such an approach would be unlikely to result in meeting river targets if the frequency of favorable climatic conditions is the same as the target frequency. For example, if a target needs to occur in a certain percentage of years, alternating to a conflicting goal in alternate years would result in a miss that disrupts the frequency of occurrence for the river.

Comment #6: l. 604, 657 – were any fish kills noted as a result of this hypoxia?

Response #6: None was observed, so it was not mentioned in the chapter. Fish kills however are notoriously difficult to detect, so nothing can be inferred from a failure to detect a fish kill.

Comment #7: This entire section on hydrology provides very detailed information and rational explanations for the observed data. The question of how climate change will affect the hydrology of this basin in the long term is critical to future planning but is not addressed in this chapter. Is there information/discussion of this in other documentation? At present, the approach seems to be based on short-term considerations only, I think with the implicit hope that these will be valid in the long term. A lot of money could be spent on system alterations that may not be the right ones in 10 or 20 years. Hopefully, there is considerable thought being given to this broader picture.

Response #7: KRREP scientists are contributing to a separate District effort to consider potential effects of climate change on restoration.

KRREP

Comment #1: 884 – adding data on temporal trends for a few key variables or indicators would provide useful information on the restoration progress being made, as many other sections of the report do. For example, Table 9-4 could be altered to present data for each of the years shown, instead of listing the beginning page numbers where these data can be found in earlier reports. Clearly this couldn't be done for all the variables presented here, but presenting time-series data for a few would help give a sense of progress.

Response #1: Time series data are typically part of the KRREP SFER presentations (e.g., hydrology, wading birds, waterfowl, water quality, dissolved oxygen, river channel vegetation, floodplain vegetation). The intent of Table 9-4 is to provide the reader with a brief index on where to find the most recent analyses of KRREP expectations during the interim period. A table is not the suitable format for presentation of data analyses or results, which requires more than just statements of a value (e.g., standard errors, units, and discussions of methodology and interpretation). Most expectations are also associated with more than one metric or component, adding additional complexity and length to the chapter. Including data in the table would be redundant since the information is already presented in the previous chapters, along with appropriate levels of discussion of the methods used, graphs and tables, and the author's interpretation of results. The purpose of the table is to provide the specific locations of expectation evaluations in previous SFER chapters for readers' and reviewers' convenience.

Hydrology

Comment #1: The proposed changes to several of the hydrology metrics, which are described starting on page 9-31, are an important modification and the rationale for the change needs to be should be explained more clearly. As it is now, the paragraph starting on line 928 implies that the current criteria and metrics were not stated in operational terms (line 934), which is not really the case. Instead, the ecological and management basis for the changes should be the focus (such as

the information given on page 9-32), making clear that the metrics were revised based on the increase in available data and (perhaps) a refinement of the district's understanding of the dynamics of the system.

Response #1: Text has been revised to clarify that the operational difficulties were with the statement of the original hydrologic criteria and to describe the general basis for making changes to the expectations. The specific changes are described in more detail later in the section.

Comment #2: That said, the new BLM hydro-period metric seems relaxed; in several of the reference years the floodplain was inundated for over 210 days total, and was just a few days shy of 210 days in the August –February window. If these are counted (and ecologically they would be very similar in their characteristics) that would give a total of 41% of water years.

Response #2: Our interpretation of the comment is that the reviewer feels the target should be more conservative (should err on the side of caution).We agree that floodplain metrics should be conservative and appreciate this insightful comment. We will incorporate some of the reviewer's suggestions in the revision.

Comment #3: l. 1044 - isn't clear, do increases in depth indicate new recession events? In other places the text says that recession is counted as days where water levels are dropping.

Response #3: The text has been revised to clarify that an increase in depth of 1.5 ft is regarded as disrupting a recession event and therefore marks the end of one event and the beginning of a new recession event.

Comment #4: Overall this section presents a sound analysis of the challenges facing floodplain restoration with lots of relevant data and recommendations for improvements.

Response #4: Thank you for your helpful comments.

Phosphorus

Comment #l: 1423 - 1428 - there are errors in the punctuation of this (very long) sentence.

