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

During September–November 2013, the peer-review panel posted their comments on draft Volume I on the 2014 SFER WebBoard at www.sfwmd.gov/webboards; public comments were also received on Chapter 8 (see Appendix 1-2). This appendix includes authors' responses to panel and public comments. With the exception of spell check and reformatting some information for better readability, this appendix was not edited by the SFER production staff and appears verbatim as posted on the WebBoard.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3A

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

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

WebBoard-Posted: 10/23/12

General Response: We appreciate the comprehensive peer review provided by the reviewer of this chapter, as always the review improves and adds to the quality of this chapter. The goal of the authors is to continuously improve this chapter adding valuable useable information that inform environmental policy and decision making.

Comment #1: *Page 3A-11 presents the list of parameter's that are generally monitored by the district. It was stated that metals may not be a problem and were not analyzed in the WY 2013. To make the argument stronger, hardness could be added to the list of analyzed parameters because the water quality criteria for key metals (excluding mercury which is extensively covered in Chapter 3B) and their toxicity are related to hardness. If the waters of the Everglades are soft (they seem not to be) than even small concentrations of toxic metals could represent the problem.*

Response #1: Noted, Thank you for this comment and suggestion. The authors will take this into advisement for next year's South Florida Environmental Report (SFER) and consider reporting hardness concentrations (if possible). However metals have been monitored for many years in EPA and their concentrations are generally below the hardness based water quality criteria due to relatively the high hardness concentrations. It has been concluded that metals met the criteria of Class III standards based on historical monitoring data. As a result, metals were not analyzed in WY 2013 and they are not expected to be analyzed in the future.

Comment #2: *Page 3A-14 discusses and presents the effects of Hurricane Isaac. Generally, hurricanes are disruptive and upset the chemical balance of the system, e.g., by stirring sediments and releasing some pollutants. Table 3A-1shows some elevated values of some parameters at the diversion structures but the subsequent analyses of long term trends did not reveal that the system was upset and needed a long recovery as it happened with some previous hurricane events.*

Response #2: Noted, Thank you. The scale of disruptive impacts is related to hurricane strength and continuation of the disruptions. Apparently the system was less impacted by Hurricane Isaac than previous events which consisted of a series of continuous strong hurricanes (2004 and 2005).

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

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Response #3: Noted, the use of acre-ft is standard practice for the South Florida Water Management District (SFWMD) and the SFER. Units of measure with conversion factors from metric units to U.S. equivalent units are provided in the preface of the SFER.

Comment #4: *Pages 3A15-17 present tabular summaries of water quality parameter excursions for four phases, starting with 1974. Generally, the number of excursions is going down for pH, ammonia, specific conductance and alkalinity. This sounds positive but there are some underlying issues. The biggest problem is DO that is judged differently for inflows and outflows where Florida Class III standard is applied (minimum DO 5 mg/L) to inflows/outflows while interiors of the management areas are judged according to a site specific harmonic function related to the time of year and temperature. This led to an obvious paradoxical dichotomy which was addressed by this reviewer at least twice in the previous reviews. In the past years it was agreed, however, that the authors of the chapter are obliged to use the standing legislature approved criteria for the assessment and extensive discussions on adequacy or reality of the standing model that was accepted by the authorities seemed to be pointless. But the dichotomy became obvious again as presented below.*

The evaluations and statistics of the number of excursions presented in Table 3A-2 on DO excursions revealed miserable compliance with the Class III standards at the points where it was applied, i.e., inflow and outflow monitoring points. At these points the DO standard was violated up to 88% of the time and the table indicated significant worsening of the frequency of violations between the baseline Phase I and WY 2013. On the other hand, the harmonic criterion for the interiors was almost never violated but there is no reason for celebration. It was pointed out in the previous reviews that the harmonic criterion is extremely lenient and allows DO to drop to very low levels approaching lethal levels (there have been periodic fish kills reported by the media in the past). Theoretically, the water quality DO concentrations in the outflow from the area are the same as those in the interior. So, until it is recognized that the present criteria are either not applicable to the Everglades (Class III criteria) or are so lenient that they do not protect much (criteria for interior areas), it is indeed pointless to make any judgment on DO conditions except report the situation and continue to analyze trends as it was done adequately and professionally in this chapter. As in the past, this comment on the problem with the DO assessment is not aimed at the authors; it is directed towards the agencies that developed and/or accepted the harmonic formula and its application to averaged DO concentrations and now may be forcing the authors and the District to use Class III criteria for a marsh system that is naturally dystrophic. Without solving the inappropriate standards problem, DO assessments and abatement programs will go nowhere. The situation may be helped by the fact the low DOs are linked to eutrophication which is controlled by the TP standard. However, the TP standard is mostly met throughout the WCA 2, WCA 3 and ENP, yet DO problems persist and appear to be getting worse in some areas.

Response #4: It is widely understood that wetland ecosystems generally have a high demand for oxygen and oxygen concentrations can fluctuate widely during the diel cycle; therefore wetlands typically exhibit low dissolved oxygen (DO) concentrations. As stated in the chapter, DO is not a direct pollutant; instead it is a secondary response parameter that reflects changes in other pollutants as well as changes in the ecosystem. With the construction and operation of the existing stormwater treatment areas (STAs; i.e. STA-1W, 1E, 2, 3/4 and 5/6) phosphorus concentrations have decreased significantly. Furthermore, as part of the State of Florida's Restoration Strategies, additional STA acreage as well as supporting infrastructure will be constructed in the coming years to aid and boost the current performance of STAs. Therefore phosphorus loads and concentrations entering the EPA are expected to decrease further. It should be noted that the DO site-specific criterion (DO criterion for the internal marsh stations) is also used to gauge STA compliance using established transects within the marsh. Please see Chapter 5B of this volume for more information related to performance of the Everglades STAs.

It should be noted that at the time of analysis and writing this chapter Florida's DO criteria for Class III (freshwaters) waters other than the interior marsh of the EPA stated that DO concentrations "Shall not be less than 5.0 mg/L" (62-302.530(30) F.A.C.) Additionally, the criteria indicated that normal daily and seasonal fluctuations above these levels shall be maintained. It is important to note that the criterion applies to all places and at all times in Class III waters. This criterion was established in 1979, largely based on early water quality criteria recommendations from the Federal Water Pollution Control Administration and United States Environmental Protection Agency (USEPA). This initial guidance concerning the establishment of appropriate DO criteria was based on very limited scientific information.

As of September 9th 2013, the USEPA has reviewed and approved a revision to the statewide marine and freshwater DO criterion that takes into account location (bio-region) and time of day. Additionally the revised criterion is based on percent saturation instead of a fixed concentration of dissolved oxygen. Therefore in next year's SFER, the revised criteria will be applied appropriately. Meanwhile the DO Site Specific Alternate Criteria will remain the same for the marsh monitoring locations within the EPA.

Based on the analysis of this chapter, the current DO criterion (at the time of analysis) of 5 mg/L for discharge structures is not suitable for warm surface water and naturally generated dissolved organic carbon in South Florida. It should be noted that no fish kills were observed during this period of 70 – 80 percent excursion. It is anticipated that the new DO criterion will better reflect the DO conditions in South Florida and the Restoration Strategies-related projects will likely reduce recalcitrant organic matter and improve DO concentrations at STA discharge structures in the future.

Comment #5: *Page 3B-19 line 477.* $CaCO_3$ *is not alkalinity. Alkalinity is a sum of OH-,* HCO_3*- and* CO_3 *= ions but it is often expressed as* $CaCO_3$ *equivalent. At pH levels typical for the Florida Everglades and other waters,* CO_3 *= and OH- components may be negligible and alkalinity is mostly bicarbonate. The reviewer agrees that occasional low pH and alkalinity are mostly natural. If the inflow to the refuge interior is dominated by atmospheric inputs, it is obvious that the refuge and EPA water will have naturally low pH and alkalinity. Typically, natural unpolluted rainfall has pH around 5.7 and has very low alkalinity. If worries about the excursions of the two (interrelated) parameters persist, a Use Attainability Analysis would most likely fix the problem and adjust the standards or identify an anthropogenic cause of the excursion that could be remedied by a TMDL.*

Response #5: Noted, $CaCO_3$ is not the same as alkalinity. However, the method used to measure alkalinity normalizes the results and expresses them as a concentration of $CaCO_3$. The text will be added/revised to clarify.

Historically, low alkalinity "soft" water was found throughout much of the EPA since the system was predominately rainfall driven. However, with the construction and operation of the regional canal system and subsequent input of large volumes of groundwater, many portions of the systems have shifted from soft water systems to hard water systems. Today, the natural soft water low alkalinity conditions are essentially confined to the Refuge because the interior of the Refuge remains largely hydrologically isolated from the canal inflows. This natural condition in the Refuge has been well documented in past versions of this chapter. Development of Site Specific Alternative Criteria (SSAC) for alkalinity and pH in the Refuge that formally acknowledge the natural conditions have been considered in the past, but, have not been a priority.

Comment #6: *Page 3A-20 – Problems with specific conductance. The freshwater groundwater zone in Florida is known to be relatively thin and has been depleted by anthropogenic overdrafts causing intrusion of salt water into groundwater zone drainage canals. This has not been addressed; however, it appears that the situation is improving and the excursions are decreasing.*

Response #6: Noted, Thank you. Surface water "salinization" via groundwater fluxes were not discussed in detail since the data seems to indicate that "salinization" is occurring at very low rates or not occurring at all. However a portion of text does discuss the ability of very large pump stations to draw high conductivity groundwater. Operational triggers and cut-offs are established to help ensure that this doesn't occur. These triggers were developed because of the potential of salt water intrusion if water is over-drawn from the surficial aquifer. Salt water intrusion has been considered in our project planning processes. Based on the analysis presented in the chapter, it is apparent that it is not an issue because the situation is improving and excursions are decreasing. It is generally understood that if water is drawn from deep reservoirs it could influence the specific conductance excursion rate for a given area; therefore these activities have been closely monitored and evaluated.

Comment #7: *Pages 3A-25to 3A-35 evaluated phosphorus. Phosphorus has been recognized as a limiting nutrient for the Everglades (EPA). To protect the biota and limit potential eutrophication a criterion of 10 µg/L of Total Phosphorus was designated for the areas. Phosphorus drives the eutrophication processes in the Everglades (as well as in Lake Okeechobee and other important water bodies) and is also linked to DO. The most important outcome of this chapter is continuing evidence that the phosphorus concentrations throughout the Everglades system water bodies are decreasing and the geometric means of concentrations are at or below the relatively stringent standard of 10 µg/L.*

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

Response #7: Noted, Thank you. It should be noted that the 10 µg/L limit is assessed within the marsh at fixed monitoring locations and is part of a larger water quality criterion that used four separate criteria to determine compliance with the water quality standard. Please see Appendix 3A-6 for the total phosphorus compliance calculations for the EPA. Weather seems to be a random factor which does not affect the long term improvement trend.

While the current STAs are fully operational, it is recognized that additional water quality improvements are necessary to achieve the Everglades total phosphorus criterion, which is the rationale for the development of the Restoration Strategies (discussed in detail within Chapter 5 of this volume). Restoration Strategies involves the construction of flow equalization basins and increased STA acreage in an effort to reduce phosphorus concentrations in STA discharges to the EPA. Restoration Strategies also includes additional source controls – where pollution is reduced at the source – in areas of the eastern Everglades Agricultural Area where phosphorus levels in stormwater runoff have been historically higher. In addition, a robust science plan will ensure continued research and monitoring to improve and optimize the performance of STAs.

Furthermore BMPs are constantly being developed and improved upon in an effort to protect and/or reduce impacts to natural areas.

Comment #8: *Figure 3A-8 shows that Total P standard of 10 µg/L for annual geometric means within the Everglades areas is generally met in the EPA Park and WCA-2 and WCA-3 and very close to being met in the Refuge Area WCA-1. The situation that years ago seemed relatively serious, almost hopeless, is beginning to look like the BPM/STA actions throughout the watershed seem to be working and more improvement may be coming because there usually is a lag time between the implementation and response in water quality. However, these positive trends should not result in complacency because it is still not clear whether or not these positive changes are caused by drought. Furthermore, the concentration maxima, while also dramatically decreasing, are still in WY 2013 higher than the standard in the interior and, obviously, more so in the inflow and outflow. It is not clear whether these deviations are part of the statistical distribution of all samples or these higher values indicate areas that are more impaired. The map on Figure 3A-9 indicates that the areas of higher TP concentrations are very small and isolated only to outflow/inflow (the outflow from one area is the inflow to the next downstream) and apparently to some canals.*

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

Response #8: Noted, thank you. As briefly explained on page 3A-4, inflows to the Refuge enter the Rim Canal in the north and is discharged in the south. Penetration of the water from the Rim Canal into the interior of the Refuge is generally limited due to the limited number of entry points (breaks in the levee) to the interior of the Refuge and negative hydraulic head differences between the Rim Canal and the interior of the Refuge (i.e., higher hydraulic head in the Refuge than in the Rim Canal). Therefore, the interior of the Refuge remains largely a hydrologically isolated rainfall driven system with little input from the Rim Canal. Because of the limited exchange with the Refuge, the water in the Rim Canal flows to the south where it is discharged essentially unaltered. In other words, the outflow from the Refuge is more of a reflection of the inflow water than the interior of the Refuge because of the reasons explained above. Another possible explanation could be that the Rim Canal collects water when marsh stages are high and depending on retention time's phosphorus concentrations can become elevated due to phosphorus flux from legacy phosphorus within the Rim Canal. Furthermore local runoff in the Rim Canal and outflow areas might contribute to higher concentrations than the interior.

Comment #9: *On Page 3A-29 lines 775-777 the authors seem to be surprised that the interior TP concentrations are smaller than the inflow. This is always so for settling and/or degradable compounds like phosphorus existing predominantly in a particulate form.*

Response #9: Noted, thank you.

Comment #10: *Pages 3A-35 to 3A-38 cover orthophosphate concentrations and loads. OP is a bioavailable mostly soluble part of TP, hence, it exhibited the same decreasing trends in the conservation areas and more or less steady state in the ENP area. OP concentrations currently are very low and since no criterion is available for assessing the impacts the conclusions would be the same as those for TP; i.e., the situation is improving.*

Response #10: Noted, thank you. It should be noted that due to the relatively low ortho-phosphate concentrations and the fact that ortho-phosphate is a portion of total phosphorus, the authors did not feel that ortho-phosphate load calculations were necessary.

Comment #11: *Pages 3A-39 to 3A-43 present the data statistics and trend for Total Nitrogen. The District collects and analyzes nitrogen separately for TKN (Total Kjeldahl Nitrogen = organic N and Ammonium) and nitrate/nitrite N and the Total N is then their summation but reports only Total N. There is now new and strong evidence that TKN and nitrate affects the eutrophication process differently. Both TKN and nitrite/nitrate are nutrients that could stimulate eutrophication; however, in the Everglades system phosphorus seems to be clearly the limiting nutrient.*

However, some overlooked older and new literature references noticed phosphorus release from the sediments is suppressed in waters with higher nitrate content and, as a result, the water body, be it an impoundment or coastal wetland fen, would not become highly eutrophic or hypereutrophic based on the nutrient loading (Andersen, 1982; Hemond and Lin, 2010; Lehman, 2011) as long as the nitrate content in the water above the sediment and top sediment layer is not exhausted. Also Lehman (2011) documented and concluded that phosphate and iron are reduced and released only when both oxygen and nitrate are depleted. In another case study of the nitrate effect on eutrophication of coastal wetland fens (Lucassen et al., 2004), eutrophication was prevented by high nitrate loads in groundwater. Liška and Duras (2011) reported the same phenomenon in a small headwater in the Czech Republic and, similarly, Lehman (2011) on an impoundment in Michigan.

Selig and Schlungaum (2003) observed two dimictic lakes and found sediments released ammonium and phosphate only when both oxygen and nitrates were absent. Andersen (1982), in a study of Danish eutrophic lakes, noticed phosphate release from sediments occurring if NO_3 *- - N concentrations were less than 0.1 mg/L and no release into the anoxic hypolimnion if* NO_3 *- - N was greater than 1 mg/L. It is therefore recommended that the authors should provide separate information for TN and nitrate.*

Fortunately or unfortunately Figure 3A-13 shows a close correlation of Total Nitrogen to the concentration of the total organic carbon indication that most of the TN is organic (KN) and ammonia is the nitrogen compound released into sediment by decomposition of the organic matter in the sediment.

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

Response #11: Noted, thank you. This section of this chapter has evolved throughout the years. Even though the Everglades ecosystem is a phosphorus limited system, it is important to understand the nitrogen conditions as well. Text will be added and/or revised regarding atmospheric deposition of total nitrogen. Additionally, the authors will review the provided peer review literature and assess it for analysis that could potentially be included in next year's chapter.

Total nitrogen within the Everglades is dominantly the organic (Kjeldahl Nitrogen) form as indicated by Figure 3A-13. Nitrate concentrations within the EPA are very low due to low redox potential (i.e. reducing environment) as indicated by low DO concentrations in the marsh. Therefore, due to the low nitrate concentrations, the suppression of phosphorus release from sediments with higher water nitrate content might not be applicable in the EPA. The authors will evaluate this concept to potentially include the analysis in next year's chapter.

CLOSING COMMENTS

The chapter did not venture into identifying quantitatively causative factors for the problems. Hence it does not provide adequate information for those developing or being interested in abatement of the problem.

Response: Noted, thank you. The authors are working on revising the chapter by providing phosphorus and flow diagrams to track phosphorus sources and provide relevant quantitative information and provide the linkage to management goals and objectives. This effort will certainly continue to evolve in next year's SFER.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3B

Paul Julian[1], Binhe Gu, Russ Frydenborg[3], Ted Lange[4], Alan L. Wright[5] and J. Mabry McCray[5]

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

WebBoard-Posted: 10/23/13

GENERAL RESPONSE

We appreciate the thorough technical assessment provided by the reviewers of this year's SFER Chapter 3B. Regarding the overall difference from last year's to this year's report, this year's chapter format consisted of sub-reports by multiple authors, The format of this year's report was changed to reflect a more data driven assessment using the best available Hg and sulfur data to assess mercury (Hg) and sulfur conditions within the Everglades Protection Area (EPA) and surrounding areas (i.e., Holy Land and Rotenberger Wildlife Management Areas), in a manner similar to the sister chapter within this volume, Chapter 3A. This data-driven assessment was deemed more appropriate by the state resource agencies because it provides more current information that is relevant to inform environmental policy and decision making. It should be noted that a general call to previous contributors and individuals conducting research within the fields of Hg and sulfur was placed well before the submission of the draft chapter. External to the state resource agencies [i.e., South Florida Water Management District (SFWMD or District), Florida Department of Environmental Protection (FDEP), and Florida Fish and Wildlife Conservation Commission (FWC)], only one individual out of ten that was contacted provided a contribution to the chapter.

SFER reviewers emphasized the differences between previous year's versions of the chapter versus this year's chapter with respect to the discussion on linkages between sulfate and methyl mercury (MeHg) production. This year's chapter reflects both the available new information and state agencies management needs, addressing an ecosystem-level view of Hg and sulfur issues. State agencies responsible for management and regulation of water resources developed their viewpoint after careful and balanced consideration of all information from defensible, rigorous scientific analysis of the best available data applicable to and derived from the EPA. The conclusions of individual researchers or personal opinions were given less weight in this year's report.

The second paragraph of this chapter states the objectives:

> "This chapter updates the status of mercury and sulfur monitoring in the Everglades region and provides a summary of biota mercury concentrations; mercury atmospheric deposition; and surface water sulfate (SO_4^{2-}) concentrations, loads, and atmospheric deposition to the EPA."

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The stated objective is consistent with the requirements outlined in the Everglades Forever Act §373.4592(13), Florida Statues. The original objective of this chapter is not to provide an extensive review of previously conducted methylation research, which would be largely redundant with past SFER chapters. Rather, the author and co-authors focused on the above stated objectives, using the best available data, while acknowledging that the emphasis of the report has changed from previous years. The authors will add a short section to the chapter to clarify our interpretation of available science on the sulfur and MeHg linkage. The elements of this new section are described below.

Within the reviewer's general comments there is a reference to the "Goldilocks" window. This metaphor refers to a supposed unimodal, bell-shaped relationship between surface water sulfate concentrations and MeHg production. The primary author responsible for this report does not feel comfortable with the term "Goldilocks window" based on actual measurements in the EPA and other scientific concerns. While it is recognized that sulfate plays a role in the sulfur/Hg biogeochemical cycle, the metaphor is not specific to sulfur and Hg, could refer to many different concepts, is not a widely accepted scientific term, and is a theoretical relationship pasted together from disparate data sources. Furthermore, the quantitative role of sulfate is still not understood so that predicting methylation rates and Hg bioaccumulation based largely on sulfate in the Everglades is simply not possible at this time. Biogeochemical cycling and bioaccumulation of Hg within the Everglades is complex and confounded by many other non-chemical variables, including the types of bacteria present, the ambient food web, and the hydrology of a particular site. Chemical factors such as the availability of dissolved inorganic Hg (i.e., Hg_{2+}), concentrations of dissolved organic carbon, pH, chloride, and dissolved oxygen also play a critical role in the Hg methylation process. Hg methylation is controlled not only by sulfate, but a combination of many environmental factors. Hg accumulation in fish also depends on diet availability and trophic position of fish.

