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

During September–October 2012, these comments from the peer-review panel were provided on the 2013 SFER WebBoard (www.sfwmd.gov/webboards). SFER panel comments were prepared under separate Purchase Orders by the South Florida Water Management District. With the exception of spell check and reformatting some information for better readability, this appendix was not edited by the SFER production staff and appears verbatim as posted on the WebBoard. No public comments on draft Volume I were received on the WebBoard.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3A

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

Posted: 9/20/12 @ 7:25 AM EST

This chapter provides an assessment of water quality in the Everglades Protection Area (EPA) during the Water Year (WY) 2012 which started May 1, 2011 and ended April 30, 2012. In total, the chapter evaluated 10 water quality parameters out of 109 monitored within the EPA and a group of pesticides. The following water constituents were evaluated in the chapter:

Alkalinity, Dissolved oxygen, Specific conductance, pH, Turbidity, Un-ionized ammonia, Sulfate, Total nitrogen (TKN + nitrate/nitrite), Total phosphorus, and Orthophosphate.

The agency used applicable Class III water quality criteria and the assessment revealed the water criteria (standards) for the following parameters were not attained: dissolved oxygen (DO), alkalinity, specific conductance, and pH. The excursions were mostly localized and occurred also in the previous WY's.

In overall, the chapter is well written, organized and consist with units, i.e., it generally adheres to SI units and, when US units are used, it provides conversions. This is important, because the SFER will be read and studied not only by US scientists, officials and the public, but also by an international audience.

Furthermore, unlike some previous SFERs, this WY report presents positive trends and progress in water quality improvement, although the reason for the optimism may be related more to climatic conditions than to the full implementation of planned water quality improvement measures.

Specific Comments

Page 3A-2, Lines 38-43. Alkalinity and pH excursions could be of natural origin which could be disposed of by a Use Attainability Analysis (UAA). It is not clear from the text, although it is implicit, whether the alkalinity excursions were on the low side. A statement why low alkalinity is a concern could be added.

Lines 52 to 55. The sulfate excursions should be of concern and this will be discussed in the review of Chapter 3B. High sulfate concentrations penetrating into sediments release phosphate into solution. On the other hand, low sulfate (but not extremely low) promotes formation of methyl mercury which is a highly toxic bio-accumulative constituent. Because enough pioneering knowledge has been developed by SFWMD scientists and their consultants, the State of Florida should seriously consider developing a site specific sulfate standard that would be related also to dissolved organic carbon (DOC) and, maybe, to other constituents (e.g., type of DOC). This will also be discussed and proposed in more detail in the review of Chapter 3B.

Lines 61-70. Total phosphorus data indicate that the TP standard of 10 µg/L is generally met in the EPA Park and very close to being met in the Refuge Area. The WY 2012 and 2013 SFERs provides a more positive outlook regarding the trends and chances of meeting the standard in the near future.

Page 3A-3, Lines 84-86. The district collects and analyzes nitrogen separately for TKN (Total Kjedahl Nitrogen = organic N and Ammonium) and nitrate/nitrite N. If possible, the authors should provide also separate information for these constituents in the report and Summary because these constituents, in some aspects, affect water quality and eutrophication differently.

New knowledge on nitrate/nitrite eutrophication effects will be presented in the subsequent section of this review.

Pages 3A-4–3A-8. The monitoring program was established years ago and has been providing excellent data bases.

Page 3A-9, Line 223. The readers generally are not familiar with the concept of "fatal" qualifiers included in the Florida Administrative Code. A brief explanation of the meaning of "H, J,K,N,O,V,Q.Y, or ?" would be very helpful. Also on page 225, there is no way pH can be measured as greater than 14. This could be stated better as "if a typographical error such as pH greater than 14 has been found…."

Page 3A-10. Water quality excursion analyses lines 263 – 261. In last year's report review this reviewer pointed out potential problems with the estimates of water quality concerns. The 10% excursion limit for "concerns" is generally used in state water quality reporting to the US EPA and Congress under Section 305 (b) of the Clean Water Act and for constituents such as DO could lead to accepting "fatal" but within the 10% excursion limit as only "a potential concern". This concern should be repeated in this review but it is not aimed at the SFWMD scientists who are constricted by the federal Section 305d guidelines adapted by most states. This evaluation leads to a preliminary list but may not be used to developing TMDLs under Section 303 (d) of the Act which are based on more stringent allowed exceedances for parameters such as DO, unionized ammonia and priority pollutants that would include pesticides. Generally, if a parameter consistently fails the 305(b) frequency criterion the water body should be put on the 303(d) list leading to TMDL or UAA. In general, the approach based on the frequency distribution in the 2013 SFERR is correct.

Page 3A-13, Lines 327-335. The concern with sulfate concentrations is warranted. Fish and other organisms residing in some parts of the Everglades system have very high mercury tissue content. The 2012 WY apparently had sulfate concentrations 75% higher than those in 2011 in the most of the interior marsh stations.

The sulfate content in water is linked to that in pore water of sediments which causes two problems:

A. Reduction of sulfates to sulfides in anoxic/anaerobic sediments by sulfate reducing bacteria (SRB) releases iron bound phosphate into solution and ultimately into overlying waters. This phenomenon is causing the managers to calls to develop measures to reduce sulfate concentrations. Sulfides in the sediment pore water are toxic and prevent formation of methyl mercury.

B. However, in some parts of the EPA and Refuge Areas, sulfate concentrations are low, below 10 mg/L. The research reported in Chapter 3B of the 2012 SFER indicated that sulfate concentrations in water above the sediment between 1 to 9 mg/L reduce the sulfide toxicity to SRB and lead to formation of methyl-mercury by the same SRB. The SFWMD scientists and consultants have now enough excellent information for developing a scientific site specific criterion that could lead to sulfate TMDL and abatement focused on reducing sulfate loads to levels that would both control eutrophication and prevent formation of methyl mercury.

Page 3A-16 – Dissolved Oxygen. Repeatedly, like in the previous reviews, this review again must point out the dichotomy of the Everglades DO site specific alternative criterion which, apparently, was developed from long term observations of DO concentrations. It is a well known fact that wetlands, including EPA, are naturally dystrophic and exhibit low DO. The SSAC criterion is actually a harmonic model which calculates "unimpacted" DOs from temperature and annual and diurnal time. It was pointed out in the previous reviews that some of these calculated SSAC were at or below the lethal values for quality fish and using annual averages for the

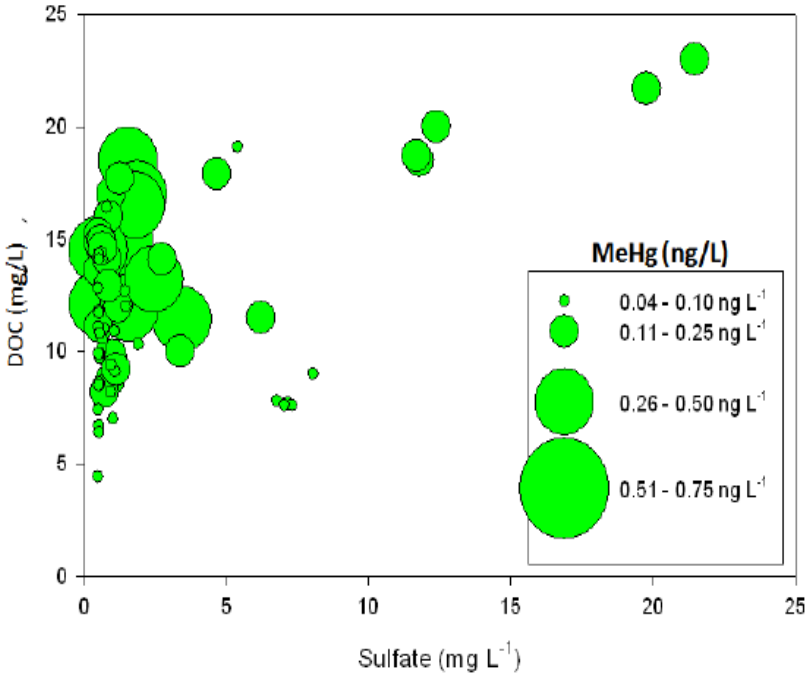
assessment does not make much sense. It was also pointed out previously that fish kills occurred in the EPA, lastly in 2010 but apparently the last fish kill was not caused by low DO. The previous reviews recognized the fact that the authors must adhere to the current standards and guidelines and not to get involved in the debate on the validity or deficiencies of the current SSAC imposed by the state or federal regulatory agencies. Hence, 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.