Response #1: We have deleted "during the post-Phase I period in comparison to the pre-construction, Baseline period". It reads "Phosphorus loads and concentrations at C-38 structures S-65, S-65A, S-65C, S-65D, and S-65E presented in the South Florida Environmental Report since 2005 have indicated that total P (TP) loads and concentrations were higher during the interim period than in the baseline period, with most of the structures releasing significantly more P during the interim period than in the baseline period (Jones et al., 2015)."

Comment #2: 1439 – the potential P load reduction of 17.75 mt would make a strong contribution to the needed decrease; can you put any error estimates on that figure?

Response #2: No error estimates were provided in the original figure from Jones et al. 2015.

Wading Bird Abundance

Comment #1: As predicted, there has been a strong positive response of the avian community to the Kissimmee River restoration. The trends in the data shown in Figures 9-24 and 9-25 are helpful in gauging the success, and would benefit from an explanation of the low numbers seen in 2007-2009 (also in figures 9-26 and 27). The comments on line 1509 state that water depths greater than 1.3 feet are too deep for foraging. With the hydrologic criterion for inundation depths greater than 1 foot, this is a narrow target for water depths. Is the goal to maintain depths between 1.0 and 1.3' for wading birds, with deeper areas for other species of waterfowl? Have floodplain zones that

differ in depths been established to hit these targets? Figure 9-26 shows the expectation line of > 3.9 ducks/km2 but the line is drawn at a value of about 9.

Response #1: Text was added attributing the low bird numbers during 2007-2009 to the drought of 2006-2007, when the floodplain was dry for nearly an entire year; thus no aquatic prey were available to foraging birds. An explanation was added stating that a depth of 1.0' in the broadleaf marsh habitat does not indicate an overall mean floodplain depth of 1.0', but rather a depth of 1.0' on average at the broadleaf marsh monitoring stations, which are significantly deeper than wet prairie habitat, the second most dominant vegetation type historically (17% vs. 50% for broadleaf marsh). Thus, when broadleaf marsh habitat is at 1.0'depth, wet prairie habitat is approximately 0.25' deep and suitable for foraging by most species. We will change the duck expectation line on the graph, just a typo.

Invasive Plants

Comment #1: Is there any concern about or documentation of the spread of (for example) Limpograss as a result of the test plots that were established (line 1701). Control of all four species might be tested for responsiveness to herbicide treatments; as it stands now only Limpograss and Peruvian Primrose-willow are subject to tests. This may be a funding issue, but should be clarified.

Response #1: The reviewer's question suggests that the limpograss was introduced for the test plots? Either way, a sentence was added explaining that the exotic grasses are not currently being mapped separately from other vegetation for documentation of coverage/expansion. Yes, the reviewer's comment is correct, this is a funding issue (i.e. there is no funding currently dedicated to treating exotic grasses on the river unless they are obstructing navigation (which they are not)). The purpose of this section was specifically to elevate the level of concern for the expansion of exotic grasses in the floodplain and to emphasize the urgent need for a management response before the problem grows larger.

KCOL and KUB Monitoring and Assessment

Comment #1: The presence of cyanobacteria in the KCOL is not surprising given the degree of eutrophication of the system, but the authors should be clear that while cyanobacteria are common in water bodies, significant levels of the toxins that some species can produce are less common (e.g., microcystin). It would help to distinguish the presence of cyanobacteria from the toxin they produce, particularly if this will be the basis for decisions to control hydrilla.

Response #1: Researchers are currently working to find a way to rapidly detect the toxin presence versus the cyanobacteria, though it's a novel toxin and they have yet to even isolate it or completely identify it, let alone develop a way to distinguish it. The following sentence was added for clarification. "Currently, researchers are developing a method to detect the presence of the neurotoxin itself, which would be a better indicator of the potential for AVM than whether or not the cyanobacteria is present."

Comment #2: 2028 – please provide some details on the monitoring stations that will be established such as the size of the plots and what aspects of the vegetation will be monitored.

Response #2: The following details were added; "Plant percent cover will be monitored in 5 m2 quadrats", and "Plant percent cover will be monitored in two, 1x2 m quadrats set perpendicular to either side of the transect at 0.5 ft (15 cm) elevation intervals."