The Goldilocks theory hypothesizes all other factors hold constant and sulfate is the only variable, which is not an accurate depiction of the real world conditions. Due to this complexity and variability, the sulfate/Hg unimodal relationship is not consistent throughout the Everglades and, in fact, mercury levels often vary orders of magnitude for a particular sulfate level. Very few studies have been able to determine a significant sulfate/Hg unimodal relationship under conditions found in natural marshes. Furthermore, analysis of the plethora of ambient monitoring data does not support the theory of a unimodal relationship between sulfate and Hg. We will provide a brief summary of ambient monitoring data in the reviewed chapter simply to demonstrate the magnitude of unexplained variability in sulfate and mercury data.

A common reference paper related to the sulfate/Hg unimodal relationship is Gilmore and Henry (1991), which includes a **Figure 1** (replicated below) that shows a theoretical relationship between water column sulfate concentration and sediment MeHg production based on a review of data collected across a large spatial extent across several ecosystem types. Based on the theoretical relationship within freshwater ecosystems, sulfate concentrations are relatively low when compared to estuarine or marine systems. At these conditions, Hg methylation within soils is sulfate limited. Meanwhile, ecosystems with relatively high sulfate concentrations, including estuarine and marine systems, potentially have a higher concentration of sulfide, therefore Hg methylation can be sulfide inhibited within these systems.

To give a sense of relative concentrations of sulfate between these systems, ionic composition within marine and estuarine water is dependent on salinity. Sulfate concentrations can make up approximately eight percent of the total ions present in saline waters, which corresponds to approximately 2,649 milligrams per liter (mg/L) of sulfate (Morcos 1973). In contrast, within freshwater systems (i.e., Arthur R. Marshall Loxahatchee National Wildlife Refuge/Water Conservation Area 1), sulfate concentrations can at times be within the method detection limit (at or below 1 mg/L; Julian et al., 2014). Marine and estuarine concentrations of sulfate are several orders of magnitude higher than sulfate concentrations observed within the EPA. Therefore, based on the theoretical relationship between sulfate and Hg, it is possible that Hg methylation within the Everglades could exist as a more linear or logarithmic relationship. However, as Gilmore and Henry (1991) suggest within their paper, other factors influence Hg methylation, therefore potentially altering the shape and intensity of the unimodal relationship. It should be noted that it is suggested that coastal Everglades Hg methylation and accumulation in fish species is also very high (Bergamaschi et al, 2012; Rumbold et al., 2011).

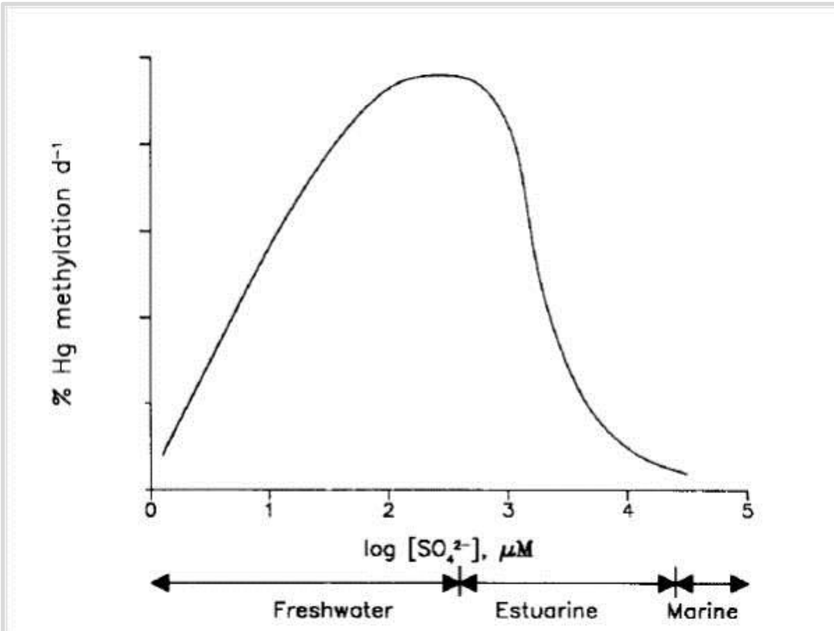

Figure 1. Theortical relationship between water column sulfate concentrations and sediment mercury methylation rate. [Source Gilmore and Henry (1991).]

The authors have observed throughout the years that factors that affect temporal and spatial patterns of MeHg production and subsequent accumulation in sport fish (i.e., largemouth bass) vary across the EPA as indicated by **Figure 2** (below). As discussed in the chapter, median Hg tissue concentrations for CA315 and WCA2U3 are virtually the same (0.55 and 0.50 milligram per kilogram (mg/kg), respectively), while sulfate concentrations at CA315 have reached concentrations below 1 mg/L and concentrations greater than 10 mg/L have been observed at WCA2U3. Trends in largemouth bass THg have followed different trajectories over the past two decades and do not seem to be controlled by sulfate concentrations alone.

Although resources are limited at both state agencies, the FDEP and SFWMD are continuing to collaborate on the development of a sulfur action plan that includes several activities related to this issue. During previous scientific workshops, several researchers, including chapter contributors from previous years, have agreed that, among the top priorities, a Sulfur Mass Balance must be completed for the Everglades Agricultural Area. As part of this work, FDEP and SFWMD are assessing sulfur sources and controls and the potential effects of controlling sulfur entering the EPA so that policy can be based on sound science. However, at this time, the agencies do not have enough information to accurately inform policy decisions related to sulfur reductions. More specifically, it is not known if the reduction of sulfur will reduce the quantity of

MeHg production and/or accumulation or if reduction in sulfate could potential shift hotspots within the system.

Based on the available data, FDEP and SFWMD have concluded that it is not currently possible to quantitatively identify the causative factors and linkages between MeHg and sulfate. What is known and accepted by these agencies is that this relationship is very complex, involving several different factors that influence both MeHg production and accumulation within biota. In the past, this distinction between production and accumulation has often times been confused: just because MeHg production is occurring does not necessarily mean that it is bioavailable or being accumulated in consumers. To help understand these distinctions, more data and additional parameters may need to be collected on a regular basis. Additional monitoring will be prioritized based upon scientific need and available resources.

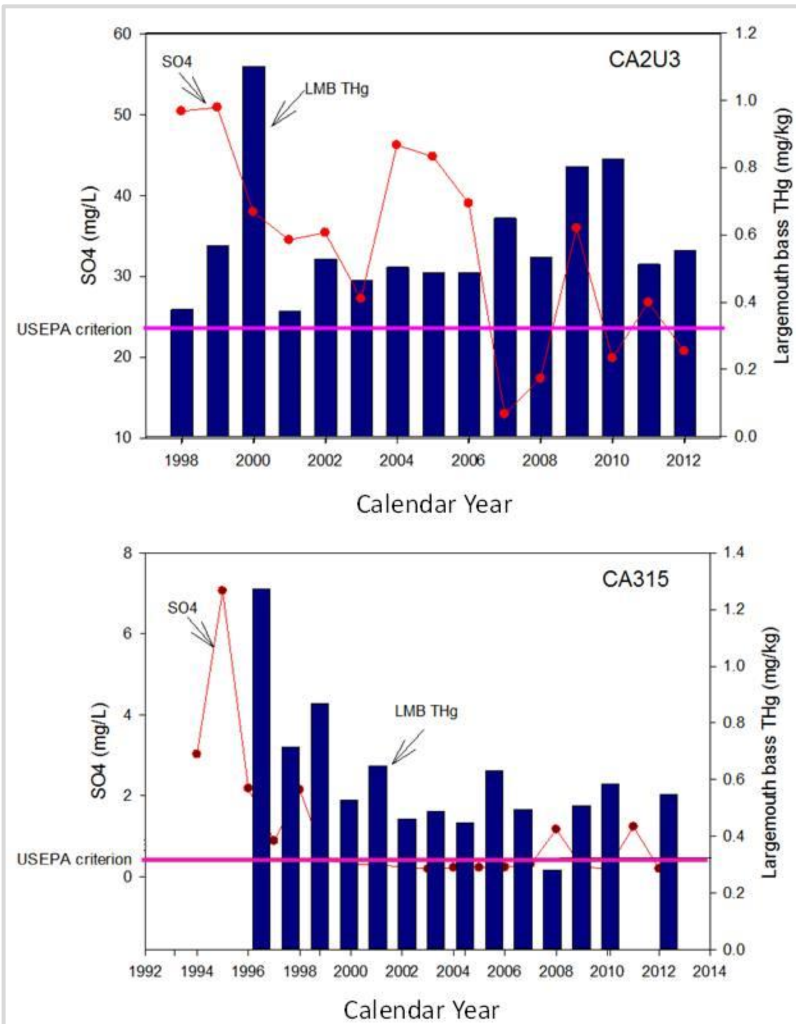

Figure 2. Calendar year mean sulfate and largemouth bass tissue total mercury (THg) concentrations for sites WCA2U3 located within Water Conservation Area 2 and CA315 located within Water Conservation Area 3. [Note: LMB – largemouth bass; mg/kg – milligram per kilogram; mg/L – milligram per liter; SO4 – sulfate; USEPA – U.S. Environmental Protection Agency.] d and available resources.

Reviewers mention suppression of methylation at modest ambient sulfate levels. To date, there has been no evidence of significant sulfide concentrations in areas with low Hg accumulation. Additionally, it should be noted that at high sulfate concentrations, sulfur and Hg^{2+} precipitation occurs, ultimately removing the limiting material in the Hg methylation equation, which leads to low MeHg concentrations. Therefore, the sulfide inhibiting portion of the hypothesized unimodal relationship (right-hand leg of **Figure 1**, above) remains unsupported. Please note that sites with very high sulfate concentrations are often associated with canals or marsh sites with a strong canal influence which represent completely different hydrological regimes than very oligotrophic marsh sites. Advection cannot be ruled out as a primary loss term for these sites.

The reviewers also comment on the Total Maximum Daily Load (TMDL) for Hg. We will provide additional information on the approved TMDL and will make the TMDL available by a link in the chapter.

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SPECIFIC COMMENTS AND RESPONSES

Comment #1: *Lines 116-119: A brief description of how fish were sampled (as is done for mosquitofish and sunfish) is warranted including number of fish (total and per site), size etc.*

Response #1: Text will be added to the chapter describing sampling methods.

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

Response #2: Text will be added to address this comment.

Comment #3: *Lines 320-329: Sampling protocol is more complicated for mosquitofish (MF) and sunfish (SF) and required a bit more description to aid in data interpretation. It appears MF individuals were composited at each site but SF were analyzed by individual fish (and site), but how sunfish species were separated by size and species this is not clearly stated. As written there is no way to know how many of each species were collected or to know if size was only a factor in sunfish (or for that matter why size matters if "up to 10 blue gill") of a specific size were collected. Please be more explicit here.*

Response #3: Text regarding sample collection will be changed in this section to provide a better description of fish collection methods. The sunfish collection protocol recognizes the size effect, so a specific range of total length is enforced during fish collection. The sampling protocol also recognize the differences in THg concentration among species of sunfish, so multiple species (all four species present in the Everglades) are collected while the 10 individuals of the most dominant species (bluegill) are collected during each sampling event. We report all trophic level 3 sunfish in this section.

Comment #4: *Lines 353-367: What is the typical lifespan of MF? Is it short enough to assume that annual variation in Hg concentrations reflects the annual variation in bio-available Hg? If so, this should be more clearly stated (even if only assumed) as this variation could be a co-determinate of the influence of wet and drought years and other factors influencing bio-availability.*

Response #4: Text will be added to address this question on mosquitofish life span and the purpose of using both mosquitofish and sunfish for Hg monitoring in biota.

Comment #5: *Lines 361-362: Something in these lines is worded oddly enough ("historically high in WY 2012"??) to make the meaning unclear.*

Response #5: Text will be revised.

Comment #6: *Lines 369-372: Does each point in the graph represent a specific year at the designated site? If so this could be clearly stated.*

Response #6: Text was added to clarify this point. Annual indicates each data point is an annual event (one composite sample).

Comment #7: *Line 398: It is not clear why SF data are not presented in a manner similar to the MF data, e.g. this year's data then the POR. Thus a figure similar to 3B-5 is a logical addition.*

Response #7: Figures will be reproduced for the site-specific (12 sites) and impoundment-specific (Water Conservation Areas and Everglades National Park) graphs, consistent for both fish species.

Comment #8: *Figure 3B-6: Since the goal of this figure is to compare temporal variation within a specific site (line 357), it makes more sense to separate the data by site then by year. Probably the easiest way to do this is to split the data into four separate panels, one for each site. As presented it is quite "noisy" and hard to decipher.*

Response #8: Figures will be added for both fish species separated by site and by year.

Comment #9: *Figure 3B-7: Error bars are missing.*

Response #9: Error bars will be added.

Comment #10: *Lines 412-436, Figure 3B-8 and 3B-9: At the least there is an error in referencing figures in these lines. The time trends mentioned on lines 412-415 are quite apparent in Fig 3B-9 (not 3B-8). More importantly, it is not clear what Fig 3B-8 represents (why is the data for this year compared to the POR)? An incomplete description of the sampling methods further complicates this section.*

Response #10: This graph will be removed. A revised sampling method description will be provided. Additionally, a graph will be added to display the THg concentrations among the three trophic level 3 sunfish.

Comment #11: *Line 514: How were areas of each "region" assigned? As with precipitation one appropriate technique would be Thiessen polygons.*

Response #11: Due to how the stations were established, a very simplistic approach was taken. Regions were delineated latitudinally with the northern region accounting for Water Conservation Area 1, the central region for Water Conservation Area 2 and Water Conservation Area 3 combined, and the southern region for Everglades National Park. This section of text will be included.

Comment #12: *Lines 558-570: The reason for, and importance of, the analysis and discussion of the hypothetical surface load is not apparent. If the surface is measurable why calculate a hypothetical load based on an assumed concentration?*

Response #12: Unfortunately, surface water total Hg is not monitored at the majority of locations entering and leaving the EPA. This section of text will be revised.

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

Response #13: Noted.

Comment #14: *Lines 571-575: These statements are certainly not supported by the data presented in Fig. 3B-11 which display a remarkable steadiness of annual values. Any "trend" over two or three years is well within inter-annual variation.*

Response #14: Noted, text will be revised.

Comment #15: *Lines 750-753: What would sulfate be limiting to, biological processes? This is not clear.*

Response #15: Yes, biological processes. The text will be revised.

Comment #16: *Lines 775-776: Something is misworded and obscures the meaning of this sentence.*

Response #16: Noted, thank you. The text will be revised.

Comment #17: *Table 3B-8: It looks like the mean and SE are reversed for flow volume of EPA inflow. It is hard to have a negative inflow.*

Response #17: Noted, this value is the mean ± standard error of inflow volume over the entire period of record (Water Year 1979–Water Year 2013; May 1, 1978–April 30, 2013). In some years, very little water entered the EPA, and during others, a lot of water entered. For a visual representation of this, please see Figure 3A-9 of the 2014 SFER–Volume I, Chapter 3A.

Comment #18: *Line 906: Same comment as on line 514. How where point values assigned to areas or regions?*

Response #18: Noted, thank you. Text will be revised.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 4

William Baker

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

WebBoard-Posted: 10/24/13

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

Response #1: For clarification, the purpose of this chapter is to report on non-point source control programs aimed at the reduction of pollutants in stormwater runoff.

Comment #2: *Apparently, the detailed review of the NP programs is performed every three years and WY 2011 (two years ago) was the year for such review. Hence Chapter 4 of the 2014 SFER is again limited in substance and results and focuses primarily on listing of the programs, their statutory background and regulations, permitting, and some future outlooks. Nevertheless, both Chapter 3A and Chapter 4 reviewed by this reviewer revealed that the Total P loadings and concentration reduction actions (both anthropogenic resulting from the implemented programs and natural meteorological effects -a note by the reviewer) surpassed the expectations of the 25% reduction of the load. The total cumulative reduction of the TP load between 1994 and 2013 water years is 55%.*

Response #2: The primary purpose of Chapter 4 is to provide annual updates on the non-point source control programs within the SFWMD. Regulatory programs in the Southern Everglades, including statutorily mandated quantitative performance metrics, are well established and annual results are provided (i.e. 41 % TP load reduction from the base period achieved for the EAA that outperformed the 25% performance metric requirement). Source control programs under development in the Northern Everglades are a mix of regulatory (SFWMD) and voluntary (FDACS) agricultural BMP programs as directed by the NEEPP. This Chapter will transition to more detailed annual reporting as program development progresses. In the interim, historical and WY2013 data are presented in Appendix 4-1 for the Northern Everglades basins.

Bullets will be added in the final chapter to the Lake Okeechobee, Caloosahatchee and St. Lucie River watersheds highlight sections beginning on line 77 of the draft chapter to clarify this point.

Comment #3: *Another positive outcome emerging from this chapter is the increasing reliance on regulation and permitting for nonpoint sources, e.g., in the Everglades Agricultural Area (EAA) which in the past in the Everglades region of impact were (and still are in most other states) voluntary. This should serve as an incentive and example to the US Environmental Protection Agency and other states struggling with their TMDL programs when they relied only on voluntarism of farmers. Obviously, as documented in the chapter, issuing the permits is not enough, it must be accompanied by the education of dischargers and the public, and continuing research as it is being done by the SFWMD and other agencies and universities involved in saving the Everglades efforts. NPDES permitting is mandatory nationwide for point sources such as urban runoff. This positive facet of the chapter was somewhat diminished by the report on participation of dischargers in the Northern Everglades which was low.*

Response #3: Regulatory non-point source control programs in the Southern Everglades EAA and C-139 basins are mandated by statute and have been implemented through issuance of permits since the program's inception. In the Northern Everglades, while an existing regulatory program has been in place since 1989 for Lake Okeechobee, the NEEPP statute directed the FDACS to develop a voluntary program for agriculture and this has been the primary approach for BMP implementation. The regulatory program is being amended to reflect the statutory changes. Unlike the EFA statute for the Southern Everglades, the NEEPP does not specify a performance metric for the source control component of the overall restoration plans in the Northern Everglades.

Comment #4: *On Page 4-3 the authors describe significant and above the goal decreases of TP loads from C-139 and Non Everglades Construction Projects (ECP). The details and information on other projects are in the bulk of the chapter. No nonpoint TP reductions were reported for the Lake Okeechobee watershed and the two coastal Caloosahatchee and St. Lucie River watersheds that drain the lake.*

Response #4: The District and the Coordinating Agencies are currently working on an appropriate approach to measure and report on progress towards meeting water quality goals in the Lake Okeechobee, Caloosahatchee and St. Lucie River watersheds. When this work is finalized the sections on the Northern Everglades will transition to more detailed data reporting as applicable.

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

Response #5: The C-23, C-24 and C-44 basins are part of the St. Lucie River Watershed. The locations are shown in Figure 4-12.

Comment #6: *Pages 4-7 to 4-15 provide an update on progress in the EAA basin. This basin is the largest source of TP both by area and the total load. It is the basin where most of the previously controversial sugar cane and other crops agribusinesses are located. It is noticed that permits were issued to the majority of dischargers and, as result of permitting and education, loads have decreased, using the terminology of Chapter 3A, "dramatically". However, after a* *closer look at Table 4-3 on page 4-11, the loads in the WY 2013 are greater than loads in WY 2011 and 2012. But the overall trend and 5year moving averages (Figure 4-3 page 4-12) show indeed significant reduction of the loads between 1995 and 2005, but leveling off after 2000. That indicates that the improvement occurred at the beginning of the BMP implementation program and current changes could be year to year statistical variations. The chapter did not adequately address this issue. Table 4.3 needs conversion factors for inch and acre-ft, and replacing archaic ppb with µg/L. We are now in the 21st century.*

Response #6: The rule does not require increasing levels of BMP implementation after initial implementation. Therefore, the conclusion that water quality improvement occurred at the beginning of the program is consistent with the expectations under the current regulatory framework. The requirement under the Everglades Forever Act is to achieve a 25% TP load reduction when comparing an individual year to base period levels. Figure 4-3 shows that some equilibrium has been established as BMPs are consistently implemented from year to year. The statistical model used to evaluate compliance with the 25% reduction requirement accounts for variation in annual rainfall for a relative year to year comparison to the base period.

Conversion factors will be added to the table for the final chapter.

Comment #7: *Pages 4-12 to 4-15 discuss control strategies and source control activities for EAA. The control strategy relies on mandatory implementation BMPs, which began in 1996 and incidentally coincides with the noticeable beginning of the decreases of TP loads from the EAA area. Comprehensive BMP plans include generally water management, nutrient management and sediment controls. As stated previously, making BMPs mandatory was a foundation for success. The goals and permits were derived from the TMDL. Basic manuals and guidance documents were prepared in the 1990s, farm operators were educated and trained, and the plan was supplemented by other actions such as flow diversions, and BMP research.*

Response #7: For clarification, goals and permits for the EAA are prescribed by the Everglades Forever Act. There is not a TMDL for the EAA.

Comment #8: *Chapter 3A revealed that most phosphorus, which is a key pollutant causing the water quality (eutrophication) problems in the protected Everglades area, originates from difficult to control atmospheric sources. Atmospheric sources were not covered in this chapter. Apparently, BMPs and the structural measures discussed in the chapter may have only limited or no impact on reduction of atmospheric sources. Also in the last review (WY2012) it was pointed out that a large phosphate (apatite?) surface mine is located in the vicinity of the Everglades watershed but the effect of the mine, definitely a significant source of the atmospheric phosphate loads, was not ascertained.*

Response #8: For clarification, the purpose of this chapter is to report on non-point source control programs aimed to reduce pollutants in stormwater runoff from areas within the SFWMD geographic region.