Page 3A-16 to 17 – Alkalinity and pH. 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 pH of rain is around 5.7 and has very low alkalinity. Again, the reviewer agrees with the report that possible excursions of alkalinity are not of a major concern; they may be natural. Similarly occasional lower pH is most likely natural due to the dominance of the rainfall as the main hydrological input in the protected areas. Natural pH of rainfall is less than 6.

Page 3A-17 – Specific conductivity. The Refuge rim stations as well as some WCA 2 inflow stations had a problem with high salinity (conductivity) caused mainly by increased pumping of salty water from canals. In general the specific conductance excursions decreased in the EPA interior areas. It is logical that if a significant portion of the input is by atmospheric wet deposition, enough dilution may be available to bring the interior salinity to acceptable levels; however, the high salinity in Refuge and WC-2 is a concern. Because the canal discharges may continue in the future, the SFWMD should develop a mass balance model and discharge management that would avoid salinity problems in the affected areas. In WCA – 3 and Park the interior salinity is normal.

Page 3A-20 and 21– Sulfate. This reviewer will address the sulfate issue again in his review of Chapter 3B ("A" review). Table 3A-2 indicates that high sulfate concentrations were pointed out on page 3A-17 as being 75% higher than in the preceding water year. This means that more dissolved phosphorus will be released into the pore water of the sediments and, subsequently, potentially in the water above the sediment. However, in the interiors of WCA-2, WC-3 and EPA, 25th percentile to median values were between 1 to 5 mg/L which is the "Goldilocks" window at which methyl-mercury is suspected to be formed. It appears that all three could be adversely affected by the sulfate concentrations in the 1 to 5 mg/L sulfate window (see Figure below).

This figure taken from the 2012 SFER shows the 1-5 mg/L sulfate window in which methyl mercury can be formed at higher concentrations if the dissolved organ carbon (DOC) is between 10–20 mg/l , (i.e., the water body is organically enriched by algae development). The methyl mercury formation is only mildly affected by mercury inputs because the atmospheric Hg deposition could be sufficient.



Pages 3A-22–3A-24 Pesticides. Unlike the previous report which identified only atrazine as a pesticide of concern, during WY 2012 nine pesticides or pesticide breakdown products were detected at concentrations above the minimum detectable level (MDL) but none of them at levels US EPA priority criteria.

Pages 3A-25–3A-31 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. As in the previous year report, this section on phosphorus presents again the trends of long term geometric averages of TP in the various locations of the EPA water bodies. These long term averages were influenced in the water years 2005 by extreme meteorological events from which the system was recovering in the subsequent, mostly dry years. It is expected (lines 599 – 605) that as the structural and nonstructural Phase II BMPs for agricultural pollution and STA measures for runoff pollution load controls will be implemented, the results will be less affected by climatic conditions and the results of long term restoration will become more clear.

Figure 3A-9 and 3A-10 document the trends. Figure 3A-9 reports annual P concentrations for refuge and WCA-2 areas. In the refuge area the P concentration trend in the inflow is downward and the interior the P concentrations have been stabilized around the goal of 10 µg/L since 1994 with a local spike in 2005. In WCA-2 the annual P concentration in the inflow have also been decreasing since 1994 and, as a result, the interior concentrations are now actually below the goal of 10 µg/L. Same trends and general compliance with the 10 µg/L goal were reported in Figure 3A-10 for WCA-3 and ENP areas.

Maximum concentrations of the TP in the inflow were around 40 µg/L in the WY 2012; however, they reached a maximum of 200 µg/L. In the ENP the P concentrations are well below the goal. These trends can be characterized as good news. Nevertheless, the last several years were affected by drought, resulting in lower P inputs into the system. The refuge site is affected by the canal inputs which were limited in the WY year. The effects of BMPS and structural measures will be noticeable during wet years.

Pages 3A-31-32 – Orthophosphate. Geometric means of orthophosphate concentrations reported in Table 3A-5 are well below the goal. However, maxima reported in the table were very high in the past but not in WY 2012. Is there an explanation? Is it the effect of the 2005 hurricane year that impacted the 2005 – 2011 period. This should be stated in the report.

Pages 3A-33- Total Phosphorus Load. This section is revealing and explains partially the downward trend in concentrations even when the BPs and STAs are essentially in the first Phases of implementation. The long term atmospheric phosphorus load to the WCAs only was reported as ranging between 107-147 metric tons while the total load from land sources is 36.7 metric tons. The total loads are correlated to the flow input (Figures 3A-12 and 3A-13). The flow is related also to atmospheric deposition. This poses a managerial and also political dilemma in pushing for completion of the remedial program; however, it can be stated that the atmospheric deposition is also favorably affected by agricultural BMPs that reduce both water and wind (atmospheric) erosion soil erosion.

In the review of Chapter 4 it will be pointed out that the largest and the only phosphate surface (Bone Valley) mine is located in the vicinity of the northern watersheds of the Everglades. Surface mines are notoriously known for being a large source of dust entering atmospheric currents. Most likely, the effect of phosphate mining in the vicinity of the Everglades' watershed may not be known.

Regarding units on the figures; while load is expressed in SI (metric) units the flow is in archaic US units. Please provide a conversion from acre-ft to m3 and also Table 3A-6 below the figures and table. The metric equivalent of acre-ft is simply m3 (or 1000 m3) or 1 acre-ft = 1233.5 m3. Unit *hectare-meter* does not exist in the SI technical literature. The adherence to SI units was requested also in the last year review.

Pages 3A-34- Total Phosphorus Load. The reviewer agrees with the assessment stating, based on the data provided in the report and appendix, that unimpacted portions of each WCA and the entire EPA were in compliance with the 10 µg/L phosphorus criterion. However, the impacted portions failed at least one of the compliance tests. The district should identify the reason and readjust the abatement program if needed.

Pages 3A-37-40 – Total Nitrogen Concentrations. Nitrogen is not a limiting nutrient in the Everglades (it is in the Saint Lucie estuary) but common sense dictates that both TP and TN loads should be reduced to protect the Everglades protected areas. Generally, most BMPs reduce both nitrogen and phosphorus.

Table 3A-37 shows that N concentrations in WY2012 were significantly lower than those for the multiple year averages of the previous periods. This could be misleading because the multiple year period may all contain an extreme climatic event (hurricane) that increased the mean when compared to the relatively dry eventless WY 2012. On the other hand, the reports suggested that one of the main sources of N, in addition to the agricultural inputs and Lake Okeechobee, is the organic matter produced naturally in the EPA wetlands.

There is also evidence in the literature that nitrate in water will form a nitrate layer on top of the sediment (below the oxygenated layer) which may suppress release of phosphate from the sediment and retard eutrophication (Foy, R.H. (1988) Suppression of phosphorus release from lake sediments by the addition of nitrate, *Wat. Res.* **20**(11):1345-1351; Andersen, J.M. (1982) Effect of nitrate concentrations in lake water on phosphate release from the sediment, *Water Research* **16**(7): 1119-1126).

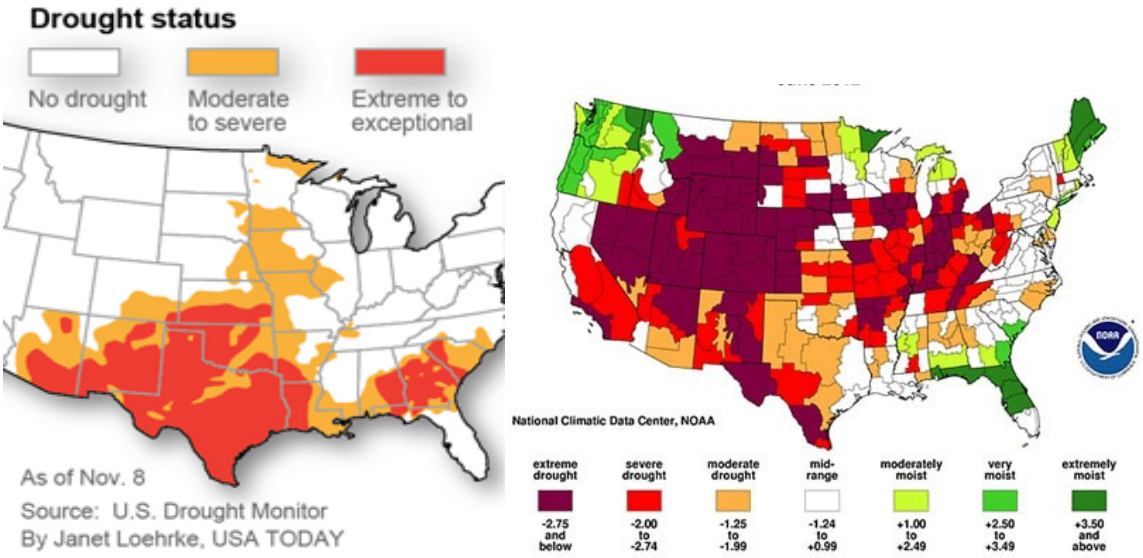
Comments on Appendices

There are no comments on the Appendices. Some problems regarding DO site specific criterion were discussed in the 2012 report review and they may be continuing but this problem is beyond the discussion and review of the work by the authors of the chapter.