Comment #3: Also, line 2046 refers to 'vegetation conditions - what does this mean?

Response #3: "Conditions" will be deleted in the final report.

Comment #4: 2059 – are all CPUE data collected by electro-fishing or are there any creel data measured? If there are, how do they compare?

Response #4: The values reported are only from electrofishing. Creel data were not included due to space limitations.

Comment #5: 2097-2122 – are there more specific causes of the decreases in the proportion of snail kite nests in the KCOL that can be added to this discussion?

Response #5: There are no specific causes and the researchers did not want to speculate as to potential reasons without supporting data.

Comment #6: 2137 – alligator data are interesting; don't remember seeing them in earlier reports. These are estimates and definitely should not be reported to 5 significant figures; 2 is probably valid, possibly 3.

Response #6: Population analyses produce an estimate, which is reported as produced. The range of error in the estimate is reported in the figures. We did at least round the percentages up instead of reporting a tenth of a percent.

RESPONSES TO COMMENTS ON THE DRAFT 2016 SFER – VOLUME I, CHAPTER 10

Fawen Zheng, Peter Doering, Lesley Bertolotti and Zhiqiang Chen

Level of Panel Review: Accountability
Reviewers: Dr. Michael A. Mallin (AA)

Comment #1: ***Overall Impression:***
Chapter 10 is information-rich and contains detailed background information on the watersheds in question, as well as the TMDL processes that are ongoing. The information is presented in a logical manner. It is very readable and generally clear information is presented on the many efforts that are either ongoing or planned to reduce nutrient loads to these two important estuaries. The water quality overviews, however, could use some enhanced explanations and presentations of results, and linking to some of the TMDL goals could be improved.

Response #1: We have compared observed water quality to existing TMDL targets (TN for the Caloosahatchee and TN and TP for the St. Lucie) and Numeric Nutrient Criteria for chlorophyll *a* (Subsection 62-302.532, F.A.C.), which will be added to the final text. In the next planned three-year evaluation (2018 SFER), it is anticipated that a more detailed analysis will be presented.

SUMMARY

Comment #2: Many readers will only read the Summary. Thus, looking at the Summary from an outsider perspective – a few things require a bit more explanation:

There are generally positive results in the Summary regarding lowered nutrient inputs for the water year. This should translate into responses from the biota, as such, regarding Page 10-3 – line 107-109 – would be good to mention if there were any large algal blooms, or any fish kills.

Response #2: There was no large algal blooms in WY2015 in either estuary. The District does not routinely monitor for fish kills in estuaries. The data we do have is largely anecdotal and of little value in assessing the real extent of fish kills.

Comment #3: Page 10-4, line 124 – mentions the positive news that larval spat supply continues to support natural recovery of eastern oyster please mention what they are recovering from.

Response #3: They were recovering from severe mortality in wet season of WY2014. The text in the final report will be revised accordingly for this clarification.

Comment #4: Page 10-4 - Lines 141-146 – this paragraph discusses meeting the average salinity goal being met. Please re-iterate here that salinity is controlled, at least in part, by agency releases of freshwater from Okeechobee.

Response #4: Yes, increased freshwater released from the lake in the dry season coupled with reduced runoff in the wet season lead to a relatively small difference between wet and dry season salinities. This clarification will be added to text in the final report.

Comment #5: Page 10-4, lines 157-159 – generally low chlorophyll levels – mention that there were no problematic algal blooms.

Response #5: This clarification will be added to text in the final report.

Comment #6: Page 10-4 –lines 164-165 – mention what the term reference concentration means.

Response #6: In the final report, the phrase will be changed to "TMDL reference concentrations for TN," and "(for detail about this reference concentrations, see the *CRE Hydrology, Water Quality and Aquatic Habitat, Methodology* sub-section of this chapter)" will be added to the text.

Comment #7: Lines 166-167 – excellent news about re-appearance of Vallisneria.

Response #7: We concur, thank you.

INTRODUCTION

Comment #8: Concise and well-written

Response #8: Thank you.

BACKGROUND

Comment #9: Well-written, excellent information is presented on the history of these systems. I had no idea that serious human re-engineering of the natural water systems had begun as far back as the 1880's.