Comment #9: *Pages 4-16 to 4-23 present the activities and results in the C-139 basin. This basin is relatively small and the loads are also small, a fraction of EAA. Nevertheless, Table 4-5 and Figure 4-5 show a significant reduction of the loads which after 2010 became better than the target loads. Table 4-5 again needs conversions factors of inches and acre-ft to metric units and replacing ppb with µg/L.*

Response #9: Conversion factors will be added to the table for the final chapter.

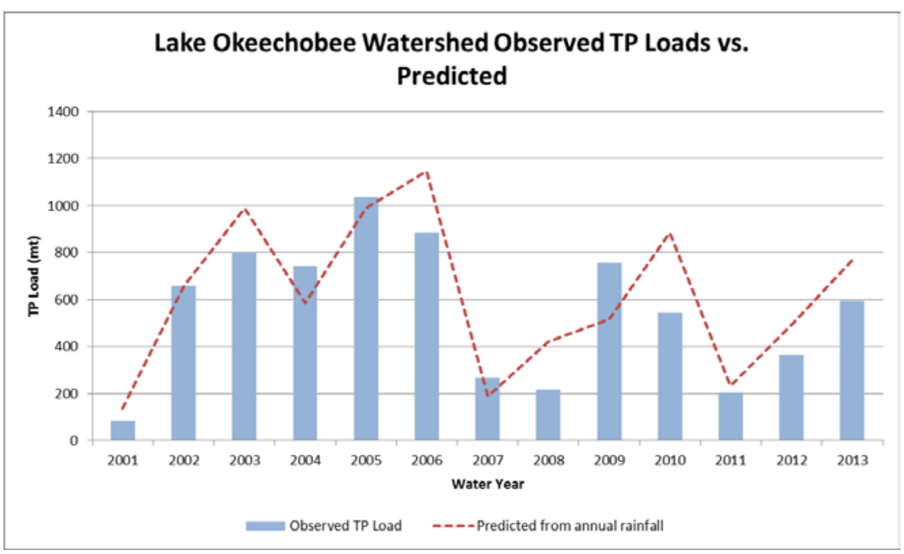
Comment #10: *Six basins that are not a part of the Everglades Construction Project Basins are covered on pages 4-24 to 4-29. Five of these basins have discharge structures operated by the SFWMD. The loads from these basins are relatively small, altogether their annual phosphorus load is currently around 15 tons and the loads exhibit a modest decreasing trend after 2006 (Figure 4-7).*

Response #10: No response is necessary.

Comment #11: *Pages 4-30 to 4-42 cover the North Everglades and Estuarine Protection Programs NEEPP. The area covers the watersheds of Lake Okeechobee, the Caloosahatchee River and the St. Lucie River. The two rivers/estuaries drain part of the outflow from Lake Okeechobee. The entire section extensively describes regulatory programs and the status of the activities. Unlike the previous sections, it does not provide any information on the progress and trends of nutrient loads. Some information on Lake Okeechobee and on the two river/estuaries is included in Chapter 10. In Chapter 10 nutrient loading information for the two rivers seem to be overwhelmed by a lot of other information and essentially lost. This could be because the hydrologic regime of the lake outlet and the two rivers is very complex, part of the flow from the lake enters the Everglades and a part feeds the rivers and goes to the Gulf of Mexico (Caloosahatchee River) and the Atlantic Ocean (St. Lucie River).*

Response #11: The District and the Coordinating Agencies are currently working on an appropriate approach to measure and report on progress towards meeting water quality goals in the Lake Okeechobee, Caloosahatchee and St. Lucie River watersheds that streamlines efforts and avoids duplication across the three agencies. When this work is finalized the sections on the Northern Everglades will transition to more detailed data reporting as applicable.

Preliminary analyses for the Lake Okeechobee watershed show positive results when watershed observed TP loads are compared to modeled loads that account for hydrologic variability (watershed rainfall) and approximate past conditions. The predicted loads in the following chart were generated by applying annual rainfall amounts and distributions to relationships established within in a reference period with little or no BMP implementation. The chart shows observed TP loads in recent years are more frequently below the predicted values, indicating an apparent reduction in loads from what would be predicted given the amount and distribution of rainfall.



Historical and WY2013 data for these watersheds can be found in Appendix 4-1.

Regarding the complexity of the hydrologic regime between the lake and the rivers, please note that detailed flow schematics for the Lake Okeechobee, Caloosahatchee and St. Lucie River watersheds can be found in Appendix 4-1 Figures 1, 2, 46 and 68.

Comment #12: *Thus the NEEPP section describes a "phased, comprehensive, and innovative "program containing source controls, construction projects, and research components that will be tied to the TMDL for the receiving water bodies. Apparently, this plan was described in detail in Chapter 8 of the 2012 South Florida Environmental Report and was reviewed and critiqued then. In this (WY 2013) report, Table 4-7 lists the nutrient control programs but no results.*

Response #12: Results in Chapter 4 are reported by geographic area as opposed to the individual programs listed in Table 4-7. The District and the Coordinating Agencies are currently working on an appropriate approach to measure and report on progress towards meeting water quality goals in the Lake Okeechobee, Caloosahatchee and St. Lucie River watersheds. When this work is finalized the sections on the Northern Everglades will transition to more detailed data reporting as applicable. Historical and WY2013 data for these watersheds can be found in Appendix 4-1.

The NEEPP requires three-year updates to the River Watershed Protection Plans and the Lake Okeechobee Watershed Protection Plan. The latest three-year update to the River Watershed Protection Plans was presented in the 2012 SFER while the most recent Lake Okeechobee Watershed Protection Plan Update will be provided in the 2014 SFER. Details on several of the programs in Table 4-7 is provided in Appendix 4-4 of the draft 2014 SFER which will be included in Chapter 8 of the final 2014 SFER.

Comment #13: *This section suffers from poor editing. It uses a lot of acronyms which are difficult to decipher. For example, an odd acronym WOD (works of district) was very difficult to find until it was located hidden in the title of Table 4-7. Also lettering on Figure 4-10 is very small and difficult to read. In general, the section on Northern Everglades provides no data and is lacking defensive synthesis and findings that would be linked to goals and objectives. The only quantitative result reported in this section is Table 4-8 reporting that only about 25% nonpoint nutrient dischargers (33% in the Lake Okeechobee, 12% in the Caloosahatchee River, and 3% in the St. Lucie River, watersheds) participate in the SFWMD programs. This is somewhat disappointing participation which makes the program at this time irrelevant as to its impact on the TP loadings to the Everglades.*

Response #13: The acronym WOD will be added where it is first described in the section and fonts will be enlarged in Figure 4-10 in the final chapter.

The percentages in the comment above only consider the acreages that are implementing BMPs under the SFWMD regulatory program which is only one component of the Coordinating Agencies' source control programs in the Northern Everglades. As described under Response #3, the strategy directed by the NEEPP includes a combination of regulatory and voluntary BMP programs. The objective of the BMP programs north of the Lake and in the River Watersheds is to ensure that landowners implement BMPs under either the District's regulatory program or the FDACS voluntary agricultural BMP program to avoid unnecessary duplication between the overlapping programs. The types of BMPs required under these two programs are consistent. Unlike the strategy in the Southern Everglades, the SFWMD regulatory program alone is not representative of progress in the Northern Everglades. Therefore, when considering overall source control program implementation under NEEPP, one must also consider the FDACS Agricultural BMP program (Table 4-10). Furthermore, it is important to note that the SFWMD's current rule must be amended to incorporate the expanded boundary described under NEEPP, that is, incorporate the northern areas of the Lake Okeechobee Watershed and the River Watersheds (see Figure 4-9). These expanded areas are in much earlier stages of development and implementation of source control programs than in the Southern Everglades and the southern portions of the Lake Okeechobee Watershed.

As such, the District and the Coordinating Agencies are currently working on an appropriate approach to report on progress towards meeting water quality goals in the overall Lake Okeechobee, Caloosahatchee and St. Lucie River watersheds.

Historical and WY2013 data for these watersheds can be found in Appendix 4-1.

Comment #14: *The last four pages of the chapter (4-43to 4-46) introduce the similar agricultural NEEPP programs carried out under the auspices of the Florida Department of Agriculture and Consumer Services. Herein, with about the same acreage (~ 2.1 million acres) the participation of agribusinesses is much larger, more than 71%. (Table 4-10). This table needs conversion factor from acres to hectares.*

Response #14: A conversion factor will be added to Table 4-10.

Comment #15: *Similarly to last year, the WY 2013 was apparently an "off-year" 2 of the three year assessment cycle. This caused that more than half of the chapter just described the programs according to some kind of prescribed outline which led to redundancies and repetition, especially for the Northern Everglades and estuarine water bodies but almost provided no data and results. These sections reported almost no results, the most important outcome were two simple tables listing the percentages of discharges that participate in the permitting program which was not that much for programs administered by the SFWMD. The report on watersheds south of Lake Okeechobee provide more information on reduction of the Total Phosphorus loading, namely from the Everglades Agricultural Area, C-139 and several non ECP small watersheds. The information on EAA (the largest of the reported watersheds), C-139 and non ECP watershed which are small, agrees with more detailed information on TP loading presented in chapter 3A.*

Response #15: The primary purpose of Chapter 4 is to provide annual updates on the non-point source control programs within the SFWMD. Regulatory programs in the Southern Everglades, including statutorily mandated quantitative performance metrics, are well established and annual results are provided (i.e. 41 % TP load reduction from the base period achieved for the EAA that outperformed the 25% performance metric requirement). Source control programs under development in the Northern Everglades are a mix of regulatory (SFWMD) and voluntary (FDACS) agricultural BMP programs as directed by the NEEPP. This Chapter will transition to more detailed annual reporting as program development progresses. In the interim, historical and WY2013 data are presented in Appendix 4-1 for the Northern Everglades basins.

Comment #16: Because of very few qualitative data being presented in the chapter it is almost impossible for the reviewer to answer the first two questions: (1) asking for assessing completeness and appropriateness of data and findings in the chapter, and (2) whether or not the synthesis is presented in a logical manner and consistent with the earlier version of the reports.

Response #16: See response to Comment 15 above. In addition to the data presented for the Southern Everglades in the chapter, supplemental data for the ECP and non-ECP basins can be found in Appendices 4-2 and 4-3, respectively. Historical and WY2013 data for Lake Okeechobee, Caloosahatchee and St. Lucie River Watersheds can be found in Appendix 4-1.

Comment #17: *Clearly, the previous reports were more comprehensive. They included results from research sites (e.g., in Taylor creek watershed). None were presented in this chapter. No results on loading and research were presented for the Northern Everglades.*

Response #17: The purpose of this chapter is to report on the status of the non-point source control programs in the SFWMD. Reporting on research in the Lake Okeechobee watershed is documented in Chapter 8 and in Chapter 10 for the River watersheds.

Comment #18: *The outline of the programs was impressive and contained a lot of good ideas. It is an integrated plan and the strategy includes (1) mandatory implementation of BMPs for phosphorus reduction, (2) regulatory programs, mostly for stormwater discharges, (3) voluntary programs, (4) educational programs, and (5) integration with local and regional water quality projects. But as shown in the second half of the chapter, participation and implementation for the northern watersheds are still far from satisfactory for SFWMD managed programs (about ½ of acres to be managed).*

Response #18: As described under Response #3, participation in the Northern Everglades has primarily focused on voluntary/incentive-based programs where as the Southern Everglades basins have relied on a regulatory approach.

Comment #19: *This chapter still has a problem with the consistency of units, mixes both metric and US units (with and without US to metric conversion factors).*

Response #19: Conversion factors will be added to the final chapter as applicable.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5A

Jennifer Leeds

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

WebBoard-Posted: 10/10/13

Comment #1: *Six new construction projects in three flow paths (described as Northern, Central, Western in the opening Summary; Eastern, Central, Western in later parts of the chapter) are described. Together they will add 6,500 acres of STAs (Stormwater Treatment Areas) and 110,000 acre-feet of water storage (116,000 acre-feet in the detailed project list) through construction of FEBs (Flow Equalization Basins).*

The information is this chapter is presented in a logical manner and clearly linked back to earlier work and to other ongoing work. I found the chapter very easy to read (although there are a few inconsistencies, noted above), and consistent with the rest of the report and previous reports.

Response #1: The inconsistency between the total FEB water storage volume of 110,000 acre-feet and 116,000 acre-feet is due to the difference between the original planning estimate for water storage volume of the A-1 FEB (54,000 acre-feet) and the detailed design water storage volume (60,000 acre-feet). The total FEB water storage volume of 116,000 acre-feet is correct and will be revised in the chapter for the final version.

In future SFER reports, this chapter will function to primarily update the status of the Restoration Strategies water quality improvement projects. The author wishes to thank the reviewer for their time and contribution.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5B

Chapter Coauthors

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

WebBoard-Posted: 10/23/13

A. Broad questions and comments that should be addressed:

Comment #A1: *This chapter is straightforward and very clearly, if dryly, written. There is very little technical information presented, really just two studies, the periphyton and mesocosm studies, discuss technical issues and certainly the first half of the chapter describing the performance of the STAs is not technical. Since Chapter 5 has now been split into several subchapters and appendices, perhaps it is time to divide it once more with one section describing STA performance for the water year relative to the POR, which is reviewed at the accountability level and the research summaries, which would be evaluated at the technical level.*

Response #A1: The subsection in 2014 SFER Chapter 5B entitled "*Applied Scientific Studies*" will be incorporated into Chapter 5C (*Update for the Restoration Strategies Science Plan*) in the 2015 SFER. Chapter 5B (*Performance and Optimization of the Everglades Stormwater Treatment Areas*) in the 2015 SFER will continue to address STA performance for the water year relative to the POR, as well as other STA performance results.

Comment #A2: *In general, operation of the STA's seems to have been very successful in WY2013, with record TP removal despite some adverse conditions, e.g. Tropical Storm Isaac. The P output from the STA's was the closest to the target values since this work started, which is promising for the future. To ensure continued improvement in the STA's functioning it is clear that whatever steps necessary to prevent the drying out of individual cells which led to large effluxes of P (and probably N) following re-wetting should be a priority. Alternatively, amelioration of dried sediments with substances that may prevent re-dissolution of P compounds or that may trap P produced by oxic degradation of the organic matter might be considered.*

Response #A2: The reviewers' assessment of the negative impact that dryout has had on the treatment performance of the STAs is correct. Within the limits of water availability, the District has made it a priority to keep the STAs hydrated during dry periods. The Flow Equalization Basins (i.e., storage reservoirs) that will be built as part of the District's *Restoration Strategies* may provide the STAs with a much-needed source of water during droughts. In addition, the District's *Science Plan* has a study that will investigate the use soil amendments to reduce the flux of P from STA soils. However, such an approach has significant logistical and economic challenges, as it would require applying large quantities of amendments to (potentially) many thousands of acres of STA.

Comment #A3: *Table 5B-14: This table is a nice way to briefly describe the research activities associated with the STA.*

Response #A3: A similar table was included in the last several SFERs. We borrowed this version of the table from the *Science Plan*.

Comment #A4: *Periphyton study: Potential results of this study are compromised by many operational difficulties, most of which are associated with a lower bed elevation leading to the influx of water and P to the periphyton cell. It appears that a water budget with an acceptable level of error is nearing completion, but a method to close the P budget remains elusive. We suggest one possible way to do this in the specific comments section. Regardless, when any important parameter (in this case the most basic, the P load) must be estimated rather than measured, the potential utility of the ensuing analysis is severely compromised. We fear this study will never lead to a determination of the effectiveness of periphyton relative to other plant and microbial assemblages yet the District seems very committed to the potential use of periphyton. Perhaps the P load issue can be adequately addressed, but the District should also consider smaller, more manageable (and less expensive) studies analogous to the mesocosm study to determine periphyton effectiveness. There is one comment not addressed specifically below. It appears that there has been no attempt to compare the periphyton cell the adjacent SAV cell, yet clearly, this was the intent of the original experimental design. Rather comparisons are made to the adjacent cells 3B and 2B. Why?*

Response #A4: The water and P budgets presented in this year's chapter are preliminary as collection of the data needed to improve accuracy of the budgets. Similarly, characterization of the vegetation and periphyton found in the STA-3/4 PSTA cell is also ongoing. It is important to note that the PSTA Cell has never been maintained as a "periphyton only" system (see response #B31). Actually, the PSTA Cell contains large amounts of SAV and associated epiphytic periphyton, as well as numerous emergent vegetation strips oriented perpendicular to flow. With respect to why a side-by-side comparison of the PSTA Cell and the Lower SAV Cell is not provided, the configuration of the Lower SAV Cell (i.e., the inflow structures are open uncontrolled culverts with no gates) has prevented delivering equal flow volumes to the PSTA Cell and the Lower SAV Cell. As a result, the Lower SAV Cell has received substantially more flow (and TP load) in its early years of operation, making a comparison of treatment performance between the two cells problematic. Instead, comparisons are made between the PSTA Cell and the large adjacent SAV cells, Cells 2B and 3B (e.g., HLR, PLR). We feel that this is a more meaningful comparison that can help guide scale-up of the PSTA Cell technology.

Comment #A5: *Mesocosm study: This appears to be a well-conceived study with the potential to shed light on the differences of plant type on performance. Interestingly the preliminary results seem to indicate the SAV communities might not be the best option, but this is confounded by two different essentially SAV treatments having different overall TP removal trends. The results on the types of P in the outflow should shed considerable light on the possible mechanisms at play. However the results are likely compromised by running the system in essentially a continuous flow mode with only by-weekly sampling. It might be useful to run the system for another year or two in batch mode to better assess the temporal dynamics and possibly calibrate removal models. That said, there will also be issue of scale-up from a mesocosm study to a full scale system.*

Response #A5: The Methods section indicates that experimental treatments were assigned to mesocosms with one factor (vegetation type) that included monocultures of three species (sawgrass, water lily, and cattail) for three treatments, a mixture of two species (spikerush & water lily, and southern naiad & muskgrass) for two treatments, and a control treatment with soil and no vegetation. As indicated, the non-vegetation control treatment was colonized by SAV species after the experiment began. In addition, two of the monoculture treatments, the sawgrass treatment and the water lily treatment, were also colonized with SAV species. Although the experiment had the potential to shed light on the difference of plant type on performance, the change that occurred in the one factor evaluated (vegetation type) has limited the ability to evaluate the difference of plant type on performance due to the loss in treatment fidelity. As alluded to in the comment, it is difficult to evaluate the role of SAV in the experiment, either as one of the treatments or through its colonization in the control and in the water lily monoculture treatment. Even with P speciation data, the change that occurred in the one factor evaluated (vegetation type) also make it difficult to evaluate the possible removal mechanisms at play. For these reasons, the study was discontinued in August 2013. The study does indicate that the water lily treatment that was colonized by SAV had the lowest mean outflow TP concentrations as compared to the mean inflow TP concentrations of any of the treatments (and some treatments had higher mean outflow TP concentrations). In addition, outflow PP concentrations from all treatments decreased relative to inflow PP concentrations and the mean DOP outflow concentrations for all treatments increased as compared to the mean inflow DOP concentrations. These results suggest that additional research be performed on the ability of alternate vegetation, particularly water lily, to remove additional water-column P to very low levels, and that P speciation be evaluated in these experiments to evaluate possible P removal mechanisms at play.

Comment #A6: *Temporal Dynamics study and Internal Water Quality studies: It is hard to consider these scientific studies analogous to the Periphyton and Mesocosm studies. Rather these are more appropriately termed ongoing monitoring and analysis studies. Nevertheless, they can be useful to the District as tools for optimization of the STAs. These studies appear very similar in nature except that the former includes an assessment of plant community as well as water quality, therefore it is more valuable. In the future it is recommended that all monitoring of internal dynamics consider water quality and vegetation distributions and densities simultaneously, enhancing cause-and-effect analysis.*

Response #A6: We agree that the simultaneous monitoring of vegetation and water quality can strengthen our internal wetland cause and effect analyses. However, because this is a resource intensive approach, we prefer to conduct this coincident monitoring opportunistically, for example, at times when we observe declines in vegetation health (and wish to assess water quality impacts, if any) or during periods when cell treatment performance declines for no apparent external reason (e.g., unusually high external P loads), and we wish to assess whether vegetation is playing a role in the observed water quality changes.

B. Specific comments and/or questions by line number.

Comment #B1: *throughout – the calculation of reduction efficiencies based on adjusted effective treatment areas gives a somewhat over-optimistic picture. I understand why the calculations were done but the efficiencies without this "correction" are also important as they represent the "true" efficiencies of the systems.*

Response #B1: Perhaps an explanation of what is meant by "effective" and the logic behind why adjustments are made to treatment areas will address the reviewers' concerns. First, treatment area is adjusted because it is not necessarily a constant in each STA. Each STA has multiple flow-ways and one or more of these flow-ways may be taken out of service, i.e., off-line, during a water year. When this happens, the area in the STA that processes water physically changes, treatment area decreases when a flow-way goes off-line and increases when the flow-way comes back on line. A time-weighted average treatment area, i.e., the adjusted effective treatment area, is the most appropriate value to use in calculations that include area. Using the total area of all flow-ways regardless of their operational status would overestimate the treatment area (a maximum value) whereas using only the area of flow-ways that were always on-line would underestimate treatment area (a minimum value). Second, the term “effective treatment area” dates back to the design of the original six STAs in the early 1990s and only refers to the total wetted surface area in a cell or STA without consideration of how much treatment actually occurs. Admittedly, there is no way of determining the “effectiveness” of any given wetted area relative to other wetted areas and use of the term “effective” to describe the entire wetted area is imprecise. Nevertheless, the terminology is firmly part of the District’s lexicon used to describe the STAs.