Comments:

This chapter is well written and is better, both in the content and style, than the last year chapter. Because of the international importance of the EPA it is appreciated that the writer used in general, the international system of units. The suggestions for further discussions and presentation to the SFWM are given below.

- The overall water quality assessment documents a hopeful stabilization of the system, reaching a steady state, which in the protected areas is generally attaining the EFA very stringent goals typical for oligotrophic water bodies. The continuing question is whether or not this current situation is a sign of a permanent trend or a result of drought. It appears that Florida might not have been affected by the severe 2011 and 2012 droughts (see Figure below, 2011 drought is left and 2012 is right) but this question still should be kept open.



- The most important revelation of the chapter, which might have been reported in the previous reports but was not discussed in the reviews, is the dominance of difficult (or impossible) to control atmospheric sources of phosphorus. This poses a dilemma to managers on one side but may not be a big problem because this chapter documents a compliance with the EFA phosphorus goals in the protected areas, with some exception in the inlet areas. Physically, the interior areas would be more affected by the atmospheric inputs while inlets may contain more surface inputs that are evidently attenuated in the water bodies by deposition. It is also hoped that the nonpoint pollution controls in the watershed may also bring about some reduction of atmospheric inputs of phosphorus. This problem is similar to that of the Great Lakes where more than 50 % of P input is also from atmospheric sources.
- The question of sulfate concentrations and controls and the effect on very high mercury concentrations in fish will be discussed in the review of chapter 3B. It has become clear that under the scenario of continuing atmospheric deposition of mercury, low sulfate

concentrations are not desirable unless dissolved organic carbon is reduced below 10 mg/L. Unfortunately, the state-of the –art of the fish mercury levels controls related to sulfate reductions is not yet fully developed and reduction of atmospheric deposition of Hg is uncertain and not achievable in a short run. However, because most of Hg deposition can be "blamed" on coal fired power plants, the recent trend to replace coal fired electricity production by natural gas and renewable sources may bring a needed relief. But, the recovery may last for years.

- The phosphorus loads from the Lake Okeechobee and other inflows may still be high and should be controlled mainly to provide improvement to the lake which is overloaded with phosphorus and not healthy. However, as stated above, surface sources to protected areas are smaller than atmospheric sources.
- As stated in the last year review, the SFWMD must continue to be vigilant regarding the low DOs in the system. It is recognized that Florida and specifically EPA wetlands are generally naturally dystrophic which is reflected in the site specific DO standard; hence, excessive excursions may lead to fish kills and sometimes, but rarely, even meeting the DO standard may not provide enough protection.

Posted: 10/24/12 @ 1:11 PM EST

This chapter provides an assessment of water quality in the Everglades Protection Area (EPA) during the Water Year (WY) 2012 which started May 1, 2011 and ended April 30, 2012. In total, the chapter evaluated 10 water quality parameters out of 109 monitored within the EPA and a group of pesticides. The following water constituents were evaluated in the chapter:

Alkalinity, Dissolved oxygen, Specific conductance, pH, Turbidity, Un-ionized ammonia, Sulfate, Total nitrogen (TKN + nitrate/nitrite), Total phosphorus, and Orthophosphate.

The agency used applicable Class III water quality criteria and the assessment revealed that the water criteria (standards) for the following parameters were not attained: dissolved oxygen (DO), alkalinity, specific conductance, and pH. The excursions were mostly localized and occurred also in the previous WY's.

The authors provided satisfactory and thoughtful responses to all comments listed in the Panel Review. A great majority of the responses by the authors noted and agreed with the reviewer's points, will consider the suggestions and include revisions, mostly minor, in the final report. Such responses were made to Comments 1 to 6, 8 to 10, 12 to 16, and 18 to 23.

Specific items requiring additions and and/or modifications of the text of the chapter are:

1. Authors will add text explaining "fatal" qualifiers included in the Florida Administrative Code (Response # 6);

2. Authors agreed that the complex problem of salinity may require SFWMD to develop a mass balance and discharge management of salinity (Response #11) – see the bullet below.

3. Authors will include an explanation of the causes of the low orthophosphate concentrations in the WY 2012 (Response # 15)

4. Authors will address the disproportion of the atmospheric and surface sources of P to the Everglades. A potential large source of atmospheric P could be surface mining of phosphate in the Bone Valley mine located in the close vicinity of the watershed (Response # 16) – see the bullet.

We continued the discussion of the appropriateness of the site specific dissolved oxygen criteria function that would allow the DO to dangerously low levels without a violation (Comment #7 and response). The issue of developing and approving such criteria, approved by the US Environmental Protection Agency, lies with the Florida Department of Environmental Protection and not with the authors. The authors correctly pointed out that DO is not a direct pollutant and low DO is one of the symptoms or outcome of unhealthy conditions caused by an excessive phosphorus load. Complying with stringent P concentration standards is the primary objective of the abatement that could also address the DO problem. Wetlands are naturally dystrophic and current US EPA DO criteria may not be applicable. There is also a problem with allowed 10 percent allowable excursions for dissolved oxygen which are in conflict with the US EPA water quality DO criteria.

The second issue partially addressed in the review was sulfate. It was pointed out that sulfate to sulfide reduction in the sediment is the "pump" releasing phosphorus from the sediment into pore water solution and, subsequently, into the overlying water, constituting the internal P load. This process is carried by sulfur reducing bacteria. The same microorganisms, under certain favorable conditions, can convert mercury into highly toxic and bio-accumulative dimethyl mercury. This issue will be addressed more extensively in the Review of Chapter 3B.

Key Findings and Recommendation (Phase III)

Regarding the control of sulfur loads into the Everglades system, the SFWMD scientists and consultants have enough excellent information and data base for developing a scientific site specific multi-parameter criterion and management plan that could lead to sulfate TMDL and abatement focused on reducing sulfate loads to levels that would both control eutrophication and prevent formation of methyl mercury. This recommendation was also included in the Final Review of Chapter 3B.

High salinity (specific conductivity) in the refuge rim and some WCA 2 inflow stations requires attention. Because the canal discharges carry high salinity that may continue in the future, the SFWMD should develop a mass balance model and discharge management that could avoid salinity problems in the affected areas.

The dominance of the atmospheric inputs of phosphorus into the Everglades system over surface sources poses a problem because, as of now, the sources are mostly unknown. It was pointed out in the review that a large and the only surface phosphate mine is located in the proximity of the watershed. Surface mining is notoriously known for large atmospheric emissions of particulates. Data should be collected that would separate these loads from those from agricultural operations. These effects should be assessed and if they are significant, the mine operators should improve and implement best management practices pertinent for erosion and dust control of surface mines such as rapid revegetation of exposed areas, landscape restoration, and other practices similar to soil erosion reduction in agricultural operations.

Chapter 3A, in general, presented positive trends and progress in water quality improvement, although the reason for optimism may be related more to climatic conditions than to the full implementation of planned water quality improvement measures. Under favorable climatic conditions, the systems continued its recovery from high loads and concentrations of several years ago and the key areas of the Everglades system are generally in compliance with the stringent phosphate criteria. The remaining unresolved issues include the effects of sulfate loads on dimethyl mercury contamination of fish in the EPA, continuing implementing best management practice for control of phosphorus loads, and identifying the magnitude of atmospheric phosphorus loads and their sources.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3B

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

Posted: 10/4/12 @ 7:30 AM EST

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

Broad questions and comments that should be addressed:

This chapter deals with the serious problem of high and unacceptable mercury contamination of the greater Everglades ecosystem and its potential causes and controlling factors. The monitoring focus is generally and rightfully on the more bio-available methyl-Hg form and in higher trophic level organisms, specifically largemouth bass (LMB); the Florida panther, and fish eating birds though for comparison fish on a slightly lower topic level (mosquito fish and sunfish) are also monitored. A serious human health problem exists in that sport fish Hg concentrations are 300–400% above the US EPA limit for safe human consumption. A general downward trend has occurred over the last twenty years; however a relatively significant decrease occurred approximately 10 years ago and despite obvious yearly variation, additional long-term reductions are not apparent in the data. Interestingly, when aggregated across all sample species and locations, there appears to be a slight increase in mercury levels since about 2010.