Nicely presented maps as well.

Response #9: Thank you.

Comment #10: Page 10-11 – top paragraph – it is noted that in the SL watershed there is 54% agriculture land coverage and in the CA watershed 35% agriculture coverage. Please discuss what types of agriculture dominates each, because it makes a difference in term of N or P runoff amounts. Also regarding livestock, I assume cattle dominates, but is there significant poultry production here as well? Both of those sources will produce manure with an N:P ration of around 3, whereas for crop agriculture the N:P in fertilizer is much higher.

Response #10: A detailed breakdown of land use types by sub-watershed was provided in the 2012 River Watershed Protection Plans (SFWMD et al., 2012a and 2012b). The agricultural land uses in the Caloosahatchee Watershed are dominated by improved pasture (10.8%), citrus (8.9%), and sugar cane (8.5%). Dominant agricultural land uses in the St. Lucie Watershed are citrus (22%) and

improved pasture (20%). Poultry production is not significant in either watershed. More details on the dominant agricultural land uses in each watershed will be included in the final chapter.

WATERSHED EFFORTS

Comment #11: Table 10-2 presents an exciting array of projects that are ongoing or planned. I am impressed with the breadth of the projects and see that the state is taking rehabilitation of the north Everglades area and improvement of the estuaries seriously.

Response #11: Thank you.

Comment #12: Unfortunately the 10 Mile Creek project did not perform as planned. Please add the reasons why the state feels this project did not work. Such information would be instructive to outside researchers as well as project stakeholders.

Response #12: Field observation of excessive seepage and piping indicators led to concerns about the stability of the reservoir embankment and its ability to hold design water levels safely. In the interim, the SFWMD has received approval from USACE to use the facility for low level storage during the 2015 wet season.

Comment #13: The FAVT project seems to be exciting and innovative research. I hope water column nutrients are being sampled directly at the project site(s) so the amount of N and P that may be escaping from the buried material into the WC can be ascertained.

Response #13: Thank you, we will forward this suggestion to FDACS, our partnering agency that is the lead of this project.

WATERSHED RESEARCH AND WATER QUALITY MONITORING

Comment #14: Overall an impressive amount of hydrological and water quality monitoring is taking place in both of these watersheds. The rainfall, discharge, and TN and TP loading are well presented in the series of figures and tables.

Response #14: Thank you.

SLE HYDROLOGY, WATER QUALITY AND AQUATIC HABITAT

Comment #15: Page 10-32 – lines 766-768 - please indicate that these are molar N:P ratios (if they are).

Response #15: It is mass ratio of TN/TP loadings and the text in the final report will be revised accordingly for this clarification.

Comment #16: I believe that we can safely round the Okeechobee watershed TN:TP ratio from 9.79 up to 9.8, and the SLE watershed TN:TP ratio from 4.98 to 5.0.

Response #16: Agreed, the text in the final report will be revised accordingly.

Comment #17: Page 10-34 – first paragraph – I found this lead paragraph to be quite vague and unhelpful.

Response #17: Agreed, the text in the final report will be revised as follows:

"Chl-*a* is frequently used as an indicator of estuarine water quality in many estuaries. Relationships between freshwater inflow and Chl-*a* are often hard to discern in small sub-tropical estuaries like the SLE (Buzzelli, 2011) because other physical (transport, sinking), chemical (nutrient supply and turnover), and biological (grazing by zooplankton and larval organisms) factors (Lucas et al., 2009; Phlips et al., 2011) cause fluctuations in phytoplankton growth and biomass."

Comment #18: Page 10-34 – second paragraph, also Table 10-6. The authors need to put the chlorophyll a data into some kind of perspective. According to the standards, if I am reading them correctly, the chlorophyll a standard for the SLE tribs ranges from 5.0 – 7.4, and for the Caloosahatchee system it ranges from 4.2 – 5.6 ppb. So the reader can get some kind of perspective on what the recent levels mean, please add discussion of how the data stack up against the standards.

Response #18: Agreed, these chlorophyll *a* standards from Subsection 62-302.532, F.A.C., will be included in the final chapter. The perspective by comparing with these standards also will be included in the final chapter.