Comment #B2: *70 – these spikes can be a major contributor to the total annual flux. It would be useful to see an estimate of the portion of annual export attributable to the drying episodes.*

Response #B2: TP spikes that occur after the STAs rehydrate following a dryout event can be a major contributor to the total annual flux. It is a good suggestion to estimate the portion of annual export attributable to the drying episodes and this will be performed in future evaluations of dryouts when they occur.

Comment #B3: *128-129 (and similar sections for individual STAs): Perhaps the details of how flow-weighted means and annual loads are calculated could explain the discrepancy, but how removal rates and inflow and outflow correlations calculated from either parameter could be different is not at all clear. Presumably an annual load is the summation of the product of the FWM concentration and discharge over discrete time periods. It would be instructive to provide the equations used to calculate both parameters (as is done for adjusted effective treatment area on line 169).*

Response #B3: Total P loads and FWM TP concentrations were calculated based on surface water inflow to and outflow from the STAs and STA flow-ways over the entire water year. The annual load is the summation of the TP mass ($C_i \times V_i$) that passed through a structure over all sampling intervals within the year. The FWM TP concentration is the annual load divided by the annual water volume.

$$Load = \sum_1^n (C_i V_i + C_{i+1} V_{i+1} + \dots\ C_{i+n} V_{i+n}) \qquad (1)$$

$$FWM\ Conc. = \ Load / \sum_1^n (V_i + V_{i+1} + \dots\ V_{i+n}) \qquad (2)$$

where:

C_i = TP concentration for the ith sampling interval during the water year (g/m^3); V_i = Water volume for the ith sampling interval during the water year (m^3).

We added these equations to a new section at the beginning of the chapter. The reason that the load correlation coefficient does not match the FWM concentration correlation coefficient for a given STA is because the water volume in the denominator of equation 2 is different each year, i.e., each annual FWM concentration is weighted differently. Creating a synthetic data set that contains the observed TP loads but has constants for the inflow and outflow water volumes for all years and then calculating annual FWM TP concentrations will result in load and FWM concentration correlation coefficients that are the same.

Comment #B4: *145 – why is outflow volume always greater than inflow volume? Is this seepage in> seepage out?*

Response #B4: Outflow water volume in the STAs is not always greater than inflow water volume (although there may be a tendency for outflow to be > than inflow). During this past water year, for example, inflow to STA-5/6 was greater than outflow. In WY2012, inflow was greater than outflow in STA-1E, STA-1W, STA-5, and STA-6 (separate water budgets were calculated for STA-5 and STA-6 prior to WY2013). Similar occurrences of STA inflow > outflow can be found in earlier water years. Cases where STA outflow > inflow may be due to seepage in > seepage out or may be simply be a function of water budget error. For example, the differences between inflow and outflow volumes in STA-1E, STA-2, and STA-3/4 during WY2013 were 5, 2, and 4 %, respectively, values which are well within the expected range of measurement error for wetland water budgets.

Comment #B5: *245 – a drought contingency plan is good but perhaps it needs to go further, given that droughts and their impacts remained*

Response #B5: It is not entirely clear what the reviewers mean by "it needs to go further". The intent of the District's drought contingency plan is to make the water that is available for the STAs during dry periods last as long as possible. However, the STAs must share a finite water resource with other consumers in the region (i.e., agriculture and urban areas). The reality of water management in south Florida is that during prolonged droughts there has never been enough water to meet everyone's needs and the STAs have partially, or entirely, dried out. The Flow Equalization Basins that are being built as part of the District's Restoration Strategies will store large quantities of water, which may eliminate, or at least minimize, future STAs dry-outs.

Comment #B6: *319-321 (and similar sections for other STAs): A dedicated call-out description of which cells are in what flow-way is a nice addition which aids in the interpretation of the data.*

Response #B6: This approach was first used in the 2013 SFER for STA-5/6. We found it useful to describe the other STAs in a similar fashion in this year's report.

Comment #B7: *329-336: The wording here is a bit confusing because data for this water year is mixed with data for POR (even in the same sentence!). The format used in STA-1W (lines 469-473) is much more logical and that should be adopted here as well.*

Response #B7: The paragraph is confusing and has been reworded.

Comment #B8: *344-345: It is interesting to note that if the first two years of operation (2005 and 2006) are removed, there is likely a correlation between inflow and outflow parameters as there is with every other STA. Clearly performance was unique in the first year and this likely carried over to the second year. Though removal of any data should always be done with caution, it might be enlightening to assess the correlation with the first two years of data removed (especially since it is the first two years where some start-up effect might have been at play).*

Response #B8: All datasets for the STAs have data from their early years of operation, so including "start-up" data in the analysis of STA-1E is not unique. One might argue that if including start-up data somehow biases the analyses, start-up data should be removed from the all datasets and not just from STA-1E. Removing the first two years of data from STA-1E did result in a significant correlation coefficient between inflow and outflow FWM TP concentrations but NOT for inflow and outflow TP load; this correlation remained nonsignificant. Likewise, removing the first two years of data from the STA-3/4 dataset did NOT result in a significant correlation between inflow and outflow FWM TP concentrations; this correlation also remained nonsignificant. There was no consistent improvement in correlations by censoring data.

Comment #B9: *483 – I think that it would be very useful to have in-place oxygen monitors at several sites to help determine the mechanism for P release from the sediments*

Response #B9: P release from the sediments, or flux, will be evaluated in the *Science Plan* study entitled "*Evaluate P Sources, Forms, Flux and Transformation Processes in the STAs*". The detailed study plan is under development and may include oxygen monitoring.

Comment #B10: *498-503:"Substantial" is an insufficient adjective here. While flow diverted prior to treatment in the STA can't be used to assess the efficacy of the STA, how much water of* *what concentration is certainly germane to the success of the STAs in protecting the EPA region. While the diversion might be unavoidable, some mention of it numerical magnitude is appropriate.*

Response #B10: The Facility Status and Operational Issues sections for STA-1E and STA-1W were edited to include the volume of water and mass of TP diverted from these two STAs during Tropical Strom Isaac.

Comment #B11: *503-509: Why is gate G-307 preferred to G-309? Was it perhaps to minimize potential short-circuiting, or perhaps due to the vegetative clogging?*

Response #B11: [Refer to Fig. 2 in Appendix 5B-1] Operating G-307 provides the longest flow-path in this cell while using G-309 short circuits the cell.

Comment #B12: *517:527 (and similar sections): It is unfortunate that bird nesting success is at odds with P removal objectives. While not an expert in avian behavior, it would seem that these conflicts could be minimized if certain small areas within the STAs were dedicated as bird nesting sites. This could be done by construction of bermed* ***areas*** *or perhaps more effectively by construction of small "islands" within the cells. In fact, areas of higher topography would more closely mimic the original landscape of islands and sloughs and have little influence on the hydraulics of the individual flow-ways or cells if oriented properly. It could be an extremely cost-effective way to minimize habitat and water quality conflicts.*

Response #B12: Similar suggestions to mitigate the impact of ground-nesting birds on STA operations have been made over the years, e.g., building nesting islands within the STAs, building wetlands adjacent to the STAs, building nesting areas on the perimeter levees surrounding the STAs, etc.. These suggestions are all predicated on the assumption that such areas would divert birds away from the cell interiors to these new locations. There is little doubt that such areas would attract birds (e.g., black-necked stilts, killdeer, common nighthawks, and potentially several other ground-nesting birds). However, it is also very likely that providing additional nesting habitat would attract birds to the STAs in even greater numbers. Black-necked stilts use the STAs because they are very productive systems with ample forage and provide high-quality nesting sites as the cells dry out. In addition, black-necked stilts are not a colonial nesting species, so some birds probably would continue to use the STAs in preference to any new nesting areas. A single active nest within a flow-way triggers the same response from the District as having a great many nests. We also have concerns that building alternative nesting areas would attract other protected species that currently do not use the STAs and may further interfere with STA management activities. We have found that keeping cells flooded before nesting begins, provided we have sufficient water, limits potential nesting sites and is an effective way to minimize the impact nesting birds have on STA operation.

Comment #B13: *543-549 (and similar sections): It is much easier to follow the discussion of effects if changes are grouped in a positive and negative format. Thus rather than describe*

changes in plant cover by cells in a numerical order, it would be helpful to group them by desired vegetation type, e.g. cells 5A and 5B changing in the same direction (more EAV) would be desirable in 5A and not so in 5B (unless an increase in 5B was due to a planting of berms to minimize short circuiting).

Response #B13: The reviewer's suggestion was taken under advisement and the descriptions of changes in EAV coverage have been revised accordingly.

Comment #B14: *584-588: In other STAs the preferred treatment order appears to be EAV cells followed by SAV cells. Why are most flow-ways in this STA SAV followed by EAV?*

Response #B14: [Refer to Fig. 3 in Appendix 5B-1] The sequencing of vegetation communities in the multi-cell flow-ways of STA-2 (Flow-ways 4 and 5) is EAV followed by SAV, just as it is in all the other STAs. The flow pattern in STA-2 is complicated and we understand the reviewer's confusion. Flow-way 4 (Cells 4, 5, and 6) receives inflow via the G-438 A-J structures at the top of Cells 5 and 6 and discharges through G-368 at the bottom of Cell 4. There is a small amount of EAV at the top of Cells 5 and 6 followed by SAV throughout the remainder of these cells and in all of Cell 4. Flow-way 5 (Cells 7 and 8) receives inflow via the G-400 A-F structures at the top of Cell 7 and discharges through G-441 at the top of Cell 8. Cell 7 is EAV and Cell 8 is SAV. There are canals that separate Cells 1, 2, 3, and 4 from Cells 7 and 8 such that the outflow from the former cells does not enter the latter cells.

Comment #B15: *594-598: Considering the extensive soil testing that has been done over the years in the STAs it is interesting that (apparently) soil P content and mobility testing to address this hypothesis has not been conducted.*

Response #B15: Extensive soil testing was conducted in Cells 1, 2 and 3 of STA-3 in 2003, 2007 and 2009, which included measuring soil TP content. The soil TP results were presented in the 2011 SFER. We have not attempted to measure P mobility in these soils to date. There is a study within the District's *Science Plan* that will investigate biogeochemical differences between Cell 1 and other cells in STA-2.

Comment #B16: *This partially addresses the comment associated with lines 594-598.*

Response #B16: The District has no response to this comment.

Comment #B17: *672-679: What is the anticipated time-frame of conversion of cells 5 and 6 from EAV to SAV? Is total conversion even desirable considering the comment from lines 584-588?*

Response #B17: Current plans to convert approximately 75% of Cells 5 and 6 from EAV to SAV will be completed by 2017. This conversion will be conducted incrementally with three phases of aerial herbicide treatments, each followed by a 1-2 year period of SAV establishment.

Comment #B18: *635 – agreed. The success of Cell 1, if understood, may give lessons that can be adopted on a wider basis.*

Response #B18: Lessons learned from investigating Cell 1 may not translate into management strategies for the other STAs. For example, we cannot change the antecedent land use (farmed vs. non-farmed) of the existing STAs and while altering the ground elevation relationships among cells (i.e., reconstructing one cell to have a higher average ground elevation relative to the adjacent cells) is technically feasible, such an undertaking may be prohibitively expensive given the immense size of the cells in the STAs.

Comment #B19: *754 – same as 635. A careful analysis of why STA 3/4 works best is warranted as this may provide information needed to optimize design in future.*

Response #B19: To understand why STA-3/4 works best we need comparative information on a subset of, if not all, the STAs, i.e., we need to compare the performance of STA-3/4 against other STAs. The data currently available with which to make such an analysis are limited. The current STA monitoring program was designed to characterize the treatment efficiency of these wetlands based on comparison of inflow versus outflow water quality. Surveys of vegetation coverage and sediment characteristics have complimented the water quality program. What we lack is information on how the controlling biogeochemical processes differ both spatially and temporally among the STAs. Such process level information will be evaluated as part of the District's *Science Plan*.

Comment #B20: *692: Confusion here; according to Figure 5b-12, cell 2 is desired to be SAV not EAV (consistent with the text). But in Figure 3 in Appendix 5B-1, cell 2 is shown to be EAV. Which is correct?*

Response #B20: Actually, both maps are incorrect. Cell 2 is partly EAV and partly SAV. Both maps have been corrected.

Comment #B21: *761-763: See the comment for lines 128-129. It is not clear how there could be a correlation for one parameter but not the other.*

Response #B21: See the response to comment #B3 as to why correlation coefficients for the loads and FWM concentrations for a given STA would be expected to be different. In addition, note that the statistically significant correlations for loads are always higher than the corresponding correlations for FWM concentrations, sometimes markedly so. Although the FWM concentration correlation for STA-3/4 was not statistically significant, its p-value (0.09) was only slightly greater than the significance level (0.05) used to evaluate all statistics. The Person product moment-correlation statistic is sensitive to the influence of outliers at small sample sizes and there were only 10 observations for STA-3/4. Removing two observations from the STA-3/4 dataset did result in a significant correlation coefficient for FWM TP concentrations.

Comment #B22: *775-777: See the comment for lines 498-503.*

Response #B22: This sentence is in error and has been removed from the chapter. There was no diversion of water from STA-3/4 in WY2013.

Comment #B23: *895: Do you mean, Cell 5-1A rehabilitation?*

Response #B23: Yes, the text has been corrected.

Comment #B24: *917 – "flow-way 7 ...operational throughout WY2013..." but "..flow-way 7 was offline..." ??*

Response #B24: This was an editing error. FW 7 was offline for part of the year. The text has been corrected accordingly.

Comment #B25: *917: If flow-way 3 was operational all year, why are the data not reported in Table 5B-11? (Perhaps there is a typographical error explaining the two comments???)*

Response #B25: Omitting Flow-way 3 data from the table was an oversight. The WY2013 data for this flow-way have been added to Table 5B-11.

Comment #B26: *942-943: Is this criterion used for both SAV and EAV cells? It seems EAV could tolerate a lower stage, (perhaps -0.5 ft) with minimal disturbance (depending on the variation in topography with the cell). Though perhaps a different criterion is appropriate if based on plant survivability versus P release due upon reflooding.*

Response #B26: This paragraph only defines the stage (i.e., water elevation) at which a cell is considered "dry" and is independent of the plant community. It is the Operation Plan for each STA that sets a minimum stage for EAV and SAV communities: the minimum stage is -0.5 ft below average ground elevation for EAV and +0.5 ft above average ground elevation for SAV. Provided there is water available to hydrate the STAs, the Operation Plans specify that water levels should never fall below the minimum stage. The intent of maintaining water at or above the minimum stage is to ensure plant survivability.

Comment #B27: *945: Was there a negative impact on the SAV community of cell 5-3B?*

Response #B27: No, none that was observed.

Comment #B28: *1012 and elsewhere – I am concerned about the widespread chemical treatments without comment on the potential or actual effects on organisms other than the target plant species. I haven't been through the appendices so I may have missed something, but information of the toxicity and bioaccumulation of whatever chemical are used should be included somewhere.*

Response #B28: The District uses only EPA registered herbicides applied by licensed applicators at the dosages recommended by the manufacture(s). None of the products we use bio-accumulate and none are restricted category herbicides. While these products are certainly toxic to plants, toxicity is negligible to non-plant organisms at the application rates used in the STAs and elsewhere throughout the District. The District's herbicide program is regulated by the Florida Department of Protection and fully complies with our NPDES permit regulations. An accounting of the types and quantities of herbicides used in the STAs during WY2013 is provided in *Volume III, Appendix 3-1: Annual Permit Report for the Everglades Stormwater Treatment Areas - Attachment E: STA Herbicide Application Summary for Water Year 2013*. The Vegetation Management section at the beginning of the chapter has been edited to provide this information.

Comment #B29: *1136-1146 (and associated tables and figures): Table 5B-15 indicates that the concentration of TP was 16 ppb and flow was maintained for 365 days. Yet data in Figure 5B-30 indicates a mean inflow TP of about 12 ppb "during the period of flow" (line 1173). These would seem to be incompatible data.*

Response #B29: In Table 5B-15, the mean inflow TP concentration of 16 ppb during WY2013 was calculated using data collected over the entire 365-day operational period. The title for Table 5B-15 has been edited to indicate that the values are for the entire operational period. The mean inflow TP concentration in Fig. 5B-30 and referenced on line 1173 was calculated from data collected on only nine dates during the period of high flow shown in Fig. 5B-29. The text has been edited to make this these distinctions clear.

Comment #B30: *1178: It is stated nowhere in this document (that we could find) that periphyton and algae are the same thing, except that it could be inferred from this title. This should be stated more clearly, or if not correct, just exactly what organisms are included in periphyton should be stated. In general this section is assuming that the reader is more familiar with the terms used than (at least one of) the reviewers are.*

Response #B30: The reviewers overlooked the definition of "periphyton" the first time the term was used on page 5B-4, lines 93-94.

Comment #B31: *1179-1182: Related to the previous comment, it is not clear why there is SAV in the periphyton cell, presumably SAV is a marcrophyte and should not be in a periphyton-dominated cell. Note that as one reads on it gradually becomes clear that the periphyton is associated with marcophytes, but then one could assume that periphyton is in all STA cells, whether they be EAV, SAV or other material. So what is unique about the periphyton cell? There is not enough description to understand what the intent of this plant characterization is or what makes this cell unique from others.*

Response #B31: As the reviewers noted, what the District terms "PSTA" is not a pure periphyton community, it also contains large amounts of SAV. The reviewers are also correct in that periphyton is ubiquitous throughout all the STAs. The species composition of the vegetation in the PSTA Cell is similar to the vegetation found in SAV cells. To avoid the hydraulic penalty incurred at the shallow operating depths needed to exclude SAV, the PSTA Cell has been run deeper and SAV invaded the cell. What makes the PSTA Cell different from other SAV cells is that the muck in the PSTA Cell was removed down to the limestone caprock as described in lines 1076 to 1079. We added a footnote with additional explanation. While the use of the acronym

"PSTA" may be technically imprecise, this is another example of a term that has a long history of usage at the District and is firmly established in the District's lexicon.

Comment #B32: *1211 – the P values seem very low. I assumed that they must be expressed on a wet weight basis instead of a dry weight basis, but this apparently isn't the case (l. 1196). They do seem about 10x lower than "normal" however.*

Response #B32: A combination of factors contributed to the low periphyton tissue P content measured in the PSTA Cell. These values are on a dry-weight basis, and were diminished by their high mineral content; Ca content of periphyton can be 10% or more, by weight. In addition, the ultra-low surface-water P concentrations, low external P loading, and the lack of muck sediments as an internal P source, would be expected to result in low periphyton tissue P content. Where the external and internal P loading rates were (presumably) higher (i.e., the muck-based STA-3/4 Cell 2B), the periphyton tissue P content was markedly higher. Nevertheless, our results were within the lower end of the range of values reported by others for Everglades periphyton, as cited in Table 5B-16.

Comment #B33: *1223: The high Ca concentration might be an indication that a significant percentage of the periphyton growing in the cell are diatoms. Is there any way to distinguish between different species of periphyton?*

Response #B33: High Ca soil concentrations are always found in SAV cells in the STAs due to the precipitation of Ca from the water column under intense photosynthetic activity by SAV and periphyton. We find heavy Ca encrustations on the leaves and stems of SAV and periphyton mats (typically dominated by cyanobacteria and green algae) associated with SAV during the summer. While diatoms do constitute a portion of the algal assemblage, they usually are not the dominate group. Yes, the species composition of periphyton can be determined by examination under a light microscope. The District and its contractors have performed cursory examinations of periphyton taxa composition from time to time, but this had not been done on a regular basis.

Comment #B34: *1235: What is APA? It is clear a few lines later, but the acronym should be defined immediately.*

Response #B34: The reviewers overlooked the definition of APA the first time the acronym is used on page 5B-59, lines 1158-1159.

Comment #B35: *1346-1351: It is not had to see why P concentrations at the outflow structure went down during flow (Fig. 5B-44) as influent when down. However it is a bit disconcerting that there was no change in TP during conditions of no flow and that all reduction in outflow TP during flow appears to be due to the decrease in inflow. These data would appear to suggest that the periphyton cells are not working as intended. One would certainly expect P concentrations to go down during periods of no flow as the system would behave as a batch reactor. This of course ignores the possible effects of seepage, but the water budget (and stage) should shed light on this.*

Response #B35: It is not a concern that the PSTA Cell did not appear to remove P when the inflow concentrations were extremely low (10 ppb or less). This very low inflow P condition is not anticipated to occur often and is below the outflow TP concentration mandated for the STAs so no further TP reduction would be needed. However, at times when the PSTA cell inflow concentrations are in the 15- 20 ppb range, the PSTA cell has been able to reduce concentrations to the 10-12 ppb range, therefore the PSTA cell is working as intended. As noted in our response above, seepage and water (and P) budget results are still preliminary, and we are still evaluating the possible effects of seepage and improvements to the water and P budgets. We have observed that during periods of no flow, water-column P concentrations often increase slightly not only in the PSTA Cell but in other STA cells as well. The mechanism(s) responsible for these small concentration increases are not clear.

Comment #B36: *1437-1442: Continuing with the above comment, it should be feasible to either a) estimate the P concentration in the seepage water by using the data in figure 5B-44 and the calculated k values and Equation 5, or b) alternatively estimate the k values by making assumptions of the P concentrations in the seepage water. The first approach probably makes more sense, and could help close the P budget.*

Response #B36: As noted above, all data presented are preliminary and subject to revision. Water quality data will continue to be collected and analyzed over the remainder of the PSTA study in order to try to close the P budget.

Comment #B37: *1481: It would appear that after SAV colonization of the soil treatment it would, for practical purposes, not be any different than the SAV treatment, yet the performance was markedly different in these treatments (Table 5B-19). Is there any possible explanation for this?*

Response #B37: There is concern that the artifacts (something observed in a scientific investigation or experiment that is not naturally present but occurs as a result of the preparative or investigative procedure) in the mesocosms may have affected the results. The SAV treatment was established at the beginning of the experiment and the colonization of SAV in the soil (control) treatment occurred later, so results from these treatments are not directly comparable.