The report, as in the past years, attributes the methyl mercury formation and tissue contamination to (1) inputs of mercury from several sources into the EPA system, and (2) chemical sulfate (SO_4^{2-}) reduction without significant sulfide (S^{2-}) formation and (3) the dissolved organic matter concentration and type. The primary source of mercury is from the atmospheric inputs with the majority purported to be from international sources. Considering the lack of control on this controlling factor the focus of mitigation is again on the influence of input sulfur, specifically sulfate, on mercury methylation.

The formation of methyl mercury (MeHg) is related to the biological activity of sulfur reducing bacteria (SRB), the same bacteria that by reducing sulfate to sulfide releases immobile iron and aluminum bound phosphorus into pore water as phosphate. This process may also be driving eutrophication of ambient soil organic matter increasing dissolved organic matter, which is also one of the factors affecting formation of MeHg. The sources of sulfate include (page 3B-2): (1) inputs from atmospheric deposition, (2) inputs of connate seawater (connate water = water entrapped in sedimentary rocks during the time of their formation) into canals, and (3) export of sulfate from the Everglades Agricultural Area (EAA).

Previous reports were focusing mainly on monitoring results of sulfates and mercury and not necessarily on the synthesis. This report presents, maybe for the first time, an extensive synthesis compiled from sub-reports of multiple authors who are members of a number of eminent institutes and agencies along with the team from the SFWMD. It can be stated at the beginning, that this effort to identify the causes and solutions of the methyl mercury contamination, the probable causes, interactions among the parameters, impacts on biota, and possible remedies (if any) represents the most comprehensive scientific endeavor since the mercury problem was identified more than forty years ago. The wealth of information presented in the report is overwhelming and it may take some time before it get into the literature. The results are also

cutting edge and new, in some cases not previously published anywhere. Therefore, the reviewers are absorbing this vast new information in real time and have formed their opinions on logic, past science and scientific feasibility. The report contains a wealth of past and recent literature for further study.

It is also evident that the authors of the chapter in the last few years, as the new evidence and knowledge has been gathered, have readjusted the new conclusions, as a good science should always do when new information is gathered. This is especially true with regards to the effects of sulfate concentrations. In the last WY Chapter 3b, the research by the authors and affiliated scientists produced evidence that the sulfate effect on MeHg accumulation in largemouth bass (LMB) is most pronounced in the water sulfate concentration between 1 to 5 mg/L. This was explained by the fact, supported also by the literature, that the higher, possibly toxic levels of sulfide concentrations produced by the SRBs may have retarding effects on the formation of MeHg, which led to a conclusion, expressed in the last year's review, that reducing sulfate levels below a certain low level, e.g. 10 mg/L or even slightly more, may be counterproductive. This was confirmed in several places in this year's report.

The sulfate content in water is linked to that in pore water of sediments which causes two problems:

A. Reduction of sulfates to sulfides in anoxic/anaerobic sediments by sulfate reducing bacteria (SRB) releases iron bound phosphate into solution and ultimately into overlying waters. This phenomenon is causing the managers to calling for development of measures to reduce sulfate concentrations. Sulfides in the sediment pore water are toxic and prevent formation of methyl mercury.

B. However, in some parts of the EPA and Refuge Areas, sulfate concentrations are low, below 10 mg/L. The research reported in Chapter 3B of the 2012 SFER indicated that sulfate concentrations in water above the sediment between 1 to 9 mg/L reduce the sulfide toxicity to SRB and lead to formation of methyl-mercury by the same SRB. The SFWMD scientists and consultants now have now enough excellent information for developing a scientific site specific criterion that could lead to sulfate TMDL and abatement focused on reducing sulfate loads to levels that would both control eutrophication and prevent formation of methyl mercury.

An important discovery in this year's report is the formation of MeHg in the sulfate range of 1 to 5-10 mg/L concentration (the "Goldilocks' window") depends on the concentration of *aromatic* dissolved organic carbon. However, these aromatics are typically produced by methanogenic microorganisms that compete with SRB in anaerobic environments. Elevated sulfate tends to enhance SRB activity over methanogenic activity. It is not clear which processes and how much aromatic organic dissolved carbon is produced in the wetlands and, specifically, in EPA, where the MeHg production is most pronounced. It is ironic that MeHg is produced in mostly oligotrophic zones of the EPA in areas which have the lowest sulfate concentrations. The dilemma is that increasing the sulfate concentrations might lead to more phosphorus release from the sediment and even greater rates of DOM availability. Thus linking methyl-mercury formation directly to sulfate concentrations only may be an over simplification. Indeed, factors such as SRB and methanogenic competition for available OM and their combined effect on P release and appurtenant OM availability may also warrant further research

That said the influence of the "Goldilock's window" and the parameters affecting it should now lead to development of a site specific bell shaped criterion for sulfate that would then result in a management model with new subroutines describing the sediment processes. This report demonstrates that this is likely the direction the researchers are headed, though it is not specifically stated.

Specific questions and comments by line number as appropriate.

Line 9: It appears that this year's report alters the reporting period from a water year as done previously to a calendar year. It is not clear why this was done and it also appears that prior year data was not adjusted for the change. While that might not have an effect on the conclusions, it certainly would adjust the statistical means, percentiles and outlier data should in the figures. Please explain the ramification of this change.

Line 229: the basis of site inclusion or exclusion could be better explained.

Line 247: Are the 32 south Florida sites the entire everglades area or a component, or a greater area. It is best to be consistent with pre-defined geographic locations.

Figure 3B-4: This documents the dichotomy of the problem whereby the lowest LMB MeHg concentrations occur in the areas of the Everglades which have the highest sulfate concentrations and LMB MeHg increase from north to south and is the highest where the sulfate concentrations are the lowest and most likely in the "Goldilocks' window. The same pattern was observed for other fish species (Mosquitofish and Sunfish).

Line 273-283: What is the typical lifespan of Gambusia? This could be a co-determinate of the influence of wet and drought years.

Line 285-301: There are clearly some lines missing in this paragraph making the meaning difficult to ascertain. The entire paragraph could use some re-wording for clarity.

Line 331-339: It is not clear how the reported data supports the conclusion of a time trend.

Line 342-388: The POR for these data is similar to that of the fish studies. While the number of samples is clearly smaller, the level of statistical analysis lags behind that of the fish studies. A higher level of statistical analysis would likely enhance the ability to make broad inferences of the collected data.

Figs 3-B-9, 3B-10 and 3-B11. Fonts are hard to read, especially the legend of 3B-10.

Line 397-452: It is not clear how the data supports the conclusion that wading birds are the primary vectors for Hg. Also the depth profiling data suggest that the impacted sites have always been higher in both parameters. Is these translocation of HG and N through the profile, if so how is depth translated to time. The stated conclusions may be supportable but are not supported as presented. In general this section is too short to convince the reader of the stated conclusions.

Line 531-537: This is an important section but how the "tagging" can determine the Hg sources by geographic region and not source (coal versus auto etc.) is not described.

Line 614: How are ambient versus new species delineated?

Line 629: While synthesis is generally expressed thought this chapter, how this study relates to the previous one is not addresses and they appear to be closely aligned.

Figures 3B-17and 3B-19: These show the maximum rate of MeHg formation occurs when the concentrations of sulfate in water are around 10 mg/L and, subsequently, the largest MeHg in the Mosquitofish occur at 5 mg/L. The bell-shape curve is more spread out than that for LMB fish in the last year's Chapter 3B. Figure 3B-19 does not show the effect of dissolved organic carbon or aromatic organic carbon compounds.

Lines 760-763: Reviewers have been making this suggestion for years. It is nice to see what appears to be the strongest language to date to this effect.

Table 3B-3. Values for median changes do not appear to match values for reported medians. How do these parameters relate if not directly?

Lines 869-888: It is not clear how connate water is separated from ambient values. If reductions in connate water are necessary, as suggested, how could this possibly be achieved? If connate water has always been there, how could it be "elevating" sulfate concentrations?

Lines 886-888: It would seem that a discussion of the ratios of sulfate sources should precede the analysis of changes in excess sulfate. We suggest moving this section and related figures to the beginning of the chapter section.

Figure 3B-21: It is a little confusing why both color and dot size vary? Do they somehow reflect different variations of the data?