Comment #19: Figure 10-14 presents a near 20-year perspective on WQ data. There are several large peaks in chlorophyll a that jump out. I know the 2004-2005 blooms were related to the hurricanes and Lake Okeechobee releases. It would be instructive to add in the narrative (in a sentence or three) what caused the large blooms in the past.

Response #19: This kind of analysis is not straightforward. More comprehensive data need to be collected and advanced analyses of hydrological, water quality and ecological data would be needed. We may consider the analysis of the large bloom events in next year's analysis.

Comment #20: Page 10-34 – lower two paragraphs. There is much mention of TN and TP in relation to freshwater inflow. Is there a statistical correlation between discharge and nutrient loads for the tributaries?

Response #20: There is statistical correlation between annual total nutrient loading and total annual freshwater inflow conducted in the St. Lucie River Watershed Protection Plan (SFWMD et al., 2009) (Appendix E, St. Lucie River Watershed Research and Water Quality Monitoring Program). The correlation analysis was conducted based on data from the 10-year period of record (1995–2005). Annual TN loading ~ Annual flow: $y=0.0041x - 979.82$ with $R^2=0.81$; Annual TP loading ~ Annual flow: $y=0.0006x - 113.02$ with $R^2=0.81$; where x is total annual flow (ac-ft) and y is total annual TN or TP loading (metric tons).

Comment #21: Figure 10-14. Need to designate in the caption which lines/colors designate inflow and which designate nutrient and chlorophyll concentrations.

Response #21: Comment appreciated. A complete description of legends will be added in the figure caption in the final report.

Comment #22: There are red lines designating the annual TMDL TN and TP reference levels. Please add red lines for chlorophyll standards (or a band) as well, so the reader has a reference for this response variable.

Response #22: We agree. A red line representing chlorophyll standards will be shown in the final chapter.

Comment #23: Page 10-37 – excellent news regarding seagrass coverage!

Response #23: We concur and thank you.

Comment #24: Page 10-40 – Significant Findings – again the chlorophyll bullet essentially says nothing – please place in perspective of standards exceedences (if any) so the reader has some benchmark.

Response #24: We agree. This will be included in the final chapter.

CRE HYDROLOGY, WATER QUALITY AND AQUATIC HABITAT

Comment #25: Page 10-42 top – there is truly impressive coverage of this watershed in terms of water quality monitoring!

Response #25: Thank you, we concur.

Comment #26: Page 10-42 – line 11-83 – states "water column properties were determined..." besides nutrients and chlorophyll, what other WQ parameters are monitored? DO? Turbidity? Others?

Response #26: The District routinely collects other water quality parameters at various monitoring stations in the CRE including DO, Secchi disk depth, turbidity, color, etc. Data on these supplemental water quality parameters are available as supporting information but not specifically reported in this year's chapter, which is focused on the core key water quality parameters (i.e., TN, TP, Chl-*a*, salinity) consistent with the annual CRWPP update goals and objectives. The DO analysis will be considered in the future version (see more about DO analysis in the response to comment #37).

Comment #27: Again – much interesting hydrological and loading data is presented within this section.

Response #27: Thank you.

Comment #28: Page 10-49 – lines 1442 – 1449 – notes that over the past 3 years the FWM TN concentration shave shown a decreasing trend. That is excellent news for sure. Please explain (or even speculate) on the reasons for this decrease. It it just flow-driven or are nutrient controls having a positive impact?

Response #28: Looking at the flow variation shown in Figure 10-23, it suggests that the reduction in FWM TN over the past three years was not flow-driven. The trend observed over this three-year period is short term rather than long term, but it is not known what factor(s) specifically caused this decrease.

Comment #29: Page 10-51 – middle paragraph – as with the SLE, please give some perspective on chlorophyll levels in relation to standards here.

Response #29: We agree. Similar to the SLE, some perspective for the CRE will be included in the final chapter.

Comment #30: Page 10-52 – Figure 10-25. As with Figure 10-14, need to clarify in the caption what lines represent what parameter; also please add chlorophyll standard designations.

Response #30: Comment appreciated. A complete description of legends will be added in the figure caption in the final report.