Comment #B38: *1515 – based on the mesocosm study, it seems that the P removal is largely PP removal, i.e. vegetation is trapping particulates in the inflow rather than using P to build biomass and sediments. How far do you think that this applies to the full-scale STA's? Table 5B-19 also demonstrates this.*

Response #B38: Settling and trapping particulates originating in the inflow probably occurred in the mesocosms and occurs in STAs. One goal of the *Science Plan* study entitled "*Evaluate P Sources, Forms, Flux and Transformation Processes in the STAs*" is to try and determine if PP at the outflow originates primarily from the inflow or is generated within the STAs.

Comment #B39: *1753-1754: Is the "TP load" from the canal sediment to the influent to STA2? If so this is not clear. Also it would be good to provide the reader with the size of the canal. How wide is it? How deep?*

Response #B39: Clarification was provided that the canal under discussion supplies water to STA-2 and a footnote was added that describes the configuration of this canal. At this time, it is not clear if this TP load was derived from the canal sediments, groundwater entering the canal, flow data uncertainty, or other computation artifact. A more comprehensive canal study is included in the Science Plan entitled "*Evaluation of the Influence of Canal Conveyance Features on STA and FEB Inflow and Outflow TP Concentrations*". This study will provide more insightful suggestions about the status of TP load contribution from the sediment.

C. Figure and table comments:

Comment #C1: *Figure 5B-14: Where vegetation sampling occurred is not clear, though one can surmise it is only the lower portion of cell 2. The left side should contain only cell 2 and the lower area highlighted to emphasize this was the area sampled (if we have surmised correctly).*

Response #C1: The figure had been edited to make it clear the area in STA-2 that corresponds to the SAV maps.

Comment #C2: *Figure 5B-25: The panel showing total SAV coverage is very useful. This should be added to the analogous figures displayed for the other STAs.*

Response #C2: A plot of total SAV coverage has been added to all maps.

D. Editorial comments by line number:

Comment #D1: *345: panels B and D*

Response #D1: Correction made to text.

Comment #D2: *580: should Basin S-2 be repeated, or are there 4 basins that can contribute runoff?*

Response #D2: Actually, there are 3 basins that can contribute water to STA-2: S2, S6 and S7. A portion of the outflow from the S-2 basin enters the S6 basin, while the remaining STA-2 outflow enters the S7 basin. The sentence has been reworded to clarify this relationship.

Comment #D3: *625: …the other two operational flow-ways…*

Response #D3: The sentence was reworded to make it clear that the reference is to Flow-way 2 and 3.

Comment #D4: *1088: …data at all of the water-control…*

Response #D4: In the case of "all (of)," SFER style excludes "of" whenever possible (e.g., all the structures), consistent with style guidelines of The Chicago Manual of Style.

Comment #D5: *1090: comma after inflow*

Response #D5: Made suggested edit to text.

Comment #D6: *1359: …exhibited a small diel pattern…*

Response #D6: Made suggested edit to text.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5C

Larry Schwartz

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

WebBoard-Posted: 10/23/13

Broad Comments:

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

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

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

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

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

Response #1: Numerous District scientists, engineers, and modelers with considerable hands-on experience with the STAs examined factors affecting STA performance over the years to determine the overarching key questions. The Science Plan was structured and developed around six overarching key questions. A more comprehensive list of sub-questions relative to the key questions was then developed. Although many potential areas of investigation were identified during the generation of sub-questions, implementing all of these for hypothesis testing at the same time is not feasible. Consequently, this information was further evaluated with the goal of determining a proposed initial suite of studies or research that should be included in the Five-Year Work Plan. Eight sub-questions were selected as the basis for developing the proposed study plans in collaboration and consultation with Technical Representatives from the U.S. Environmental Protection Agency, Florida Department of Environmental Protection, U.S. Department of the Interior, and U.S. Army Corps of Engineers. This included a special workshop with the Technical Representatives and federal agency experts and their consultants to review and deliberate on the list of proposed study plans, make refinements and, based on their input, translate the most relevant sub-questions into seven proposed initial studies (two sub questions were combined into one study plan). Based on their recommendations, two additional studies were also added as other areas of investigation (STA Water and Phosphorus Budget Improvements and Evaluation of Sampling Methods). The selection process was semi-quantitative and utilized best professional judgment of the Science Plan Team, Technical Representatives, and federal agency experts and consultants. In addition, several opportunities for public and stakeholder participation and review of the sub-questions were provided. The remaining sub-questions or any new ones generated as the Science Plan is implemented will continue to be evaluated and prioritized annually through an adaptive management process. Information on the Science Plan will be reported in Chapter 5C (Update for the Restoration Strategies Science Plan) of the SFER.

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

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

Response #2: Chapter 5A (Restoration Strategies) is referenced twice in the Background Sub-section of Introduction Section of Chapter 5C (Update for the Restoration Strategies Science Plan).

Are findings linked to management goals and objectives?

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

Response #3: No comment needed.

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

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

Response #4: These recommendations can be implemented.

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

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

Response #5: The acronym FEB will be defined at its first occurrence (line 150).

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 6

Fred Sklar and Thomas Dreschel

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

WebBoard-Posted: 10/23/13

Dodds, WK. Primary reviewer (AA)

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

P. Dillon, secondary reviewer (A)

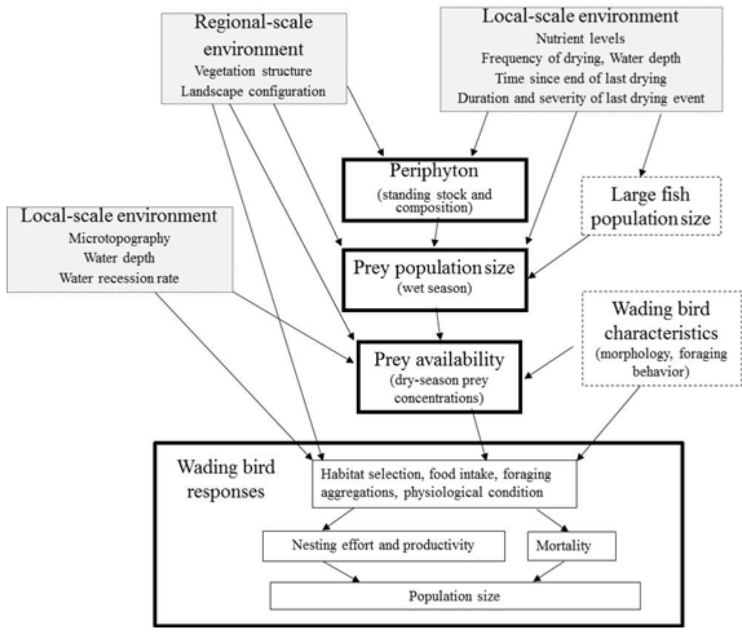
General

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

Comment #1: *The higher than normal precipitation was correlated with a high rate of nest failure, and perhaps led to some food limitations in the wading bird populations. Crayfish, Procambarus fallax, movement during desiccation was explored. Exotic fish continued to increase. In general, there are quite a few statements in the Wildlife and the Hydrology sections about factors that could influence nesting success. It seems that there should be some effort building a unified conceptual model to predict nesting success for each of the species. Conceptual diagrams could then be used to assess progress toward understanding the factors that encourage restoration of population sizes.*

Response #1: Author Response – The District agrees with the reviewer's comment. Multiple conceptual ecological models have been developed as a framework for evaluating, synthesizing, and organizing existing understanding of key ecological linkages among hydrological drivers and wildlife responses in the Greater Everglades. The example below is the primary conceptual model the District uses to guide its wading bird and prey related research.

Figure 1. Trophic Hypothesis for CERP MAP linking environmental drivers affected by restoration and wading bird population size. A chain of direct and indirect effects link human actions to wading birds, largely mediated through wading bird food availability during nesting season. Environmental drivers are in grey boxes, monitored variables discussed in this SSR are in boxes with bold borders. Ecological factors discussed in this report, but not monitored, are in boxes with dashed borders.



For more details see:
http://www.evergladesplan.org/pm/ssr_2012/ssr_2012_pdfs/2012_ssr_full_web.pdf

Comment #2: *The crayfish work is solid, some more advanced tracking devices could prove more definitive, but the results would likely stand.*

Response #2: Author Response – More research is needed to understand how crayfish become available to birds, and in this respect we are currently investigating the use of passive integrated transponder (PIT) systems and field enclosures for tracking detailed movement behaviors of crayfish in an experimental setting (see Rehage et al. 2013 for work we have conducted so far on small fishes in LILA). That said, a detailed account of the current crayfish experiment is in review in the journal Freshwater Biology where we discuss in further detail how the abundance and mark-recapture data provide strong evidence for inter-habitat movement of crayfish. In particular, we provide a post-hoc examination of the abundance data in terms of hypothesized expected population responses in each habitat, which we test with a simple mathematical model (see below).

To understand the observed changes in crayfish abundance in terms of a prey concentration mechanism it is important to establish how much the changes represent actual movement from ridges to sloughs. If the majority of the crayfish population moved from the ridges to the slough as water level decreased, and assuming that (1) the catchability of crayfish was similar in the two habitats, and (2) the ridge and slough environment within each wetland was itself a closed system such that there was no loss or gain in crayfish due to burrowing, predation, recruitment or migration, then the sum decrease on the ridges should approximately equal the sum increase in the slough. To test this post-hoc hypothesis we evaluated the changes in abundance with a simple mathematical model that accounts for differences between the habitats in area and initial crayfish abundance. We define LR(I), SR(I) and S(I) as the total number of crayfish in the large ridge (LR), small ridge (SR) and slough (S; slough and gator hole) habitats, respectively at the intermediate (I) stage. LR(L), SR(L), and S(L) represent the corresponding abundance in these habitats at low (L) stage. We use an empirical linear regression developed by Dorn, Urgelles & Trexler (2005) to transform the crayfish abundance per trap (Ntrap, individuals per trap) measured in this study into crayfish density (N, individuals m^2; N = 0.63 + 0.62 * Ntrap). The relative change (del) in crayfish abundance in these habitats can then be expressed as:

del-LR = [N_LR(I) – N_LR(L)] * A_LR (E1.1)

del-SR = [N_SR(I) _ N_SR(L)] * A_SR (E1.2)

del-S = [N_S(I) – N_S(L)] * A_S (E1.3)

where A_LR, A_SR and A_S represent the area (m^2) of the large ridge, small ridge and slough, respectively, in a given wetland. Using areas for these habitats (~11,250, ~4,800, and ~4,500 m2, respectively) and the change in total trapped individuals in each habitat (averaged over the two recession wetlands) of 76, 42 and 280 individuals, respectively, we can solve equation 1.1-1.3 and compare the relative movement between the two habitats:

Total decrease in crayfish on the two ridges = del-LR + del-SR = 1562 + 8835 = 10397

Total increase in crayfish in the slough = del-S = 9765

The model indicates that within a macrocosm, approximately 10,400 crayfish moved out of the ridges and 9,800 individuals moved into the slough. That is, almost all (96%) of the decline on the ridges was accounted for by the corresponding increase in the sloughs. This suggests that few individuals were lost during the drydown to factors such as predation, burrowing or emigration to other habitats. To provide a more conservative estimate of the relative change in abundance, we alternately assume that only 50% of the crayfish in the large ridge may enter the focal study slough, while the other half move to the adjacent deeper slough on the north side of the ridge (Figure 6-15a). In the latter case, total crayfish movement from the two ridges would be 5980, leaving 40% of the increase in slough crayfish unaccounted for. Nevertheless, the remarkable similarity of these values, using nominal or conservative estimates, suggests that movement alone, from ridge to slough, can explain most of the observed change in crayfish abundance during the drydown.

Rehage et al. (2013) Turning passive detection systems into field experiments: an application using wetland fishes and enclosures to track fine-scale movement and habitat choice. Acta Ethol. DOI 10.1007/s10211-013-0154-4

Comment #3: *The plant ecology section focused on two areas, species surveys of ridge and slough areas, and detailed sampling of tree species on tree islands. The LILA experiment has shown some interesting results, and given that there are trends, but that they seem to be ongoing, this experiment should be maintained and periodically evaluated (maybe every 3-5 years).*

Response #3: Author Response – Thank you for your comments. The LILA project as a whole is ongoing and the experiments discussed here are monitored and evaluated on an annual basis (likely more frequently than that, but reported on yearly).

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

Comment #4: *The LILA hydrology work with the artificial islands is a neat experiment, very interesting and nice that it is accomplished at relevant scales.*

Response #4: Author Response – Thank you for your comments.

Comment #5: *The cattail removal experiments are promising, but the AMI-1 study is not replicated and difficult to interpret.*

Response #5: Author Response – The AMI-1 wildlife study is treated as replicated in that there are two plots blocked across two potentially different hydrologic conditions. The primary wildlife objective is to understand the within-plot spatial responses of wading birds and waterfowl in relation to habitat structure (e.g., ridges, sloughs and plot edges), microtopography and hydrology. These data will not only improve understanding of relatively small-scale habitat responses of focal restoration species from a CERP perspective, but will also help guide future active marsh improvement efforts in terms of optimal plot shape, structure and size for wildlife use. From a vegetation perspective, this is more of a proof of concept, with within plot replication. However, treatment effectiveness will be compared by modeling the vegetation patterns created with those of adjacent areas, to assess whether a ridge and slough pattern is restored. Overall variability for many parameters will be obtained from data collected in CHIP control plots.

Comment #6: *The carbon dynamics work is strong, and the results are compelling. To complete the balance, an estimate of dissolved organic carbon loss would be good, and a standard ecosystem flux diagram might help visualize the overall budget.*

Response #6: Author Response – This empirically-based model includes some of the aggregated processes involved in the cycling of C dynamics in tree islands. Dissolved organic carbon compounds leaching from the soil profile might contribute to the loss of soil carbon in an ecosystem such as the Florida Everglades which is known to have typically rich dissolved organic carbon waters. An empirical analysis would be improved if dissolved organic carbon is estimated; at this point we have some evidence from groundwater chemistry data, almost all in concentration of ions (Sullivan et al., 2012) but it has not been incorporated in our model.

--The conceptual diagram of C cycling used in LILA tree islands has been included.

Comment #7: *In the sediment modeling work I cannot help but wonder about the effects of bioturbation on sediment suspension and movement. For example during dry periods, crayfish move in and are concentrated and birds actively feed on them. Could this be a time of increased sediment transport?*

Response #7: Author Response – Bioturbation and periods of increased/decreased sedimentation during dry/wet conditions might occur, but because we measure net changes in sediment accretion on annual basis, we assume that the model reflects temporal variations in the tree islands. Previous studies have demonstrated that flowing (~0.5 cm s^{-1}) vs no-flow LILA macrocosms did not drive differences in litter decomposition (Serna et al., 2013).

Comment #8: *The plant-salinity monitoring makes sense, but more years of data are needed here to firm up results.*

Response #8: Author Response – If this refers to C111 monitoring of soil salinity and plant growth, which is not presented in detail but may have been mentioned as a program, the response would be: "agreed. Data collection is ongoing."

Comment #9: *It is difficult to evaluate the output from the "Simulation Modeling to Evaluate CEPP Alternatives and Effects on Florida Bay Submerged Aquatic Vegetation" with the provided information (citation not in reference list).*

Response #9: Author Response – Add text to line 1825: "SEACOM (Madden and McDonald, 2010; Madden, 2013) outputs biomass and species composition for the three dominant seagrasses in northern Florida Bay."

Add refs:

Madden, C. J. and A. A. McDonald. 2010. Seagrass Ecosystem Assessment and Community Organization Model (SEACOM), A Seagrass Model for Florida Bay: Examination of Fresh Water Effects on Seagrass Ecological Processes, Community Dynamics and Seagrass Die-off. South Florida Water Management District, West Palm Beach, FL. 120 pp.

Madden, C. J. 2013. Use of models in ecosystem-based management of the southern Everglades and Florida Bay, Florida. In: J. W. Day, Jr. and A. Yañez-Arancibia [eds.] The Gulf of Mexico: Its Origins, Waters, Biota and Human Impacts; V. 5 Ecosystem Based Management. Harte Research Institute for Gulf of Mexico Studies, Texas A & M University-Corpus Christi, Texas A&M University Press, College Station, TX. (2013). 460 pp.

Comment #10: *Page 6-2 The third ecosystem project is?*

Response #10: Author Response – Changed to: "Two ecosystem-scale projects are reported…"

Comment #11: *Line 188. Better and worse means for wading bird success? Maybe it would be better to use more neutral terms, as they seem like value judgments.*

Response #11: Author Response – Perhaps use "drier" instead of "worse"?

Comment #12: *Line 209 ' It is believed that…" Should be stated that tree islands may require some dry periods for survival, rather than something is "good" for tree islands.*

Response #12: Author Response – Change lines 207-209 to: Although 1–2 months is not considered enough time to cause any long-lasting tree island damage (Wu et al., 2002), it is believed that tree islands may require some dry periods for survival (Heisler et al., 2002).

Comment #13: *Line 244 something is missing.*

Response #13: Author Response – Change line-243-244 to: In the northeastern region of WCA-3A (gage 63), the WY2013 wet-season began early and water levels rose above the tolerance of tree islands for some 100 day (Figure 6-4).

Comment #14: *Line 286. Amusing comment but probably should be removed.*

Response #14: Author Response – Agreed.

Change from this: This year, it appears that the prey stock had time and water to get relatively rejuvenated (there is that word relatively again).

To this: This year, it appears that the prey stock had time and water to get relatively rejuvenated.

Comment #15: *Fig 6-13 which of the sets of mesocosms is this for, or is it a composite of M1-M4?*

Response #15: Author Response – The graphs are of the combined mean ground water values for all the tree islands (peat versus limestone) normalized to surface water levels. Clarification has been added to the figure caption.

Comment #16: *Figure 6-14 Is there any way to get normalize the number of nests with the total population number? This might tell a slightly different story.*

Response #16: Author Response – The goal of Figure 6.14 is simply to provide context to the summary of the nesting year and is not meant to relate to specific performance measures of Everglades restoration. Even if that were the case, I'm not sure that normalizing in relation to a current total population (which is difficult to determine and not necessarily relevant in an Everglades restoration context) is appropriate here. A series of wading bird PMs have been developed and refined for Everglades restoration, all based on historical conditions and thought to be representative of key ecological features of the bird-prey-hydrology relationship (Frederick et al. 2009). In this respect, we did include the PM for the 3-year running average of the numbers of nesting pairs of key avian species in the mainland Everglades (see table 6.3). Using averages in this way reduces the natural inter-year variability in nesting effort that is characteristic of wading bird nesting, and it provides a useful inter-year comparison of nesting effort for each species in relation to a specific restoration target. We can provide these data as a figure rather than the current one if need be.

Other than that, the closest PM to a normalized score is the "Return to an interval between exceptional white ibis nesting events, defined as greater than the 70th percentile of annual nest numbers for the period of record." The interval between large ibis nestings in the predrainage period was 1.6 years and this serves as the target for restoration. This measure has improved very markedly since the 1970s, with the target achieved in 9 of the last 10 years. The 2013 ibis nesting did reach the criterion, and the interval averaged over the last five years is 1.6 years, almost exactly the same as in the 1930s. We have added this information to the report.

Comment #17: *Line 618. A significant portion statistically or a substantial proportion. How about a range of biomass?*

Response #17: Author Response – We've changed 'significant' to 'substantial" and added information on crayfish biomass.

Comment #18: *Table 6-4 Italicize species names*

Response #18: Author Response – will be addressed in final edits.

Comment #19: Table 6-6. With the replication, can't the variance in responses be reported on this table?

Response #19: Author Response – Data assessments are categorical so do not have typical variances associated with them. We will look into other options to provide an estimate of variability.

Comment #20: *Line 1264. Maybe say it was marginally significant? $P < 0.05$ is an arbitrary value or say it was less but not quite at the $p < 0.05$ level.*

Response #20: Author Response – Figure 6-24 Mean litterfall….. --Macrocosm M3 year of planting has been fixed.

"…point-form summary of the key findings/conclusions at the end of each section in addition to, or integrated with, the paragraph on relevance to water management" --It has been included.