Lines 931-940: AS written the chronological order of the sampling and data is a bit confusing. Does the USGS data come before or after 2001?

Lines 1004-1007: Not sure that the data in the figure supports the statement that the SUVA254 data shows less variability. Some simple statistics would back up this claim.

Figure 3B-28 (and supporting text): It seems highly unlikely that radial oxygen loss influences on pore water chemistry would show variation of the scale measured. Several studies in wetland environments do should radial oxygen variation, but at a spatial magnitude one, or two times smaller. Differences on the scale of mm would likely be more revealing, but admittedly hard to do in the field.

Lines 1124-1136: Are there significant areas of leafy green grown in the EAA? If not, it is not clear how these paragraph related to the bigger picture.

Editorial page and line comments:

Line 233: Watershed area??

Line 311: Should this be Figure 3B-6?

Line 527: spatially *and* temporally

Posted: 10/23/12 @ 8:01 PM EST

The authors; a team of experts from SFWMD (the District), federal agencies and laboratories and universities; are commented for a well-written draft Chapter 3B. The team of experts preparing the chapter and response to the reviewers' comments are highly knowledgeable about mercury methylation, have collected a superior data base and contribute significantly, year after year, to the state of the art of this difficult issue, in general, and for the Everglades ecological system in particular. The draft chapter, the reviewer's comments and author's responses to reviewer comments present a thorough and scientific discussion of issues and suggestions.

Due to the length of the review and response documents, in the following closure the reviewers will focus on the issues where there were differences of opinion or additional recommendations. If a comment is not specifically numbered below, the reviewers believe the authors' response satisfactorily addressed the original comment.

Comment #1: All agree that the interactions between mercury methylation and concentrations of sulfate and DOM are extremely complex and not completely understood, let alone quantified. However, several other factors must also be considered when contemplating strategies to mitigate the effects of Hg methylation. First, mercury in the food chain is not only a serious hazard to the greater Everglades ecosystem, threatening the survival of some top-predator wildlife (e.g. Florida Panthers), but is a demonstrable human health issue. Second, it is unlikely that Florida has the ability to further reduce mercury deposition to the environment since it is from (primarily) non-US sources. Third, our understanding of the issue has increased dramatically over the last several years (One need only compare the last several years of reports which we consider to be not only a

concise description of the of the issue as it applies to south Florida, but a fairly comprehensive review of the state-of-the-science, period). For example, it is now possible to state with a fair degree of accuracy the range of sulfate concentrations most conducive to Hg methylation and we are well on the way to characterizing the influence of DOM on this range, offering the potential for even more refinement. Fourth, it may take many more years before we "completely" characterize the interactions controlling mercury methylation in the Everglades ecosystem, and surely it will take many more years before any effective mitigation strategy starts to reap demonstrable improvement to the ecosystem and human health. Based on these premises it is clear that strategies to reduce the rate of methylation must be applied sooner than later and that the only viable mitigative option is to consider sulfur and DOM controls. So the issue becomes deciding when there is enough data to support a mitigation strategy, realizing there will always be more to know, balanced against the immediate need to mitigate the problem . There is also a difference between the scientists and managers as to the amount of information needed to arrive at managerial solutions. The TMDL guidelines recognized the problem and recommended an "adaptive management" approach. Justifiably, scientists are always thriving for more knowledge. However, managers have to begin developing plans for abatement of this serious problem. This can be done in adaptive fashion, starting with the current knowledge and adjusting the criteria and the plans as new science is being developed and models adjusted accordingly. This review team believes there is sufficient information to start formulating the required mitigation strategies that might vary by ecosystem sub-region so as to keep sulfate and DOM out of the "Goldilocks" range.

Comment#3: It might make sense to report the data on a calendar rather than water year for the reasons given in the response, but if this change is made then all previous data should be re-categorized in the same way so that there is a consistency of the long-term POR data. While the raw data would not be altered, statistical parameters would have to change due to the re-ordering. Whether this changes any conclusions cannot be determined definitively until the re-analysis is done.

Comment 13: We suggest this explanation be added to the text.

Comment 15: Indeed, Figures3B-17 and 3B-19 contain less information than Figure 3B-14 of the last year SFER. It would be worthwhile to consider this type of "three dimensional" representation in this report. As the new information on the most impacting forms of DOM will be discovered, new representations of the effects of various parameters on fish tissue concentrations will emerge.

Comment 17: A very thorough response. From the response it appears that the ratio of sulfate to chloride ions is the premise to indicate connate seawater intrusion as a significant source of sulfate to the ENP region. A subsequent paragraph then mentions that a major increase in all ions occurred once the ENP was hydrologically connected to the EAA via the L-67A canal. From previous reports we know that significant sulfur amendments occur in the EAA, so one would presumably see a significant shift in the sulfate/chloride ratio compared to purely connate groundwater. It is not clear whether this scenario is considered in the analysis of sulfate sources to the ENP.

Comment 22: We are not sure the change in text provides more clarification, but the comment was directed more at the research methodology and resulting interpretation that the wording. It is not clear from Fig3B-28 that there is a statistically significant difference in sulfide with the distance from the stem. The mean is a little lower at 5 cm but the SE bars overlap, and clearly no other values are statistically different. The point of the comment is that this might be expected because the distances are too large to capture any possible effects of ROL on redox chemistry.

Key Findings and Recommendation (SOW Task 3)

- The team and subcontractors working on the MeHg problem should continue research searching for a better causative model describing quantitatively the nonlinear relationship between the input Hg, sulfate concentrations and simultaneous effects of phosphate on DOC and DO and their effect on methylation.

- For the development of the model, further studies are needed to better understand what forms and concentrations of DOC are most affecting methylation.

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

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 4

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

Posted: 9/20/12 @ 07:25 AM EST

Chapter 4 presents an update on the nonpoint source programs mandated by the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA). These programs, as stated in the draft chapter, 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, atmospheric deposition, not addressed in this chapter, is greater that 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.

Apparently, the detailed review of the NP programs is performed every three years and WY 2011 (last year) was the off-year for such review. Hence the Chapter 4 of the 2013 SFER is somewhat limited in substance and results. Furthermore, Chapter 3A revealed that most phosphorus, a key pollutant causing the water quality (eutrophication) problems in the protected Everglades area, originates from difficult to control atmospheric sources. Apparently, the structural measures discussed in the chapter may have only limited or no impact on reduction of atmospheric sources. On the other hand, atmospheric P sources are regional and the only source is wind soil (or mine spoil) erosion. This reviewer recently noted that a large Bone Valley phosphorus surface mine, the only one in the US, is at the edge of the Everglades and Lake Okeechobee systems and could be most likely a source of atmospheric P, more than agricultural and urban sources reported in the chapter combined. There is no doubt that surface mines with heavy excavation machineries and no surface protection are a major source of atmospheric phosphorus. It is expected that the mining will run out of phosphate ore in about 40 years. So far no one looked yet at this possibility and the mine impact on the Everglades system is unknown. The reviewer encountered the same situation thirty years ago when dealing with sources of nitrogen limited and heavily impacted 500 km^2 Lagoon of Venice in Italy, where a nitrogen fertilizer manufacturing plant was located right on the shores of lagoon. At this time the effect of the Bone Valley mine on P inputs to the Everglades has most likely never been assessed.

The chapter also revealed that plans for NP controls have been mostly developed but they are still in the first phases of implementation. Nevertheless, the chapter reported a 71% (154 metric tons) reduction of the total phosphorus (TP) when compared with the predicted load. It is not clear what is meant by the "predicted" load. WY 2012 was hydraulically and climatologically an average year and in the 2011 and 2012 (calendar) years, droughts did not impact Florida.

The goal of the NP control plans is to minimize the undesirable flows to the estuaries and improve the quality of water delivered to Lake Okeechobee and the estuaries through source control programs, construction research projects, and water quality monitoring.

For the Southern Everglades, the source control program planning was incorporated into the Long-Term Plan for Achieving Water Quality Goals in the Everglades Protection Area (see Chapter 5). The strategy includes implementation of BMPs for phosphorus reduction, (2) regulatory programs, (3) voluntary programs, (4) educational programs, and (5) integration with local and regional water quality projects.

Apparently, because WY 2012 is the off year of the three year assessment cycle, the first twenty pages of the chapter describes the programs in a sequence of 1 to 3 pages status reports but does

not provide results or technical details. Hence, in this review the programs were noted with a reference to the last year report and reviews which described the intermediate results.