Comment #31: Page 10-53 – first line – notes that TN concentration of the CRE is proportional to variations in freshwater inputs. Are these relationships statistically significant?

Response #31: The relationship between TN concentration and freshwater inflow was established based on the statistical analysis conducted by Doering and Chamberlin (1999) and nutrient budget results in Buzzelli et al. (2013b). The statistical analysis by Doering and Chamberlin (1999) indicated it was statistically significant. Doering and Chamberlin (1999) will be added to the final chapter.

Comment #32: Page 10-54 – top paragraph regarding shifting of species dominance of seagrass. It might be useful to note in a concluding sentence if coverage here is sufficient in relation to targets.

Response #32: There is a target for density coverage but no target for species occurrence. We will clarify this in the final report.

Comment #33: Page 10-54 – lines 1580-1586 – seems to be excellent news on oyster density returning!

Response #33: Thank you. We concur.

Comment #34: Page 10-54 – lines 1587 – 1599 – are there any targets for oyster recruitment in the estuary? If so, it would be worth discussing the present results in relation to desired targets.

Response #34: Currently, there is no oyster recruitment target established for the estuary. There is an oyster habitat index model under development which will be used to estimate acreage of suitable habitat in the future. We will clarify this information in the final text.

Comment #35: Page 10-58 – Significant findings for the CRE:

Lines 1744 – 1750 – please place the chlorophyll results in perspective of the estuary-specific standards so the reader has a perspective from which to understand this response variable.

Response #35: We agree. Similar to the SLE, the perspective for the CRE will be included in the final chapter.

Comment #36: Lines 1757 – 1763 – it would be worth mentioning the amount of total seagrass coverage in the estuary, and how that relates to any target coverage levels.

Response #36: Most recently in 2014, we have taken aerial photographs of the seagrass in San Carlos Bay, Pine Island Sound, and Matlacha Pass and the associated map is under development. In the Tidal Caloosahatchee (upstream of Shell Point), the water is too dark for photos to be taken. Once available, it is anticipated that this information will be compared to the targets developed by the Charlotte Harbor National Estuary Program in next three-year update.

Comment #37: In general, a well-written, helpful document that the authors have compiled. Many of my review comments are designed to improve the reader's understanding of the linkage between findings and goals, and between nutrient loading and chlorophyll a responses.

Lastly, are there any dissolved oxygen issues in either estuary? Since it was mentioned within that DO conditions can impact oyster survival, this might be a proper topic to explore in future versions of this chapter.

Response #37: We agree. The vast majority of our DO data are from synoptic profiles in the Caloosahatchee River Estuary, which can be heavily biased by the time of day at which they are collected. We have continuous DO data from one station in the St. Lucie Estuary. It is anticipated that a detailed analysis of DO will be included in the next three-year update (2018 SFER).

References

Doering, P. H., and R.H. Chamberlin. 1999. Water quality and the source of freshwater discharge to the Caloosahatchee Estuary, FL., Water Resources Bulletin 35:793-806.

SFWMD, FDEP and FDACS. 2009. St. Lucie River Watershed Protection Plan. South Florida Water Management District, West Palm Beach, FL; Florida Department of Environmental Protection, Tallahassee, FL; and Florida Department of Agriculture and Consumer Services, Tallahassee, FL. January 2009. Available online at http://www.sfwmd.gov/northerneverglades.

SFWMD, FDEP and FDACS. 2012a Appendix 10-1: St. Lucie River Watershed Protection Plan 2012 Update. In: *2012 South Florida Environmental Report – Volume 1*, South Florida Water Management District, West Palm Beach, FL; Florida Department of Environmental Protection, Tallahassee, FL; and Florida Department of Agriculture and Consumwer Services, Tallahassee, FL.

SFWMD, FDEP and FDACS. 2012b. Appendix 10-2: Caloosahatchee River Watershed Protection Plan 2012 Update. In: *2012 South Florida Environmental Report – Volume 1*, South Florida Water Management District, West Palm Beach, FL; Florida Department of Environmental Protection, Tallahassee, FL; and Florida Department of Agriculture and Consumer Services, Tallahassee, FL.