Comment #21: *Line 1417. Is one month a year often enough for particulate P collections?*

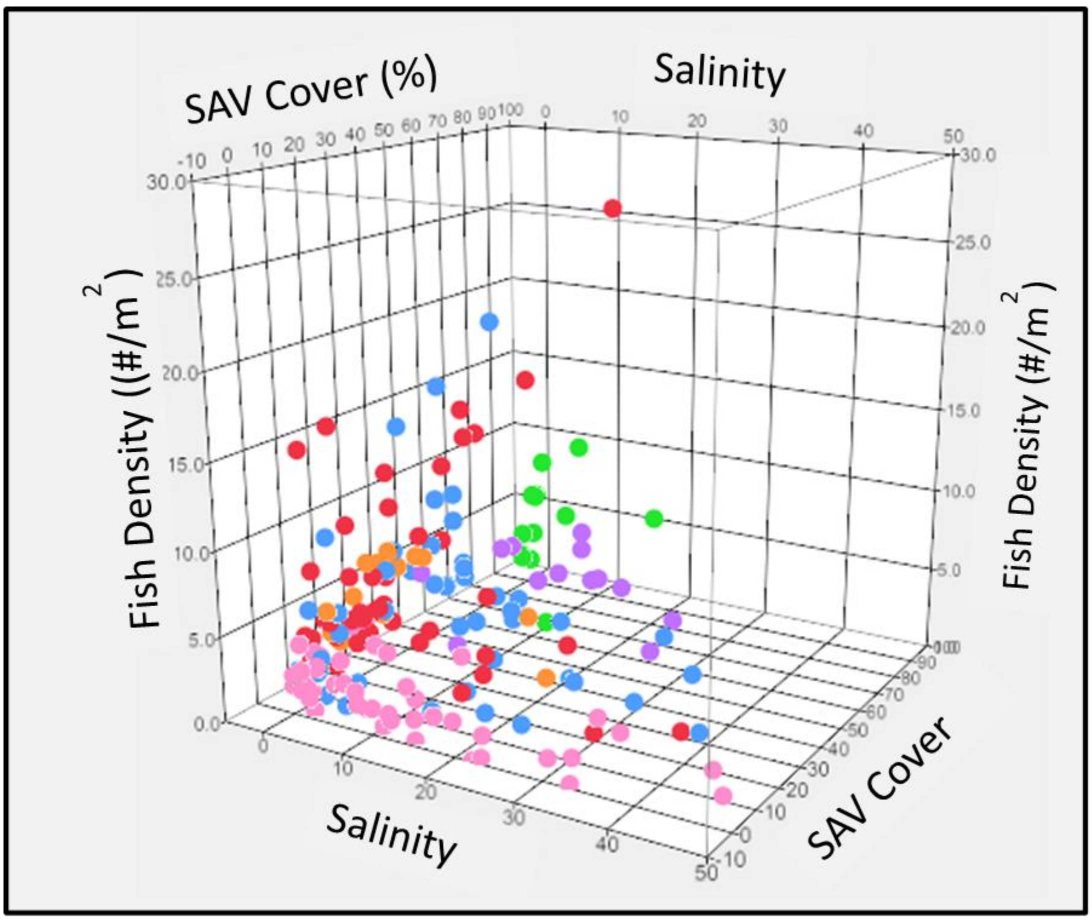
Response #21: Author Response – The BACI sampling requires comparing only data collected within the same operating window of Nov-Dec of the "before" years (2010-2012) and "impact" (2013, 2014); therefore, sampling multiple months outside of the operational window cannot be used for the statistical analysis. Moreover, in the before-impact period (i.e., 2010-2012 sampling), with the exception of DB sites in 2010, the particulate concentrations measured among years, and among habitats, indicate very low sediment particulate concentrations (<1.5 mg/L) at all times. Therefore, given the limited variability observed even between years, there is no reason to assume values measured in the before-flow period need to be refined by measuring additional days in the short operational window; thus, it is difficult to justify added sampling. However, when flow velocities are increased to entrain sediments during this year's flow event, we expect particulates to increase several-fold to order-of-magnitude or more. This year we will measure particulates over 2 periods within the operational window and also by daily autosampler collections. It is likely particulate concentrations will decline over the experimental flow period as flow rates decline due to tailwater build-up downstream of the culvert.

Comment #22: *Line 1551 Sentence incomplete.*

Response #22: Author Response – Change to: "An ongoing research program has focused on the effects of the rule on ecological conditions in the mangrove ecotone and the open waters of Florida Bay. The research has particularly addressed how patterns of hydrology and salinity affect the density, spatial extent and recruitment of SAV communities that form critical habitat in those areas."

Comment #23: *Figure 6-35 is pretty difficult to interpret*

Response #23: Author Response – Replacement figure for 6-35



Comment #24: *Figure 6-38 Include error bars? Separate graphs if needed.*

Response #24: Author Response – This is an excellent recommendation and will be explored for next year's SFER.

Comment #25: *Figure 6-40. These are some nice plots to help visualize what is going on.*

Response #25: Author Response – Thank you, we agree.

Comment #26: *Line 1775. This is data not shown, right?*

Response #26: Author Response – Correct. Text will be added to indicate this.

Comment #27: *The overview (table 6.1) is very useful. An addition that would help the reader assimilate the large amount of information in this chapter would be a very brief, point-form summary of the key findings/conclusions at the end of each section in addition to, or integrated with, the paragraph on relevance to water management.*

Response #27: Author Response – This is an excellent suggestion and we will include it in our 2015 SFER if the general editors think it is appropriate and if space allows.

Comment #28: *l. 28 – it would useful to provide relative amounts in addition to absolute numbers for figures such as this – without hunting around, I don't know how much of a relative change 12,232 acre-feet is, or even whether it is significant*

Response #28: Author Response – Average cumulative annual flow from the five creeks for WY1997-2011 is 255,893 acre-ft. Flow for May-September of WY2013 was 268,125 acre-feet (where the 12,232 acre-feet difference came from) while total annual flow for WY2013 was 358,235 acre-feet making FY2103 flows nearly 40% higher than the average. It was not, however, the highest flows on record. That occurred in 2006 with 401,440 acre-feet from the five creeks. Text will be changed to include the total average annual flow number: "Cumulative flow from the five creeks discharging to eastern and central Florida Bay for the first 5 months of WY2013 (May-September) was 12,232 acre-feet greater than the average annual total from WY1997-WY2011 of 255,893 acre-feet."

Comment #29*: l. 90 - and the third study is?*

Response #29: Author Response – Changed to "Two ecosystem-scale projects are reported…"

Comment #30: *l. 138 – this section needs some editing; it is written in a colloquial style unsuitable for a scientific report*

Response #30: Author Response – Below is an edited version of lines 138-184.

The amount of rain in the Everglades Protection Area (EPA) for Water Year 2013 (WY13) (May 1, 2012 through April 30, 2013) was 3" to 9" more than last year and significantly above average historic conditions (**Table 6-2).** In Everglades National Park (the Park), the rainfall was 3.3 inches more (6.0%) than the historical average, and 4.7 inches more (8.5%) than last year. Water Conservation Areas WCA-1 and WCA-2 experienced the largest deviations from the historical average. Rainfall in WCA-2 and 3 was 10.8 inches (20.9%) above the historic average and 8.9 inches (16.5%) more than last year. The rainfall in WCA-3 was 3.3 inches more (5.9%) than the historical average and was 4.7 inches more (8.7%) than last year.

Table 6-2 Average, minimum, and maximum stage [feet National Geodetic Vertical Datum (ft NGVD)] and total annual rainfall (inches) for Water Year 2013 (WY13) in comparison to historic stage and rainfall.[1] (Average depths calculated by subtracting elevation from stage.)

Area	WY2013 Rainfall	Historic Rainfall	WY2013 Stage Mean (min; max)	Historic Stage Mean (min; max)	Elevation
WCA-1	62.80	51.96	16.49 (15.50; 17.35)	15.65 (10.0; 18.16)	15.1
WCA-2	62.80	51.96	12.45 (11.48; 14.43)	12.51 (9.33; 15.64)	11.2
WCA-3	56.15	51.37	10.24 (9.18; 11.64)	9.57 (4.78; 12.79)	8.2
ENP	58.49	55.22	6.52 (5.59; 7.38)	5.99 (2.01; 8.08)	5.1

[1]See Chapter 2 of this volume for a more detailed description of rain, stage, inflows, outflows, and historic databases.

Above average rainfall translated into above average stage throughout the Greater Everglades, except in WCA-2A (**Table 6-2**), where WY13 was a slight 0.06 ft lower than the historic mean. Average WY13 depth above the historic mean was 0.84 ft, 0.67 ft, and 0.53 ft, for WCA-1, WCA-3 and the ENP, respectively, for an average depth of 1.4 ft, 2.04 ft and 1.42 ft, respectively. For the entire Greater Everglades, this above average stage was due to an above average minimum and not an above average maximum.

Last year, the analysis of the ecohydrology was based upon four water-years to highlight a unique time-series pattern of two droughts and to discuss the ecological implications of a drought-wet-drought-wet sequence on the restoration of wading birds. This year, the analysis returned to an evaluation based upon the last three years. To review: the WY09 drought supported many species of wading birds (prey density was high and no reversal during the dry season), Then, it was followed in WY10 by low wading bird population densities due to flooding, and low populations again in WY11 because the WY11 drought was extreme (see **Figures 6-1 to 6-7**). Water Year 2012 started out looking good for wading birds, but was another bad year because prey abundance was low and because wet season water levels rose very quickly; cutting short the fledgling period.

The following hydropattern figures highlight the average stage changes in each of the WCAs for the last three years in relation to the recent historic averages, flooding tolerances for tree islands, drought tolerances for wetland peat, and recession rates and depths that support both nesting initiation and foraging success by wading birds. These indices are used by the District to facilitate weekly operational discussions and decisions. Tree island flooding tolerances are considered exceeded when depths on the islands are greater than 1 foot for more than 120 days (Wu *et al*., 2002). Drought tolerances are considered exceeded when water levels are greater than 1 foot below ground for more than 30 days, i.e., the criteria for Minimum Flows and Levels in the Everglades (SFWMD, 2003). **Figures 6-1** through **6-7** show the ground elevations in the WCAs as being essentially the same as the threshold for peat conservation.

The wading bird nesting period is divided into three simple categories (red, yellow, and green) based upon foraging observations in the Everglades (Gawlik, 2002). A red label indicates poor conditions due to recession rates that are too fast (greater than 0.6 foot per week) or too slow (less than 0.04 foot for more than two weeks). A red label is also given when the average depth change for the week is positive rather than negative. A yellow label indicates fair conditions due poor foraging depths (i.e., depths greater than 1.5 ft), or slow recession rate of 0.04 foot for a week, or rapid recessions between 0.17 foot and 0.6 foot per week. A green/"good" label is assigned when water depth decreased between 0.05 foot and 0.16 foot per week and water depths are between 0.1 - 1.5 ft.

Comment #31: *l. 187 – this section also needs editing for grammatical errors*

Response #31: Author Response – Thanks. This section will be sent back to the general editor for corrections.

Comment #32: *l. 247 – it is mentioned frequently that recession rates are a controlling factor in foraging but I can't recall anywhere where the science behind this is presented or referenced. Is this so widely known that it goes without saying?*

Response #32: Author Response – Here are two references that quantify nesting and foraging responses to recession rates:

Beerens J.M., Gawlik D. E., Herring G. & Cook M. I. (2011) Dynamic habitat selection by two wading birds species with divergent foraging strategies in a seasonally fluctuating wetland. The Auk, 128, 1–12.

Herring G., Gawlik D.E., Cook M.I. & Beerens J.M. (2010) Sensitivity of nesting Great Egrets (Ardea alba) and White Ibises (Eudocimus albus) to reduced prey availability. The Auk, 127, 660−670.

Comment #33: *l. 333 – same comment as line 26*

Response #33: Author Response – See response to comment #28. Text will be changed to "The cumulative flow from the five creeks discharging to eastern and central Florida Bay for the first 5 months of WY2013 (May-September) was 12,232 acre-feet greater than the average total for the entire year from WY1997-WY2011 which is 255,893 acre-feet."

Comment #34: *l. 364 – the work in this section is interesting but the analysis is somewhat weaker than that in other sections*

Response #34: Author Response – This report serves as an update to the results reported in:

Sullivan, P.L., R.M. Price, M.S. Ross, L.J. Scinto, S.L. Stoffella, E. Cline, T.W. Dreschel, F.H. Sklar.2010. Hydrologic Processes of Tree Islands in the Everglades: Tracking the Effects of Tree Establishment and Growth. *Hydrology Journal* DOI 10.1007/s10040-010-0691-0, 12 pp.

Comment #35: *l. 511 – I complained about significant figures last year and I will again despite the authors' statements about different error levels in different agency's surveys. It should still be possible to make an educated guess at the accuracy of these counts – are they likely good to the nearest thousand, or to the nearest ten?*

Response #35: Author Response – We agree that this is a valid point, and we would certainly like to see error estimates associated with these counts. However, these data are not collected or funded by the District and are thus beyond our influence in this respect. More importantly, the researcher involved (Peter Frederick from University of Florida), while currently working hard to address the problem, has not yet achieved a workable strategy for deriving a reasonable error of the estimate. Below we discuss in detail (with Dr. Frederick's input) some of the issues involved and the current efforts to address them. When considering this, bear in mind the detectability issues associated with a large landscape, comprising hundreds of colonies that vary markedly in physical characteristics (e.g. size, shape, vegetation type) and nesting responses (from a few nests to tens of thousands, single or mixed species, dark vs. white species, ground vs. tree nesters, and nesting that can start anytime from December to June).

There are several kinds of measurement error that are probably additive or even multiplicative.

1) Observer counting error – the variation among and within observers in counting nests that are plainly visible. This is relatively easy to quantify using double observer approaches and photographs. Work on this is ongoing but there are not yet enough years to work with yet.

2) Visibility bias – not seeing all the nests that are present because many are occluded by vegetation or angle of viewing. This can be measured with some labor at any individual colony; the error is substantial but perhaps not overwhelming – 11 – 23% underestimate on average for white ibis and great egret respectively, but hard to do with much confidence for the hundreds of colonies over the entire ecosystem. The most important number here from a restoration performance measure perspective is understanding the variation in bias from colony to colony (i.e. are birds moving to the coast?) and from year to year (i.e. is restoration improving nesting effort?); unfortunately much of the variation in visibility error derives from location AND year. Moreover, error estimates have not even been measured yet for some of the more difficult species to count, like the snowy egret.

3) Mis-estimation of the population of nest starts – this derives primarily from not detecting nests that start and stop between surveys, and from confusing failed/finished nests with new nests. While this is not a contributor to the estimate of true counts on any given survey, it is an enormous contributor to overall error in estimating the true number of nest starts in any season. According to estimates from Williams, Frederick & Nichols (2011), current counts may be underestimating the numbers of nest starts by 47 – 380%. Note that this is totally independent of the other sources of error, and appears to be highly dependent upon year and colony. Thus one cannot derive correction factors that apply to more than one nesting season or perhaps even more than one colony.

If the interest is in the numbers of breeding birds rather than number of clutches –then #3 takes on less meaning, since many birds may be nesting more than once per season, especially if nest success is generally poor. But even if numbers of breeding adults is the primary focus, there is still no way to estimate that (one would need to know the average number of times a pair initiates a nest per season, which is hard info to come by). It is also clear that the metric that has traditionally been used (nest starts) has a lot of hidden variation in it, and that it takes a huge amount of effort to control for and may or may not be of biological interest. This has been called nesting effort and it has been thought of as a measure of the quality of nesting conditions during any given nesting season. Yet if more nest starts are a result of previous nest failures, the definition of "quality" is muddied.

The key contributor to confidence in estimating nest starts (nest turnover within a colony) can only be estimated by following individual nests through time – and in particular throughout the season. This is fairly easy for small colonies but of course difficult in large ones (1000 -10,000+ nests) that have few landmarks, particularly where birds are nesting densely. This has led to trying to use robotic aircraft (UAVs) that can take high resolution photos that are also accurately georeferenced. This essentially allows for "marking" or at least geoidentification of large number of individual nests and with sequential flights, following them through time to produce turnover estimates and eventually an estimate of the seasonal numbers of nest starts. It also provides nest success though for various operational reasons it is not that accurate particularly once the eggs have hatched. This looked really promising until the FAA prevented the use of UAVs to fly within the MIA Traffic control area. Frederick's group is currently experimenting with putting the "guts" of the unmanned craft on a Cessna wing strut. This will only be possible using a better resolution camera to fly higher and therefore to avoid the need for extremely accurate transect tracks (robots are more accurate than real pilots).

If this last step can be completed, then it will be possible to measure error all along the way – observer bias, visibility bias, and mis-estimation of nest starts. However, this will involve a lot more effort: putting marked quadrats down in each colony, and flying many more dense transects around some colonies. There will also be considerable effort involved in post-processing. This in turn will require additional dollars during this time of fiscal reality. Nonetheless, if this can be done for the big colonies in any year, this will certainly help provide error estimations in the future.

This then brings up a new problem: how to compare the new data with the old (and especially historical) data. On the face of it this is easy: do both techniques at the same colonies and years and look at the error. However, we already know that the error is primarily generated by year and colony effects, so we are bound to see some extremely high variances in this comparison; so much that it may not be possible to hindcast. This is not a reason not to start measuring error, but given the cost involved there needs to be a justification for how we are going to compare with past estimates.

Williams, K A., P.C. Frederick & JD Nichols (2011) Use of the superpopulation approach to estimate breeding population size: an example in asynchronously breeding birds. Ecology 92:821-828.

Comment #36: *l. 1102 and elsewhere – the use of large quantities of herbicide warrant some discussion of the possible (or observed) effects on non-target organisms (other than other vegetation). Are the chemicals bioaccumulating? toxic to non-plant life? This may be covered in other chapter s or appendices and if so should be referred to here.*

Response #36: Author Response – The accumulation of arsenal and glyphosate and their potential impact on non-target aquatic species were assessed when these herbicides were used in the CHIP plots. Nuclear magnetic resonance methods were used to evaluate the accumulation of the breakdown products in the soil- there was no evidence of accumulation. Similarly concentrations determined in the water column and on fish were below concentrations known to cause any damage. As these products have not been used in these studies since 2007, no additional justification is provided here. As far as the use of Clearcast, the justification for the use of this herbicide and its low risk is provided in the Rodgers and Black citation, this will be more clearly acknowledged in the text.

Comment #37: *l. 1487 – I'm glad this was recognized – it's an almost universal problem with sediment traps in my experience, with efficiencies of <10% in high flow areas*

Response #37: Author Response – We concur with this comment. Further, we are pursuing controlled flume tests to evaluate trap biases.

Comment #38: *l. 1719 – are blue-green algae an issue here at all?*

Response #38: Author Response – Yes. As reported in the 2007 SFER (V1, Ch12), algal communities associated with the eastern Bay bloom that started in WY2006 and the blooms in the central Bay are "composed chiefly of cyanobacteria, primarily in genus *Synechocystis* and *Synechococcus* (several species, including *elongatus*)."

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 7

LeRoy Rodgers, David Black,
Mike Bodle and Francois LaRoche

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

WebBoard-Posted: 10/21/13

Comment #1: *The Melaleuca results are promising as are some other biocontrol efforts. Hopefully the biocontrol agents will not expand their host range.*

Response #1: Substantial scientific effort has gone into minimizing risks of damage from expanded host range. However, you are right to suggest that a certain risk remains; we share your hope. Damage from not controlling the species is certain. Chemical control is very expensive and also presents risk.

Comment #2: *The vegetation mapping data (and all other data) section should probably contain links to the actual data and statements on compliance with Ecological Metadata Language. In addition, statements on alternative repositories for data would be useful. There is a tremendous amount of ecological information here, and it should be archived for the future in more than just SFWMD data.*

Response #2: Although most District data are not currently stored in files with metadata that conform to Ecological Metadata Language specifications, that situation is changing. Scientific and monitoring staff are learning to use Morpho to generate EML compliant metadata files. At this time much District data is not organized and documented in a way that would make it readily useable to others. Our team is looking into the feasibility of using EML for the vegetation mapping data and will include links to the current data repository.

Comment #3: *In figures 7-5 to 7-8 it would be good to note what a "high" or a "low" level of cover actually is, in terms of % of community dominance, stems per square meter or some other measure.*

Response #3: Figures will be modified to make the range of percent cover values more evident.

Comment #4: *Line 485 Is the Florida Invasive Species Partnership also involving tribes?*

Response #4: Tribes have not been signatory to the Florida Invasive Species Partnership; however there is an open invitation to tribes and other interested parties to participate.

Comment #5: *I appreciate the places with the links embedded in this chapter to follow up for further information, and would recommend doing this throughout.*

Response #5: We accept your recommendation and will embed links to additional information.

Comment #6: *Line 735 Be specific that Brazilian pepper is fire positive.*

Response #6: Document wording will be changed.

Comment #7: *Line 770 top 10 according to who?*

Response #7: Citation will be added.

Comment #8: *Line 802. Is this published data or a personal communication?*

Response #8: Citation will be added.

Comment #9: *Line 994. Citation?*

Response #9: Citation will be added.

Comment #10: *Line 1042. Interesting case of conflicting management outcomes.*

Response #10: Indeed!

Comment #11: *Fig 7-31. Scale?*

Response #11: Length of tegu will be added to the caption.

Comment #12: *Line 1408 Don't forget to update number.*

Response #12: Thank you.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 8

Lesley Bertolotti, Joyce Zhang and Bruce Sharfstein

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

WebBoard-Posted: 10/24/13

Comment #1: *The chapter is well-written, clear and concise. Again, the conclusions drawn are well-supported by the data presented. In general, the writers have produced an excellent summary of the ongoing work on Lake Okeechobee and its watershed.*

Response #1: Comment appreciated.

Comment #2: *As stated last year, the greatest concern related to this section remains the progress, or lack of it, made towards reducing the TP to the target value of 140 metric tons/year. Despite very substantial efforts to reduce loads, many of which have had significant success, and many of which have cost large amounts, the total phosphorus load is in the same ballpark as it has been for the past several years, in fact it increased substantially in the WY2013. This also led to an increase in the in-lake TP concentration. As was indicated in last year's review of this chapter, a path to meeting this target load needs to be described in detail and realistic scenarios for the future need to be developed and presented. The potential for very large internal P loads makes the desired changes in water quality even more difficult; however, on the positive side, notable progress is reported this year in terms of experimentation with methods designed to prevent release of sediment P.*

Response #2: For more than two decades, a suite of phosphorus reduction projects has been implemented to improve the water quality and hydrology of the Lake Okeechobee Watershed. TP loads from the Taylor Creek and Nubbin Slough Sub-watershed have shown decreasing trend from 2001 to 2009 (SFWMD et al., 2011). This sub-watershed included basins that were identified as priority basins for nutrient reduction efforts. Although not statistically significant and attributed more to changes in flow than TP concentrations, several sub-watersheds have also displayed decreasing trends in TP loading (SFWMD et al., 2011). However, total phosphorus loading to the lake has not exhibited a significant trend and is still well above the TMDL. This could be due to legacy phosphorus in the watershed, which delays in the response time of these efforts. Recent scientific information with regard to significant amounts of legacy phosphorus in the watershed and in Lake Okeechobee suggests that achievement of the TMDL will be a long-term process. The path to meeting the TMDL will be prescribed through the iterative process of FDEP's Basin Management Action Plan (BMAP) being developed for the Lake Okeechobee Watershed, which builds upon the decades of work already done in the watershed including the Lake Okeechobee Watershed Protection Plan.