A general and repeating comment by this reviewer pertains to the use of units. The chapter is not consistent with units, it uses US and SI units in the same table and added units the authors invented. Repeatedly, it has been emphasized that the SFERs are read not only by US readers but has and should have an international audience. Furthermore, today scientific reports written by government agencies are consistent with units and most predominantly use metric units. Hardly anyone today uses ppm for mg/L. Some problems will be identified; however, in most cases the authors provide conversion factors.

Specific Comments

Page 4-4 – Table 4-1. This table summarizes the total (in metric ton) and specific (in pounds per acre). It identifies West Caloosahatchee and Lower Kissimmee watersheds with the highest phosphorus loads but apparently these loads may not be directly affecting the Everglades. The Kissimmee River is the main tributary of Lake Okeechobee. Apparently, only a small portion of the Kissimmee loads, after attenuation in the lake, are discharged into the Everglades but not at all times. The Caloosahatchee River discharging into the Gulf of Mexico and St. Lucie River discharging into Atlantic Ocean are the main outlets from the Lake Okeechobee.

Page 4-7, Lines 159-160. The chapter identifies voluntary and regulatory approaches used to control the nonpoint sources. The authors state that *the regulatory source control programs have been demonstrated as the foundation for effective strategies for reducing pollution loads*. The program is still in its initial phases of implementation, some results are not promising. Maybe this statement expresses premature optimisms.

Page 4-8 – Table 4.2 lists 9 nonpoint pollution control programs and 4 point source controls in the Northern Everglades. The NEEPP covers a phased, comprehensive protection program that included construction projects, source control programs, and research and water quality monitoring programs. The source control programs are carried out by the SFWMD, Florida Department of Environmental Protection (FDEP), and Florida Department of Agriculture and Consumer Services. The University of Florida is responsible for the development of effective yard fertilizer controls. The pollutants of concern are phosphorus for Lake Okeechobee and both phosphorus and nitrogen for the estuaries of the two rivers. Programs with the largest impact could be the programs of the Florida Agriculture and Consumer which are implementing Agricultural BMPs, animal manure applications and urban turf fertilizer rules.

Pages 4-13 to 4-20. These pages describe the status of the control programs in the Northern Everglades Watersheds. The programs are both voluntary and regulatory. The regulatory programs rely on implementation of Environmental Resources Permits that apply mostly to control of pollution by stormwater runoff and wetland dredging and filling wetlands. Voluntary approaches are mostly for agricultural sources.

Page 4-14, Table 4-3. This table lists the percentages of acres with permits. Some percentages seem confusing since the permitted total acreages for most watersheds are greater than 100%. This should be explained. Do some basins have two permits? What does it mean that basins are located in overlapping watersheds? Watersheds are separated by watershed divides and do not overlap. Provide a conversion from acres to hectares. Linking Table 4-3 to 4-1 is not clear, the majority of programs listed in Table 3 are located in the Caloosahatchee and St. Lucie watersheds which do not discharge into the Everglades.

Pages 4-15 to 4-17 briefly describe the WY 2012 and WY 2013 anticipated activities. They include performance measures development, nutrient monitoring, and implementation of BMP

regulatory programs. These programs are ongoing and very little specific results were provided in this section.

Table 4-4 lists the programs administered by the Florida Department of Environmental Protection. Again no specific results were provided in the section.

Pages 4-21 to 4-38 focus on the source controls by the Everglades Construction Projects (ECP) in the EAA area and C-139 basin but the bulk of writing is about the C-139 basin. Based on Table 4-1, the P load from the EAA is 63 metric tons and the unit load is 0.32 kg/ha (0.29 lbs/acre), those of C-139 basin are 15 metric tons and 0.22 kg/ha (0.2 lbs/acre) respectively.

The EAA basin is required to achieve a 25 % reduction of the loads when compared to the baseline period. The C-139 basin is required to meet and maintain phosphorus levels relative to the EPA defined baseline period.

Page 4-22 – Lines 507-508 report the P load decrease of 71 percent, when compared to the predicted load from the pre-BMP baseline period. This represents the seventh year the basin is in compliance. These results are also listed in Table 4-5. In the Table *ppb* should be changed µg/L. The average listed concentration of the flow from EAA area of 111 µg/L is still not acceptable.

The trend and target values of the percent P load reduction from the EAA area are shown on Figure 4-8. The confusing issue in this figure is the fact that the target was met as early as 1992 when no BMPs or STAs were implemented. Yet the P concentration in Lake Okeechobee and canal inlets into ENP remain high. Is the target too lenient, giving a false satisfaction to the agency?

Table 4-6 on page 4-25. This table is a units mismatch nightmare. Provide a conversion or convert (easily in Excel) inch to centimeter (cm) or millimeters (mm). Substitute µg/L for *ppb* which nobody is using today. There is no conversion between the two. Kac-ft is an invented mismatch of units. You cannot put together Kilo which is a metric prefix with acre-ft which is an archaic US unit. A proper metric unit for this magnitude of flow would be 10^6 m^3 and a proper US unit designation would 10^3 acre-ft. Conversion between the two is 1000 acre-ft = 1.233 x 10^6 m^3.

Page 4-28. Because the BMPs in the EAA are mostly voluntary, the list of activities describe briefly research, workshops, and development of regulatory programs.

Pages 4-29 to 4-37 focus on C-139 basin. Table 4-7 presents the results of observed load with the base line calculations. The basin load apparently meets the guidance limit. In the table again use µg/L instead of *ppb* without a conversion. The five year TP concentration from the basin is high, compared to the limit of 10 µg/L to be achieved in EPA, in spite of meeting the load limit.

Table 4-8 on page 4-32 has the same unit mismatch problem as Table 4-6.

Table 4-9 describes BMP levels and performance but does not specify what the "levels" are. The table is confusing.

Figure 4-10 – page 4-34. Change ppb to µg/L on the Y axis label, if possible. The figure shows very high concentrations before 2010 followed by a sudden drop in HY 2010 year. Can it be explained?

Page 4-35, Line 790. Table 4-1 listed the C-139 watershed area as 168,450 acres. Reconcile the two values.

Page 4-36, Lines 861-867 reports a disappointing low performance of aboveground impoundments for reducing phosphorus. This follows the last year results of more detailed reporting on wetlands and ponds removal efficiencies which were also disappointing (see last year review of Chapter 4).

Page 4-38, Line 932 -934. HY 2012 was not a drought year in Florida. See this reviewer's critique of Chapter 3A which presented the extent of calendar years 2011 and 2012 severe and extensive droughts. Florida was not affected.

Pages 4-39 to 4-44 describe the status of source controls in the non -ECP basins. There are seven small basins outside of EAA and C-139 basins with relatively small contribution to the EPA.

Chapter 4 Review Closing Comments

WY 2012 is an off-year for evaluation and synthesis of the nonpoint nutrient source programs. The chapter consists generally of a number of short progress reports briefly describing activities, ongoing and planned, but provides very little specifics. This makes the chapter confusing but one can assume that this chapter at the board review may receive less attention. This makes the chapter confusing. If possible, this chapter should provide the highlights of the last full assessment, summary of corrective actions that transpired from the last assessment, and then continue with the description of current and planned activities.

This chapter has a problem with the consistency of units, mixes both metric and US units.

Similarly to the last year evaluation, most of the source programs are in the implementation phases. The district is still rapidly working to implement the BMPs to achieve the objectives of the river and lake protection plans.

Posted: 10/24/12 @ 1:12 PM EST

Chapter 4 presented an update on the nonpoint source programs mandated by the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA). 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, atmospheric deposition, not addressed in this chapter, 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.

Because the detailed review of the NP programs is performed every three years, WY 2011 (last year) was the off-year for such review. Hence Chapter 4 of the 2013 SFER was somewhat limited in substance and results.

The authors provided satisfactory and thoughtful responses to all comments listed in the Panel Review. In many responses the authors noted and agreed with the reviewer's points. Such responses were made to Comments 1 and 2, 4 and 5,10, 15, and 27. Several comments by the reviewer pointed out the inconsistencies with the units (e.g., Comments #7 and 8, 18, 21, 23, 29). The authors either explained or accepted these comments and will rectify, when needed, the problem. However, the fact that SI and US units (e.g., metric tons and pounds/acre) are listed in the same paragraph or even on the same line should be corrected and more consistency with units would improve the quality of the chapter.

In the response to the **Comment #1** the authors agreed that the chapter did not address atmospheric deposition which constitutes 2/3 of the P loading to the Everglades National Park. This reviewer recently noted that a large Bone Valley phosphorus surface mine, the only one in the US, is at the edge of the Everglades and Lake Okeechobee systems and could most likely be a source of atmospheric P, probably more than agricultural and urban sources reported in the chapter combined.

Comments and Responses requiring a short commentary are:

Comment #3: The chapter also revealed that plans for NP controls have been mostly developed but they are still in the first phases of implementation. Nevertheless, the chapter reported a 71%

(154 metric tons) reduction of the total phosphorus (TP) when compared with the predicted load. The authors satisfactorily explained the question about 71% reduction of the P load, which apparently, is restricted to the Everglades Agricultural Area.

Comment #6: The authors provided a clarification that annual updates for EAA, C-139, and non-ECP basins were included in the chapter.

Comment #9: The authors satisfactorily explained the reviewer's argument about NP programs being mostly in the initial phases of development and implementation. The source control programs in the EAA have been in place since 1995, preventing 2565 (metric) tons from leaving the EAA.

Comment # 11: Satisfactory explanation on the multifaceted approach to control NP sources should be included in the chapter.

Comment # 12: The authors provided satisfactory explanation about the apparent inconsistency of overlapping areas resulting in total permitted areas being greater than 100%. They will also provide conversion factors for areas reported in acres.

Comments # 13 and 14: The authors will provide better explanation of the annual results of the NP programs. It was noted that a better explanation was provided in 2012 SFER. The reference to the 2012 SFER with a short summary should be provided in the Chapter.

Comment # 16: It is unclear whether or not the authors will switch the archaic concentration unit ppb to the widely accepted equivalent unit of µg/L. Also they did not comment on the inacceptable P concentrations in the ENP inflows of 111µg/L.

Comment # 17: It remains apparent that the agency seems to be satisfied with the NP loads from EAA which met the goals of 25% reduction in the late 1990s after "full implementation" of BMPs. The trend in the last decade is more or less steady. Do you expect any further improvement, considering the current status situation of eutrophication in the Everglades system?

Comment # 18: The authors should continue their efforts to rectify the mismatch of units. Some other chapter (e.g., 3A and B) are far more consistent.

Comment # 20: Same as #16.

Comment # 21 and 23: Same as Comment # 18 above.

Comment # 24: The authors will rectify the inconsistencies between areas reported on page 4-35 and in Table 4-1.

Comment # 25: The authors' response to the reviewer's comment on disappointing low performance of aboveground impoundments provides generalities but not a satisfactory answer.

Comments # 28 and 30: The authors promised to include current and prior year assessment in future SFERs, implying that this year' report is an off-year. The reviewer has some problem with this as each annual report should provide an overview of trends and year to year assessment. As recommended in the review of this chapter, highlights of the last full assessment and summary of corrective actions that transpired from the last assessment should have been provided followed by the description of current and planned activities. On the other hand, other issues such as budget and strategic allocation of writing efforts may have overwhelmed the teams.

Key Findings and Recommendations (Phase III):

- This chapter is an off-year report consisting mostly of a collection of short 2-3 pages progress reports. While this type of reporting has definitely an internal value and benefit, its value for outside readers and evaluators is diminished. A cohesive summary of past developments and

current trends and a position paper on what will happen next would have had more value even in an off-year.

- The problem with the atmospheric deposition of phosphorus emerged fully in this report as a side finding in Chapter 3A. However, this may be a major issue since, as reported in Chapter 3A, loading from land surface carried to the Everglades by water represents only about 1/3 of the total load. A study on identification of sources of atmospheric loads and their quantification, similar to that for mercury reported in the last year Chapter 3B of the SFER, is worthwhile. It was pointed out in the review of Chapters 3A and 4, the impact of extensive surface phosphate mining near or in the watershed should be assessed, followed by a proposal for controls. It is quite possible that the phosphate rich geological layer may extend into the watershed.
- SFER authors and technical editors should address the serious problem with units that are not even uniform between the chapters. At minimum, each chapter should provide a list of units used in the chapter which should be cross-checked by the technical editors of the entire report for uniformity and appropriateness and the list of units with their SI equivalents and conversion factors should be provided.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5

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

Posted: 9/20/12 @ 10:51 PM EST

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

Broad questions and comments that should be addressed:

This chapter is straightforward, very clearly written and easy to read. Much of the work is very descriptive, which is fine, but there are a few subjects that could be addressed in a more in-depth fashion, e.g. the importance of droughts.

As noted last year, the new more streamlined format of the chapter, starting with overviews of each STA followed by brief reports on selected ongoing research project and very brief overviews of recreational opportunities and the Long Term Plan, is a considerable improvement over previous formats. The reintroduction of brief overviews of the research programs, as recommended in last year's review, is a welcome addition compared to last year. While improvement compared to last year is noteworthy, a still-valid criticism is that there is, in general, a dearth of interpretation of the reported data. The second question above asks whether there are *other* interpretations of the data, but too often the document simply reports data rather than any scientific analysis of the data. It is difficult to offer an alternative interpretation when an original is not provided. This criticism is levied more on the performance reporting section than on the research summaries, where there is typically at least a paragraph of discussion and/or recommendations but these too are all very descriptive; some provide useful information but rarely is it put in context of what is already know about the topic from other work.

The panel appreciates that the authors reorganized the performance section first by STA then by parameter of interest as was suggested last year. Hopefully this format will allow the District focus more on better interpretation of results leading to better management of individual STAs and the entire STA infrastructure as a whole.

It is not clear why the PSTA cells in STA1E have been abandoned while the PSTA cells in STA 3/4 are still in an active research mode. I understand that the cells in STA1E were initially developed by the Corps but considering the expense to create them and the obvious scientific merit of replication, it seems odd that the cells in STA1E would simply be abandoned. Please explain the logic behind this decision.

With the build out of Compartment B and Compartment C nearly completed the capacity of the STA system will be increased significantly. This has to be considered a positive development as more water can be treated but it is clear that management of the STA under drought conditions has been problematic with the current area in STA. How will the District keep even more area wet during the dry season, especially in drought years considering the importance of keeping cells hydrated to prevent P concentration spikes upon rewetting? (See also the comment on line 798)

Considerable effort is put toward identifying the species of SAV in the various cells (it is a component of all STA sections and even some continuing research). A few years ago an explanation of which species are consider more and less desirable was presented, but this

information has not been relayed to the reader for several years. In line with the comment that more interpretation is required, it would be good to include a brief discussion of which species are (at least at this point in time) considered desirable and undesirable.

Drought clearly causes re-release of phosphorus and is a critical factor in the P removal efficiency of these systems. This is addressed often in the report but the potential mechanisms of P release are never really discussed in any detail nor are there attempts to determine the mechanism in the experimental section. There may be supporting sediment data in other reports, appendices, etc. but we would like to see either a brief summary of evidence supporting potential release mechanisms or direction to where the appropriate data are. For example, it is well-known that in lakes, oxic conditions in bottom waters and surface sediments favor the presence of oxidized iron (Fe^{3+}) which forms quite insoluble complexes with phosphorus minimizing P release during droughts, while reducing or anoxic conditions favor the much more soluble reduced Fe and release of P from sediments. Thus, drought might logically be expected to lead to oxidation of sediments/soils and trapping of more phosphorus. On the other hand, oxidation of organic matter would be more rapid under drought conditions and oxidation/decay of organic matter may release associated P and other nutrients. In addition, high calcium levels may result in Ca control of the P cycle with Fe being less important. In summary, we would like to see some discussion of why drought causes P spikes in these systems. This is particularly important because of the potential for increasing frequency of droughts as a consequence of climate change. It is not clear to me that there is any long-term plan developed or being developed to address this likelihood and its consequences for these P removal techniques.

The overview section should focus on fluxes rather than concentrations because it's not concentrations that matter. Perhaps this is done because regulations are written as allowable or target concentrations rather than load reductions?

Throughout the overview section- a little more detail about sampling frequency, storm events etc. in here would be nice; there are no standard deviation estimates or confidence limits given for any of the data in the section; if this is available in appendices or other volumes it would be good to refer to it here.

Specific comments questions by line number.

Line 32 and elsewhere: there are too many significant figures used frequently; 3 significant figures is probably realistic, not 6

Line 33 and elsewhere: the mix of acre-feet and metric tons is strange; surely the hydrology can be expressed in metric units too

Line 257: Figure 5.7 is mentioned here and again much later in the document, yet there is no Figure 5.7 in the document. It looks like it was simply omitted.

Line 280: The a long-term plan will need to address how to deal with increasing drought frequencies

Line 1033: it would seem to me that getting permits before building the system would make sense – why not?

Lines 540, 654, 678: It is not clear what land use was converted to what new land use.