Comment #3: *Again, more intensive efforts to quantify the nutrient load-trophic status response relationships through modeling are encouraged so that expectations with different target loads can be identified.*

Response #3: We appreciate the suggestion and will consider it as a future item for the Research and Water Quality Monitoring Plan.

Comment #4: *The Stormwater Treatment Area (STA) projects appear to be proceeding better than in the past. Implementation has and will lead to significant reductions in P leading to the lake. Incorporation of HWTT technology is an excellent idea and clearly (figure 8.7) shows much promise for additional reductions in load. The use of this technology should be expanded wherever possible.*

Response #4: Comment appreciated. The expansion of HWTT technology is subject to funding availability.

Comment #5: *The modeling remains problematic; the hydrology is reasonable but the P modeling, despite considerable "tweaking", is unsatisfactory. It is suggested that the modelers look into the INCA modeling framework developed at U. Reading and Oxford (UK). The original INCA was designed to model N flux through and out of watersheds while INCA-P does the same for P. Although these models were developed for the UK, they have been used in many countries including Canada, and have proven to be extremely useful. My understanding of the data available for the Everglades system leads me to think that INCA is ideal for this situation. It is very good at looking at source areas, evaluating the potential effects of BMP's, quantifying the links between climate, hydrology and nutrient flux. We have modified INCA-P to use in Canada on a large (725 km2) shallow lake, as well as all of its tributaries. There are many papers in the literature on INCA; if there is interest in following up on this, I can direct staff to appropriate papers.*

Response #5: We appreciate your thought and recommendation. The Integrated Catchment Model (INCA) (http://www.reading.ac.uk/geographyandenvironmentalscience/research/INCA/) is new to us and it would be nice to see if it works better for the Lake Okeechobee watershed than the WAM model we currently use. In April 2009, a Panel of five experts completed a peer-review of WAM (Graham et al., 2009). The following paragraph is obtained from the panel report "The Panel believes that WAM is capable of simulating the relative effect of alternative land use and management practices on surface and subsurface hydrology and pollutant loads, on a watershed scale. It has the flexibility necessary to consider upland landscapes with deep water tables, landscapes with **shallow water tables,** with and without artificial drainage, and special cases, such as wetlands, urban areas and mining sites." We want to emphasize that WAM works under both deep and shallow water table conditions. The shallow water table condition, flat topography, and sandy soils are unique characteristics for South Florida landscape, and many other models are not suitable under these conditions.

Comment #6: *It is good to see the thought put into the Strategies For Moving Forward section and the discussion on key activities. The reader is left with the thought that the target load will be very difficult to achieve but perhaps not impossible.*

Response #6: Comment appreciated.

Comment #7: *Finally, this is one of the largest and best coordinated sets of environmental monitoring and research experiment networks in the United States and perhaps the world. Making certain these data are available online in a stable source outside SFWMD would help ensure this work has a lasting mark. Links to the actual data in future versions could facilitate the process of making the data more obvious to outside parties. It could be useful for expert workshops to tie all this together from a science point of view. This may require external funding, but the background information is present for so many levels of ecological hierarchy, and the obvious next step is to work on the big picture and any potential emergent properties that might occur in such a large and complex system.*

Response #7: We strongly agree. Conducting an expert workshop to synthesize data and present findings is valuable to the District. We will share this thought with our coordinating agencies.

Comment #8*: line 80 – the same paragraph expresses load in metric tons, followed by areal load in pounds per acre and concentrations in ug/L. It is time to be consistent with units. Since you already calculate loads in mt and concentrations in ug/L or mg/L, I suggest doing away with pounds/acre, acre-feet and other obsolete units*

Response #8: The choice of units considers the regional stakeholder audience as well as the potential international audience. A "Units of Measurement page" with US and SI unit conversions for commonly used units in the SFER will be included as front matter in the final 2014 SFER version.

Comment #9: *line 137 - blue green algae is an old fashioned term, cyanobacteria should be used*

Response #9: The authors agree and will change blue green algae to cyanobacteria.

Comment #10: *line 147 - better indicates a value judgment. Higher or greater? Better food for wading birds?*

Response #10: The term better was used to indicate that the prey base was more in line with historical levels, but the authors have no objection to substituting the word greater for better.

Comment #11: *line 155 – the summary says mean depth is 9 feet; this says 7.5. In any case use m.*

Response #11: It should be 9 feet (2.7 m). Revisions will be made.

Comment #12: *Table 8.1 - I would round off these areas – they can't be accurate to 7 or 7 figures; 4 maybe but even that is unlikely.*

Response #12: These values, along with the land use acreages presented in Table 8-2 were generated with ArcGIS tool, which provides reliable results with today's technology.

Comment #13: *line 360 – work described in chapter 6 seemed to demonstrate that SAV trapped nutrients better than emergent vegetation; why was the focus here not on establishing SAV?*

Response #13: The STAs north of the lake are very different from the STAs south of the lake (i.e. STA1W) in terms of soil properties, plant communities, surface water TP concentrations, rainfall patterns and hydraulic loads. The north STAs were designed to remove P loads to the maximum extent possible whereas the south Everglades STAs were designed and managed to achieve a mandated outflow TP concentration. The emergent vegetation will provide greater reduction at the higher TP concentration condition. SAVs are typically being used in the lower reaches of the treatment train to achieve a target goal.

Comment #14: *line 383 – the HWTT approach seems to be a good one; what coagulants were used here?*

Response #14: To make it clear, we will add a paragraph to the final document: "During WY2013, in addition to alum, a polyaluminum Chloride compound was used at two facilities as a supplemental coagulant. Water chemistry at Grassy and Mosquito Creek HWTT sites was different during parts of the rainy season when compared with the other HWTT sites. Alkalinity in the drainage waters from these two sites was low (<60 mg/L) and pH was close to neutral (>6.5). Under these circumstances of low alkalinity and circumneutral pH, the limerock beds at these facilities were not able to supply the needed alkalinity for a better floc formation with alum. Polyaluminum Chloride is a supplemental coagulant that can be used in conjunction with alum during these periods to improve flocculation and reduce total P and total Al export from these HWTT facilities."

Comment #15: *Figure 8-7 - needs better figure legend, more descriptive and more complete y axis label*

Response #15: Figure 8-7 will be revised.

Comment #16: *line 1209 – why the switch to TP in mg/L from ug/L; surely accuracy is better than in 10's of µg as indicated by 0.02 mg/L*

Response #16: Agreed and changes will be made.

Comment #17: *line 1357 – figure 8-14; why were there no mid-lake station used for the water clarity measurements, just nearshore stations?*

Response #17: The intent was to try and keep this data as comparable to our previous approach to calculating transparency as possible. Previously, the data for this metric came from Secchi depths collected as part of our regular SAV mapping program, and those were all nearshore sites; as that is where SAV is located in Lake Okeechobee.

Comment #18: *line 1668 - it could be that diatoms are sensitive to allelopathic compounds produced by components of the SAV. It looks like they were low at the same time that the diatom/ cyanobacteria ratio was high.*

Response #18: Yes, certainly another possible explanation for our results.

RESPONSES TO PUBLIC COMMENTS ON DRAFT VOLUME I, CHAPTER 8

Coordinating Agencies' Responses to Audubon Florida Comments in 11/6/2013 Letter

Comment #1: Explain how the listed key activities moving forward will lead to meeting the TMDL. "Key Activities Moving Forward" on pgs. 8-103 to 8-109 of the 2014 LOPP Update lists a variety of actions, including but not limited to Lake Okeechobee Basin Management Action Plan development, the continued implementation of source control programs, and dispersed water management. However, the 2014 LOPP Update does not detail how these actions will lead to the necessary load reductions to meet the Lake Okeechobee TMDL. The plan should include information on how each of these actions, individually and cumulatively, will add to meeting the Lake's TMDL.

Response: Strategies for load reductions are included in the Strategies Moving Forward Section and were described in more detail, including load reductions both individually and cumulatively, in the LOWPP 2011 Update. It was recognized in the 2011 LOWPP Update that with full plan implementation there was a shortfall in meeting the TMDL and that additional watershed phosphorus reduction strategies were needed. The major new key strategy for achieving the Lake Okeechobee TMDL identified in the "Strategies Moving Forward" section is the development and implementation of the Lake Okeechobee Basin Management Action Plan. A BMAP is the "blueprint" for restoring impaired waters by reducing pollutant loadings to meet a total maximum daily load (TMDL). The LOWPP and Lake Okeechobee BMAP provide a reasonable means of achieving the TMDL requirements and achieving and maintaining state water quality standards. Working in partnership with the SFWMD and FDACS, the FDEP through the Lake Okeechobee BMAP development is currently building upon the projects in the LOWPP and working to identify and develop projects, estimate project specific and cumulative load reductions, and refine the near term project implementation plan.

The SFWMD and FDEP staff work together closely to streamline efforts between the LOWPP and the BMAP, following the direction of the Florida legislature and governor to maximize resources across the agencies and minimize duplicative work. One such streamlining effort is to continue to work closely with FDEP through their BMAP process and to adopt the projects and associated load reductions.

Comment #2: Include a timeline for implementation. The Northern Everglades and Estuaries Protection Plan authorizes and directs the coordinating agencies to establish priorities and implementation schedules for the achievement of total maximum daily loads. However, a timeline for the implementation of the "Key Activities Moving Forward" does not appear in the 2014 LOPP Update. Using terms such as "current", "near term" and "long term" is inadequate. Including a timeline for implementation is particularly important in light of the upcoming TMDL deadline of January 1, 2015. The 2014 LOPP Update must re-state the commitment of the State agencies to work toward meeting this deadline.

Response: The Lake Okeechobee Phase II Technical Plan included an implementation schedule for the preferred plan. This implementation schedule was also updated in the 2011 LOWPP Update. With inherent uncertainties associated with projects and strategies identified in the Plan such as federal authorizations and funding, it is not feasible to provide an overall implementation schedule more detailed than current, near-term and long-term. The BMAP is an iterative process and the first iteration will include projects to be completed in the initial five years. Once the first iteration of the BMAP is completed, the initial projects and any other implementation information therein, will be incorporated into the LOWPP. Please note that the anticipated status of near term activities has been included at the end of "Strategies Moving Forward" section.

Comment #3: Include funding requirements to implement the LOPP. Most importantly, the 2014 LOPP Update must clearly articulate funding requirements. Florida law states, "The Legislature finds that a continuing source of funding is needed to effectively implement the programs developed and approved under this section."10 Thus, it is incumbent on the drafters of the LOPP 2014 Update to explain what funding is required. In the 2011 LOPP update, the lack of funding was cited numerous times as a reason why programs and projects could not proceed as rapidly as desired. In this draft, there should be an explicit discussion of the funding requirements necessary to complete the plan. Table 8-20 on pg. 8-110 and 8-111 in the Draft 2014 LOPP update should be expanded to include funding needed to complete the plan. Funding requirements for the full implementation of best management practices throughout the watershed should be clearly articulated. In addition, funding for edge-of-farm retention and detention measures as well as needed Stormwater Treatment Areas (Lakeside Ranch- phase II) should be increased.

Response: The intent language of NEEPP cited in this comment contains an acknowledgement that a continued source of funding is needed to effectively implement the program. Please note that estimated funding requirements to implement the initial plan components were provided in the Lake Okeechobee Phase II Technical Plan and these were updated in the 2011 LOWPP Update. As in many cases, there is insufficient information to provide reliable cost estimates beyond the near-term projects/activities due to several factors including lack of project specific details and/or uncertainty in implementation dates. However, in recognition of this legislative intent cost estimates of near-term activities, additional funding information on the FDACS Agricultural BMP Program and the Dispersed Water Management Program have been included at the end of the "Strategies Moving Forward" section.

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 9

Bradley L. Jones, David J. Colangelo, David H. Anderson,
Stephen G. Bousquin and Michael D. Cheek

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

WebBoard-Posted: 10/24/13

Comment #1: *Line 345 – How certain is it that bouts of anoxia were not common historically before the hydrology was altered?*

Response #1: We are not certain about the pre-channelization occurrence of hypoxia because there is no time series of dissolved oxygen data prior to 1962 when the channelization project began. Dissolved oxygen is expected to improve based on comparisons to reference streams, which are discussed later in the chapter in the section on Dissolved Oxygen (see Expectation 8).

Comment #2: *Figure 9.7 – Usually rainfall is represented as daily bars as opposed to a line connecting points.*

Response #2: This figure has been revised to show daily bars.

Comment #3: *Figure 9.8 B – What is the frequency of dissolved oxygen measurements? These are daily means according to the text, but what is the measurement frequency? Also the words "daily mean" should also be included in the figure legend. Also Dodds seconds the call by Stein (A) reviewer to work out the mechanisms of low DO.*

Response #3: Dissolved oxygen is measured at 15-minute intervals. The figure legend has been revised to indicate that these are mean daily values calculated from 15-minute measurements. We are continuing to work out the mechanism for low DO (see response to Reviewer Stein's comment).

Comment #4: *Line 447 – At least one of the chapters is using SI units consistently! Except figure 9-16.*

Response #4: In Figure 9-16, discharge was shown in acre-feet because this unit of measurement is commonly used by water managers. It is also consistent with Tables 8-6 to 8-9 in Chapter 8, which list discharges and nutrient loads for the Kissimmee River and other Lake Okeechobee inflows. Lines 446-448 state that hydrologic responses are presented in SI units instead of English units, but this only refers to the Hydrologic section of the KRREP update. So, no change to the text is needed.

Comment #5: *Line 723 – It is also worth noting that the sub-basins most contributing to Okeechobee nutrients are those the closest to the lakes, implying some degree of retention is occurring.*

Response #5: The lower sub-basins, which contribute higher amounts of nutrients, have more intensive agriculture. In the text referenced in the comment, we are talking only about nutrient retention in the river floodplain, which is only a small portion of the drainage area of each sub-basin.

Comment #6: *Line 726 – I would like to see some measures of denitrification rates associated with various sediment types. The total nitrogen concentrations at the river sites are pretty high.*

Response #6: This is a good question that we have not yet considered. The nitrogen concentrations shown in Figure 9-20 are typical of ambient concentrations in this part of the Lake Okeechobee watershed. In fact, if Figure 9-20 is compared with the TN concentrations for lake tributaries listed in Tables 8-7 and 8-9, it is evident that the Kissimmee River data are near the low end of the range. We could investigate the literature to see if we can apply denitrification rates from similar wetlands, but a field study of denitrification in the Kissimmee River floodplain soils would be low priority.

Comment #7: *Line 847. Interesting observation. Was the areal herbicide to control invasive species?*

Response #7: Yes. At line 849, the underlined text will be inserted in the following sentence: "Prior to abandonment, many of the nests were being built in dead and dying shrubs, mostly wax myrtle (*Myrica cerifera*), with some red bay (*Persea borbonia*) and red maple (*Acer rubrum*), that were decomposing and collapsing as the result of an aerial herbicide treatment of invasive exotic vegetation sometime during 2011.

Comment #8: *Figure 9-24 – Given the variance over the years, it is pretty clear that the two baseline years were not near enough to establish changes over time from the restoration.*

Response #8: Yes, that is a weak point in the comparison of Before and After. Two years of data is certainly not ideal with this type of variable population. But the assumption is that habitat conditions hadn't changed significantly between the 1996-1998 Baseline surveys and the initial post-channelization surveys from 1972-1980 (ducks) and 1978-1980 (waders only) (Perrin et al. 1982). These surveys showed the rapid negative impact of channelization on both ducks (93% reduction) and waders (significant decline), so we assume the habitat did not degrade any further or improve significantly either after channelization.

Comment #9: *Line 1073. Good point.*

Response #9: Additional water quality data are being collected from the minor tributaries around Lake Tohopekaliga and East Lake Tohopekaliga. We are also looking into collecting flow data from some of these tributaries. Discharges, loads, and concentrations estimated with District's PN Budget Tool will be compared to estimates obtained through field measurements of water quality and flow.

Comment #10: *"Broad Questions and Comments" The chapter provides an overview of the current year's efforts to assess the success of activities designed to restore the ecological health of the Kissimmee River to a pre-channelization condition. The premise is that the ecological health of the river basin was severely compromised by channelization of the river into the C38 canal but that obliteration of the middle section of this canal and restoration of the original river channel will reconnect the restored channel to its associated floodplain in that section. This will in turn allow the associated assemblages of flora and fauna to revert to something similar to the pre-channelized basin. In addition to activities associated with this major restoration project, other topics such as the restoration and overall health of some of the upper lakes region of the basin are provided.*

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

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

Response #10: The selection of years sampled by KRREP studies in the interim period (after Phase I construction but prior to KRRP completion) is driven by the specific needs of individual studies, and considers competing project demands to allow reallocation of limited resources among different studies in different years. Most interim data collection has already been completed to align monitoring plans with previously-projected construction completion schedules, which were recently extended. Most KRREP studies either collect data during the interim period for only a brief time (2-3 years) or at some interval less often than annually (e.g., 3-5 years); or do not collect interim data at all. As shown in Table 9-3, most studies requiring floodplain inundation for sampling are not collecting any data in the interim period. For the few studies being conducted annually in the interim period, most (hydrology, DO, nutrients) use data that are collected routinely or by automated data loggers. The major updates shown in Table 9-3 (page numbers bracketed and bolded) are primarily for (a) hydrologic and water quality data (both of which use data that are collected routinely by the District, i.e. are not collected specifically for restoration evaluation) OR (b) various river channel studies (e.g., geomorphology, turbidity, DO, river channel vegetation, and river channel invertebrates and fish). Because river channel hydrologic conditions are dominated by restoration of flow, which has been nearly continuous since 2001, most interim monitoring of river channel metrics have in fact documented substantive positive responses since Phase I following the trajectories predicted by their expectations; many of these expectations have been met or nearly met. In addition to these river channel metrics, two interim studies are monitoring the floodplain; these studies are the floodplain vegetation mapping study and the wading birds/waterfowl study, both of which provide information about landscape-scale responses to the interim status of hydrologic restoration for adaptive management and status assessment. Because of the large scale of this monitoring, both are expected to respond to some extent to the partial floodplain inundation we are experiencing in the interim period. Floodplain vegetation is a critical indicator of changes in hydrologic conditions and provides feedback to adaptively manage the system. With the exception of long-hydroperiod marsh, floodplain vegetation has shown a large positive response in Phase I since 2001. Monitoring of floodplain vegetation has also allowed us to document difficult-to-reverse trends such as an incursion of an invasive shrub species and its possible effects on native marsh reestablishment. Reasons for monitoring birds during the interim period are discussed below.

Because hydrology has not been completely reestablished, none of the sections where construction is complete is considered fully “restored” at this time. The logic of interim monitoring is described in several places in the chapter, starting at lines 49, 401, and 418.

Interim monitoring is used to guide adaptive management, assess the current status of ecological responses, and to evaluate and potentially address difficult-to-reverse and potentially far-reaching trends in the recovering system – examples are DO sags and incursions of invasive species. Interim monitoring is providing opportunities to learn about the system, which still has to be managed effectively although the project is not complete. Examples include:

- Our understanding of the relationship of DO crashes to flow has changed recently as a result of analysis of interim data. Before Phase I the mantra on DO was that crashes resulted from rapid recessions.
- Our developing understanding of the backwater effect of S-65C will likely translate into knowledge to facilitate management of S-65D, which will likely present similar management challenges downstream when the final phase of construction (Phase II/III) is complete.

Interim monitoring is viewed as especially crucial in a situation such as the KRRP’s, where the time between completion of restoration construction and complete hydrologic reestablishment is projected to be long (currently this period is expected to last >14 years in the Phase I area). The situation is analogous to monitoring a patient’s recovery from a serious illness: although the patient may not be fully recovered, it is important to monitor for signs of recovery and to avert problems before they become serious. None of our predictions is known exactly with absolute certainty, and interim monitoring helps to eliminate this uncertainty about recovery for both the current system and the ultimate outcomes of restoration.

We do not know the source of the reviewer’s statement that “there is little measurable success of these [bird abundance and reproduction rate] expectations”. The data presented in the chapter documented exceedance of the abundance expectation for wading birds in restored areas in all but two years since 2001 and for waterfowl in all years since 2001.

Birds are monitored in the interim period because (a) they are useful indicators of hydrologic conditions, (b) they have high interest for the public, and (c) we actively manage hydrology in the interim system to provide appropriate habitat for foraging water birds to the extent that is possible. Bird monitoring is conducted primarily in the dry season.

There are no expectation targets for avian reproduction. However, the reproduction data are useful in a regional context in that they allow comparison of the project area to the Everglades and Lake Okeechobee. In addition to being published in peer-reviewed journals and the SFER, these data are reported annually in the South Florida Wading Bird Report.

Regarding the question about continued surveys of birds during the interim period, while our seasonal surveys do provide interim data for the Phase I area, most of the data they provide is baseline (pre-restoration) data for the Phase II/III area (not restored yet) or control data for areas of the project footprint that will not be restored. These data will be used for comparisons with future restored conditions per our project mandates.

To address the DO concern, adaptive management in the past year seems to be helping with DO sags. We appear to have been able to lessen severe crashes by cutting back on the allowed rate of floodplain inundation this wet season, and we will be learning more in coming years about managing these events.

We anticipate that changes in hydrology following implementation of the headwaters schedule will reduce the frequency and severity of hypoxic events for several reasons. The headwaters schedule should reduce the rate at which discharge changes (as seen in this year's adaptive water management), reduce the frequency of high discharge events (>3000 cfs), and inundate the floodplain later in the year when temperatures are cooler, which should result in higher DO from increased solubility. Slower changes in discharge and lower peaks in discharge should reduce the effects of shading by deep water and the effects of scouring on periphyton, both of which reduce photosynthesis in the water column. The need to monitor during the interim period is here again due to the uncertainty of causation and the potential to learn to manage discharge to manage DO response.

Regarding nutrients, the reviewer may have misinterpreted the text in the Expectation statement, which has been revised to clarify that there is no expectation for P or N reduction by KRRP. Nutrient reduction was not included among the restoration expectations because:

- Nutrients were not monitored before river channelization, so there are no reference data from the Kissimmee River. Estimating former concentrations in the natural river is
- difficult to do with any certainty because there are no similar streams in the region with reference data.
- The KRRP was designed to restore natural habitat, not to remove nutrients. (Although as was stated in the chapter, the KRRP is anticipated to result in reduction of nutrient loads leaving the restoration area.)
- The restoration area is different from a constructed wetland that has well-defined boundaries, highly-regulated flows, and measured inputs and outputs that can be monitored conveniently. Consequently, distinguishing the KRRP's effects from other factors such as variations in annual discharge and changes in land use and runoff is difficult. Although the District does monitor at several locations within the restoration area, we do not have monitoring in place to isolate the effects of loading within the sub-basin (Pool B/C) that includes the restoration area.
- Water quality modeling and data available for modeling were insufficient at the time of expectation development to arrive at reliable predictions of predicted nutrient reductions within an acceptable level of certainty. Better modeling tools and additional data are available now to improve our predictive ability, but significantly better predictions would require considerable effort.

In the nutrient section, the author did not reach a conclusion regarding an increase or decrease in loading that could be attributed to restoration. So, we do not know the source of the reviewer's apparent conclusion that a "chemical expectation" has not been met. If the reviewer is comparing load at S-65A (north of the restoration area) to S-65C (south of the restoration area), the comparison is problematic because it does not consider the potential effect of loading within the sub-basin (Pool B/C) between the two structures. The correct calculation for retention is (load at S-65A + load from Pool B/C) – (load at S-65C). A positive value would indicate retention; a negative value would indicate a loss of nutrients from storage within the pool. The author of this section did not reach a conclusion on this point because of the current lack of appropriate data for this analysis.He will add clarification of this fact. Instead, on Lines 664-674, the author has offered a set of conclusions, based on his examination of data from C-38 structures collected over 40 years, that include the following:

- TP measured at S-65C is highly dependent on the TP discharged from S-65.
- Average annual loads and concentrations at S-65C have followed the increase at S-65.
- Although the restoration area has demonstrated the ability to retain a large amount of TP following storms, higher TP loading from S-65 is an important factor in considering why the TP load and concentration at S-65C have not yet declined, overall, since the 1974-1995 baseline period.

For several reasons, we believe that nutrient uptake will increase when the new Headwaters schedule is implemented. Most significantly, the expected development of a large area of wetlands on the floodplain and more time with discharge in the 1500-3000 cfs range (above channel bankfull) will result in more contact of phosphorus with floodplain sediments and vegetation. Again, there is uncertainty in how much would be retained, so there is a need to monitor during our extended interim period.

It is true that there are correlations between some metrics that under some circumstances are affected negatively (e.g., DO) by high discharge and floodplain inundation, while others require long periods of floodplain inundation (e.g., marsh vegetation or the hydrologic expectation for longer inundation). We anticipate that through adaptive management, these metrics will not be found to be mutually exclusive – it will be a matter of finding the right timing and balance of management goals in the future. The Headwaters schedule should provide the water management flexibility needed to do so. Thus, our plan is to learn as much as possible about system response by monitoring and adaptively managing during the interim period, and then implement the headwaters schedule and manage the system in the future using this valuable knowledge.

The reviewer's conclusion that "the data collected so far is not encouraging" is because the data collected to date is for a system for which the hydrologic condition is known to be lacking in specific and quantifiable ways, which will be changed under the Headwaters schedule.

Comment #11: *Lines 345-352 – Clearly the causes of hypoxia are not understood; nevertheless the data collected to date suggest that even when restoration is complete meeting the expectations for DO will be a challenge. It is not clear why more research is not being directed to this concern considering that fish assemblages are dependent on sufficient oxygen and the return of wading birds and perhaps waterfowl is likely dependent on sufficient fish prey. In these lines four hypotheses are provided for reduced DSO concentrations and these should be used as a starting point for research. That said, some comments on them are warranted. If the decrease is due to an influx of BOD from the floodplain, I would expect there to be a lag phase of a few days before DO started to decline as heterotrophic microbes responded to the increase in food source. If this is the cause, increased wetland coverage of the floodplain might exacerbate the problem by increasing primary productivity of the floodplain. If it is due to upstream dilution, tracer studies and/or volumetric balances could be uses to determine its magnitude. If it is due to photosynthetic inhibition, a comparison of diurnal cycles at high and low stages could be compared. Groundwater influx seems the least likely cause as elevated river stage probably minimizes groundwater efflux, but this could be evaluated by comparing the chemical signature of the groundwater to the surface water at times of high and low DO.*

Response #11: We recognize that hypoxia can affect responses to restoration, especially by fish, and have made increased efforts in the last year to investigate the causes of hypoxia in the Kissimmee River. These efforts include (1) developing a conceptual framework of the relationship of DO to environmental factors, (2) analyzing existing data to characterize past events (timing, magnitude, duration) and to examine the relationships of hypoxic events to other ecological data, (3) collecting additional DO data in the floodplain and upstream in C-38 during an event earlier this year, and (4) examining the potential for adaptively managing flow to reduce the frequency and severity of hypoxic events. The results of these efforts are still being analyzed and will be reported in the future. The four hypotheses are being used to guide the analysis. Many of the reviewer's comments on the hypotheses are consistent with our thinking. Preliminary results based on changes in specific conductance suggest that groundwater inflows are not an important cause of hypoxia in the Kissimmee River.

Comment #12: *Lines 466-472 – I do not understand what data are being compared here. The coefficient of variation Cv is defined as the standard deviation divided by the mean of the analyzed data. If the data consists of mean monthly flows, I understand a Cv between months over an annual cycle (one value) but do not understand how it could be different for different months. Is a varying Cv for different months used on data for daily flows within the month?*

Response #12: The text has been revised to indicate that CV is calculated for mean monthly discharge (each month of the year) and that it is calculated across years.

Comment #13: *Lines 478-479 (and subsequent discussion) – Interpretation of this Expectation #3 is quite difficult. What is meant by "average ground elevation"? Does this mean a cross-section? If so, how far from the river does it extend? What is the depth of the channel at the cross section? How is an appropriate cross section determined? The expectation should be more explicit and the methodology used to between cross-section to measure compliance should be discussed.*

Response #13: The caption of Figure 9-11 indicates that ground elevation is at a monitoring site and the text indicates that these monitoring sites are in the floodplain. Text has been added to indicate that the monitoring locations were selected to be representative of the same position within a floodplain cross-section as the pre-channelization reference data that were used to develop the expectation.

Comment #14: *Lines 542-593 – It is clear from Figure 9-13 that "baseline" reaches not only have a lower average DO in either season (compared to "reference" blackwater reaches) but also vary more between the wet and dry seasons, generally due to even lower concentrations in the wet season. Could the Kissimmee River have a different response than the selected reference streams or is this observation due to more disturbance?*

Response #14: Since prechannelization DO data do not exist, we used DO data from reference streams that we believe are representative of the prechannelized Kissimmee River. It is possible that the restored Kissimmee River could have a different response than the reference streams, but the reference streams are the best available substitute for actual data from the prechannelized river.

Comment #15: *Lines 542-593 – Additionally the discussion here seems to conflict with the earlier discussion of DO in lines 309-344. Here it is suggested that restored reaches have improved DO levels but the earlier discussion talk of two hypoxic events and DO level low enough for fish kills. While the locations might be different or other non-addressed factors not fully explained but the reader comes away with a very different conclusion on DO concentration in these two sections.*

Response #15: DO conditions in the restored reaches of the Kissimmee River have improved in general. However, we continue to have issues with DO sags during the wet season in most years. This apparent contradiction in results is because the expectation metrics are not greatly influenced by short-term declines in DO. We are working to determine the causes of the sags and are developing techniques to ameliorate DO sags through adaptive management. We will add this information to the text.

Comment #16: *The panels of Figure 9-11 are very small and layout results in too white space. Enlarge and square the layout*

Response #16: This figure has been revised and enlarged.

Comment #17: *The legends on figures 9-16 through 9-20 are too small to see which bars represent which parameters. Part of the issue might be the little white bar at the top of the bars which carries over to the legend.*

Response #17: We will make these legends clearer.

Comment #18: *Lines 39-44 – A very long sentence, try replacing "and" on line 42 "with in addition to"*

Response #18: We shortened this sentence.

Comment #19: *Line 65 – for "a"*

Response #19: This has been changed to read, "The expected seasonal flow pattern…"

RESPONSES TO PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 10

Christopher Buzzelli, Peter Doering,
Lesley Bertolotti and Chapter Contributors

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

WebBoard-Posted: 10/24/13

Comment #1: Apparently, in WY 2013 the TP and TN concentrations and loads were elevated and increased in the last two years but the authors stated that they were still "below the long term averages". This is not a good comparison because the long term average reflects high and unacceptable nutrient concentrations. Developing 5 year moving averages, as it was done in some other chapters of the Florida ER, gives a better picture about the progress towards improvement.

Response #1: Comparing WY2013 loading to the long term load was provided merely as a comparison, not to imply that nutrient loading does not need to be further reduced. The purpose of the tables in this section is to provide a comparison between the past three water years and a long term average for reference. The TMDLs for the St. Lucie are based on an annual load. The Lake Okeechobee TMDL is based on a five year rolling average and therefore a five year rolling averages were calculated for the Lake Okeechobee loads. In the future, we will consider including five year rolling averages for the St. Lucie loads as well for comparison purposes.

***Comment #2:** What is missing in this discussion on otherwise excellent reporting on monitoring, is what is the limiting concentration that could affect algal development and the trophic status of the estuaries. Marine waters are generally nitrogen limited. The monitoring also found that salinity levels were generally favorable to the oyster population (Figure 10-9 on page 10-28).*

Response #2: Establishing limiting concentrations of nutrients is not straightforward in estuaries. Nitrogen limitation of phytoplankton in controlled bio-assays on daily time scales suggested that reductions in N loading could decrease primary production in the SLE. This result is reasonable given that N has been emphasized as the dominant limiting nutrient in estuaries. However, the extrapolation of knowledge gained from fine scale experiments to the estuary scale may not be appropriate because of feedbacks related to internal biogeochemical cycling. This may be particularly true for N and P recycling within the SLE where there is surplus DIN produced under increased DIP loading. The recognition that sub-tropical estuaries have the potential to respond to both N and P inputs is an important step to establish nutrient load limits. However, merely setting criteria for either total or dissolved nutrient loading based on their correlation to water column concentrations fails to appreciate the complexity and uncertainty of estuarine biogeochemical cycling. In both the discharge and the receiving basin, the DIN:DIP ratios are often very different than the TN:TP ratios due to the differential composition and reactivity of dissolved organic N and P.

***Comment #3:** In the summary, the monitoring found that in WY 2013, contribution of Lake Okeechobee decreased to very low levels, yet TP and TN loads are correlated to fresh water inflows which today are mostly from the watershed. This gives an incentive to developing TMDL and watershed NP pollution reduction programs which is lagging in the estuarial watersheds behind Everglades' watershed south of Lake Okeechobee. The fact that the Total N and Total P are exceeding the target concentrations makes this urgent.*

Response #3: Yes, the River Watersheds were recently recognized as part of the Northern Everglades through the 2007 Northern Everglades and Estuaries Protection Program (NEEPP) legislation and therefore are in much earlier stages of development and implementation than the programs in the Southern Everglades and the Lake Okeechobee Watershed. The Coordinating Agencies (FDEP, FDACS and the District) are committed to continuing to development their respective source control programs and to increase participation.

Comment #4: *The summary findings generally express average conditions for the estuary, somewhat short of freshwater flow so that flow interventions were necessary to maintain salinity favorable to the oyster population. Similarly to SLE waters, the Total N and P were elevated over the WY 11 and WY12.*

Response #4: In WY2013 the CRE experienced rainfall and freshwater inflow that were similar to long-term average values. There was no explicit effort by the U.S. Army Corps of Engineers (USACE) or the SFWMD to modulate freshwater discharge in consideration for estuarine resources. Pulse releases from January-April 2013 occurred within the confines of the Lake Okeechobee Regulatory Schedule (LORS).

Comment #5**:** *It looked to the reviewer like there were no major problems with the CRE except with the low focus on reducing TP and TN loads form the watershed which may become a problem in the future. Some signs of the problem were already revealed in the scientific surveys (Adaptive Protocols) which will be subsequently pointed out.*

Response #5: Unlike the SLE, the relationships between watershed nutrient loading and water quality patterns are less clear for the CRE. It is suspected that biogeochemical feedbacks including coupling of nitrification-denitrification at the sediment-water interface could account for the observed differences between the CRE and SLE. Internal production of C, N, and P was more influenced by external loading to the SLE than the CRE. While the CRE is 2.5 times larger and receives twice the freshwater inflow, spatially normalized DIP and DIN loadings are only 60-70% of those to the SLE. Water column DIP concentrations, DIP consumption, and system autotrophy increased with external DIP loading to the SLE. Empirical and modeling studies to decipher and quantify the relative roles of inflow magnitude, inflow TN and TP concentrations, and internal nutrient recycling are being planned.

Comment #6: *In the Summary on Page 10-60 the authors state that chlorophyll a concentrations were low. Actually, they were quite high during some cruises in the zone of transition from fresh water to higher salinity marine conditions. However, the authors emphasized in the chapter several times that marine life depends on the productivity in the upper fresh water section. The authors could express their opinion as to whether or not having a higher productivity eutrophic upstream fresh water reach is good.*

Response #6: Determining the significance of increased primary production in the oligohaline estuary presents a difficult situation. As stated in Responses 2 &5, the limited information on biogeochemical cycling in the CRE indicates that such determinations might be premature. Increased phytoplankton and biomass in the upper-to-mid estuary result from the combined effects of nutrient and light availability. In general, submarine light conditions improve and nutrient concentrations decline in the downstream direction in estuaries. This appears to be the case for the CRE.

Comment #7**:** *One strategic problem (not attributed to the authors) is the lack of implementation of BMPs in the watershed that would reduce the potential eutrophication salinity transition zone. Unlike in the other South Florida Zones discharging into the Everglades, participation of dischargers in BMP phosphorus and nitrogen (the estuaries are most likely nitrogen limited) load* *reducing programs is very low and most practices reported in this chapter were structural, many focusing on flow manipulation and not on load reduction.*

Response #7: With BMP enrollment it is important to note that the strategy directed by the NEEPP includes a combination of regulatory and voluntary BMP programs. The objective of the BMP programs north of the Lake and in the River Watersheds is to ensure that landowners implement BMPs under either the District's regulatory program or the FDACS voluntary Agricultural BMP program to avoid unnecessary duplication between the overlapping programs. The types of BMPs required under these two programs are consistent. Unlike the strategy in the Southern Everglades, the SFWMD regulatory program alone is not representative of progress in the Northern Everglades. Therefore, when considering overall source control program implementation under NEEPP, one must also consider the FDACS Agricultural BMP program (Table 4-10). Furthermore, it is important to note that the SFWMD's current rule must be amended to incorporate the expanded boundary described under NEEPP, that is, incorporate the northern areas of the Lake Okeechobee Watershed and the River Watersheds (see Figure 4-9). These expanded areas are in much earlier stages of development and implementation of source control programs than in the Southern Everglades and the southern portions of the Lake Okeechobee Watershed.

With that said, it is important to note that has been substantial efforts to implement water quality, storage and retention projects in the River Watersheds. There have been over 138 local storm water projects implemented in the two watersheds, two components of the CERP IRL-S authorized project are underway, and the Dispersed Water Management Program is very active in the River Watersheds. Furthermore, the C-43 West Basin Storage Reservoir is a project proposed for authorization in the current version of the proposed Water Resource Reform Development Act in Congress.

Comment #8*: *Hence, in conclusion this document represents a defensible account of data that appears to be complete and appropriate. The data presenting is thorough and comprehensive.

Response #8: Thank you we appreciate your constructive comments.

Comment #9: *This report is very strong on presenting data and information on the actions undertaken during the WY 2013. However, because of the cruises, being done just recently, a full synthesis of the collected data and forming hypotheses was not done. One may look forward to peer reviewed articles and special reports by SFWMD that will significantly contribute to the understanding of the two important Florida estuaries and their interactions with Lake Okeechobee. The findings of the Adaptive Protocol research will definitely lead to management decisions but this was not done in this report.*

Response #9: We have several technical efforts that are relevant to this report.

Buzzelli, C., Boutin, B., M., Ashton, M., Welch, B., Wan, Y., Gorman, P., Doering, P. (in press). Synoptic-scale detection of estuarine water quality with managed freshwater releases. *Estuaries & Coasts*.

This paper summarizes the first Adaptive Protocols Release Study of the CRE in 2012. It is a very unique effort that links targeted freshwater inflows to estuarine properties on synoptic time scales.

Buzzelli, C., Doering, P., Wan, Y., Boyer, J. (in press). Seasonal dissolved inorganic nitrogen and phosphorus budgets for two sub-tropical estuaries in south Florida. *Biogeosciences*.

This important paper examines inter-seasonal and inter-annual variations in rainfall, freshwater inflow, external nutrient loading, internal nutrient concentrations, and net ecosystem metabolism in the SLE and CRE from 2002-2008.

Buzzelli, C., Doering, P., Wan, Y., Gorman, P., Volety, A. (2013). Simulation of potential oyster density with variable freshwater inflow to the Caloosahatchee River Estuary (1965-2000). *Environmental Management*. 52(4):981-994. DOI 10.1007/s00276-013-0136-3.

This paper addressed potential impacts of long-term (1965-2001) water management in the CRE watershed using oyster-salinity relationships to help define optimal freshwater inflows.

Buzzelli, C., Chen, Z., Coley, T., Doering, P., Samimy, R., Schlesinger, D., Howes, B. (2013). Dry season sediment-water exchanges of nutrients and oxygen in two Floridian estuaries: Patterns, comparisons, and internal loading. *Florida Scientist* 76(1):54-79.

This paper combined empirical measurements, bathymetric data and GIS to quantify internal cycling of C,O,N, and P in the SLE and CRE in dry season 2008. Equivalent wet season studies are being planned for 2014.

Buzzelli, C., Parker, M., Geiger, S., Wan, Y., Doering, P., Haunert, D. (2013). Predicting system-scale impacts of oyster clearance on phytoplankton productivity in a small sub-tropical estuary. *Environmental Modeling & Assessment* 18(2):185-198. DOI 10.1007/s10666-012-9338-y.

This effort used an oyster simulation model to explore responses of oysters to freshwater inputs and predict the potential benefits of oyster habitat restoration on water quality in the SLE from 1998-2008. The base oyster model is being used for a variety of applications in water and resource management for both the SLE and CRE.

Buzzelli, C., Robbins, B., Chen, Z., Sun, D., Wan, Y., Doering, P., Welch, B., Schwarzchild, A.. (2012). Monitoring and modeling biomass dynamics of *Syringodium filiforme* (manatee grass) in southern Indian River Lagoon. *Estuaries & Coasts* . 35:1401-1415.

This effort used a simulation model to explore responses of seagrass to freshwater inputs, salinity, and submarine light in the southern Indian River Lagoon from 2002-2008. The base seagrass model is being used for a variety of applications in water and resource management for the both the SLE and CRE.

Specific Editorial Comments:

Some lettering on Figures 10-2 and 10-3 are difficult to read.

Yes, that is noted. These figures were not necessarily generated for this presentation, but had to be reduced in size for efficient utilization of space within Chapter 10.

Figure 10-4 needs a conversion from inches/day to mm/day. "Inches/day" appears in the caption twice

Thank you, corrected.

Table 10-4 needs a conversion factor to mm or cm.

We are unsure about this comment. A description of the unit conventions used throughout Chapter 10 is provided on pg. 10-5. Mixing of units between English and metric systems is a common concern.

Figure 10-6 needs unit identification on X-axis (cfs)

Thank you, corrected.

Figure 10-14 needs conversion factor to mm. Also remove (d-1), it is redundant.

Thank you, corrected.

Table 10-6 needs conversion factor to mm or cm.

Thank you, corrected.

Table 10-7. Be consistent. Include conversion from acre-ft to m3 as it was done in the other tables

Thank you, corrected.

Figure 10-15 Conversions from cfs to m3/sec is needed in the caption. Same for Figure 10-1

Thank you, corrected.

Table 10-9 Conversion from acre-ft is missing.

Thank you, corrected

Figure 10-27 Conversion from cfs to m3/sec

Thank you, corrected

Figure 10-29 Identify stations also by Km

Referred to Figure 10-22 for station locations in the CRE.