Lines 555-556: Here and in a few subsequent section authors make claim that previous land use has an influence on performance. However no explanation as to why the District believes performance of areas that were previously farmed are not as effective as areas that were in a more native state. Some background to this statement is warranted.

Lines 557-563: In general it seems to be that he preferred flow path is to have water flow first through an EAV cell, then through an SAV cell (I am assuming this is a prefer BMP). Most data seems to support that configuration as increasing performance. Yet in this case flow through an EAV cell only consistently produces the lowest P concentrations. Can the District offer any possible explanations?

Lines 678: Here it is suggested that the land use change in southern portion STA2 cell 2 is from EAV to SAV. This change is not reflected in any of the figures (including Fig 3 of Appendix 5-1).

Line 798: It would appear that a way to keep SAV cells hydrated and let EAV cells dry out (at least occasionally) would be add structures or reconfigure flow to put water directly into the SAV cells without passing through the EAV cells first. It appears this might be happening in some cases, will these improvements be continued?

Line 842: Looks like the date is wrong here as Fig 5-27 shows a depth of approximately 2 ft. at this time.

Line 871: Was the water level too low for the boat?

Line 882: The entire section focused on STA 5/6 is not as clear as the previous sections. In numerous locations components mentioned in the text (such as flow-ways, cells etc. are either not identified in any figures or are sometimes inconsistent (north flow-way versus Flow-way 1 for example). It is nearly impossible to follow this section if the reader cannot identify which geographic sections the text refers too. Similarly Compartments B and C are often mentioned but identified in the figures for the appropriate STAs.

Lines 967, 975, 976, 978: It appears that some of the figure numbers are incorrect, but it is hard to tell which are off. Clearly the text does not match the referenced figure

Lines 979, 1077: What is an "environmentally sensitive area"?

Lines 966: Since there is more than one cell labeled 2,3,4, and 5, the STA number needs to be included in all nomenclature (See comment on line 882)

Lines 1008-1012: With the exception of STA5 C1A, it is hard to see any obvious trend in the data. My guess is that there is some threshold dryout period that must be crossed before a real effect is apparent, as perhaps seen in STA5-C2A. Rather than taking an arbitrary concentration level (50 ppb), why not use a relative concentration for before and after drying as a threshold for a TP spike. Such a criterion might provide more data to analyze over the POR.

Line 1055: Though there is some detail in earlier reports about the herbicides used and toxicology issues, but it would be useful to at least identify what is being used

Line 1200: The rationale for this experiment is not clear. Why should periphyton matter? Its biomass or potential biomass couldn't be great enough to trap significant P could it? Some calculations showing its relevance to support the experiment would be useful.

Lines 1274-1277: The meaning of these statements is not clear; perhaps a rewording is in order.

Lines 1327-1331: A more detailed plan figure would help with interpretation of these results.

Lines 1371-1467: While the study is obviously still continuing and the issue may be due to start-up effects, it is a little disheartening that the mesocosm studies results seem to be running counter to what has been observed in the full scale cells. One could argue that the anomalous results basically show that mesocosms often don't work because setting them up disturbs the system they are trying to represent too severely. If these trends continue, the study will highlight more questions than answers.

Line 1415: to comment on the mesocosm experiments in detail, I think

Lines 1468-1543: It is hard to justify the conclusions that are drawn from the data. There appears to be little to no correlation between density and dry out (see Fig. 5-51 versus Fig. 5-50). There is no change in density due to the dry-out of Feb 2011-June 2011. This study needs to be either conducted under more controlled conditions to test cause and effect or be abandoned.

Lines 1544-1730: This appears to be one of the more comprehensive studies that the district is conducting and is in general an example of how to conduct more of the experiments. The section of the chapter is quite hard to follow due to an excessive use of acronyms and the use of cells numbers rather than a physical description of the plant type effect. The point appears to be differences induced by (primarily) SAV versus EAV dominant communities. Why not use that designation as opposed to cell location.

Line 1734: What about Fe and Ca in the sediments; these must have major roles in the release and trapping of P?

Lines 1829-2066: Both of these studies appear to be of high quality and we look forward to seeing the more fully detailed papers that will ensue.

Figure and table comments:

A generic comment on many figures is that the fonts for legends and axis labels are too small to read, often even when read on a monitor where they can be expanded in size quite easily. Printed at 100%, there are virtually indecipherable. More egregious examples include Figs. 5.5, 5-23, 5-24, 5-25, 5-63

Fig 5-6: The series are not labeled or defined.

Fig. 5.6 and other related figures: the manner of plotting the time series data doesn't make sense, at least the 12-month moving averages. These are normally plotted at the mid-point of the moving time period, i.e. at 6 or at 7 months for a 12-month moving mean. The first point is plotted at the 13^{th} month suggesting that it is the average of the preceding 12 months, but the numbers don't seem to work. (See also the comment on Fig. 5-26)

Table 5-8: The font size changes on the last two lines

Fig 5-26, 5-34: There are a few months without symbols for FWM TP data. It could be the data was simply omitted or maybe they were months in which there was no outflow? If the latter, this should be noted in the caption. Additionally, how is zero flow accounted for in the calculation of a12 month moving average? Several options exist and the one taken should be noted.

Fig 5-31: This figure clearly demonstrates the improvement to SAV coverage when a grid the emergent vegetation is put across SAV cells. Is it feasible to continue this practice in all SAV cells? What timeline is considered for this type of improvement?

Fig 5-36: The legend should read STA5 Cell 3.

Table 5-11: What is "section 2"?

Table 5-12: This is a nice way of presenting the various scientific studies underway. Also SRP was measured in some of the applied scientific studies but never in the routine monitoring?

Table 5-13: TP settling velocity of 12 m/yr is exactly the historic literature value for lakes. Curious coincidence or universal constant?

Figure 5-43: The figure caption is confusing. Though ultimately deciphered, as worded the caption implies there are three transects when there are obviously more.

Editorial comments by line number:

Line 411: change *STA1E* to *STA1W*

Line 511: change *than any of the other STAs* to *of any STAs*

Lines 607, 615: I believe *2001* should be changed to *2012*

Line 613: Obvious typo here.

Line 780: change *and* to *to*

Line 788: causing *the* previously desiccated

Line 1101: Change *degraded to lowered*

Line 1172: Figure 5-4*1*

Line 1209: rating *curve* was

Line 1274: Should this be *areal*?

Lines 1476 and 1487: Some typos in here.

Line 1578: Figure 5-5*3*

Line 1579: use ft. rather than ‘

Summary and Recommendations:

For the most part, this chapter is a straight-forward accounting of what has happened in the past year with regard to the stormwater treatment areas. In terms of the results achieved, there seems to be very little that is controversial. At most of the sites, results have been adequate, while two sites performed poorly and did not achieve their TP target levels. It is important to understand why these two failed and how future operation might improve performance and/or maintain it in the long run. While the chapter fulfills the requirements for an accountability review, the presentation leaves something to be desired from the Technical review perspective.

The panel believes it is time to carve out a more manageable experimental system somewhere within the STA system where the scientific approach can be applied to replicated experimental units large enough to representative of the field scale, but small enough to not be influenced by currently uncontrollable variables such as hydrology (water depth, dry out) soil type etc. Data from these experimental units would provide a baseline of “best achievable” TP removal results and used to optimize controllable inputs such vegetation type, hydroperiod, water depth etc. Though the initial cost of such a system might be high compared to continued annual expenditures of monitoring of current full scale system, it seems highly unlikely the current approach of monitoring such an expansive, variable system, even if conducted over another decade or more, will ever produce the desired output of knowing what controllable factors can be manipulated to optimize performance. Monitoring of a controllable experimental unit, with multiple cell sizes on the order of a few acres, would ultimately lead to better operational decision making.

Posted: 10/23/12 @ 7:59 PM EST

Due to the length of the review and response documents, in the following closure the reviewers will focus only on comments and responses the reviewers believe additional actions or clarifications are necessary. If a comment is not specifically numbered below, the reviewers believe the authors’ responses satisfactorily addressed the original comment.

BQ2: The reviewers are encouraged that the District is developing a scientific plan that we assume will help to prioritize and coordinate the various research projects that too often appear to be run on an ad-hoc basis. It is our hope that this plan would consolidate theses "projects" into fewer but more comprehensive "research programs" that coordinate small research objectives (projects) to address information gaps at a more systematic level. After more than a dozen years of STA operation it is time for District to shift research from addressing short-term non-compliance issues to long-term operational guidelines that meet objectives on a year-in year-out basis. This comment is not to suggest the District has no long-term plan, but that it needs to continue to focus its research on STA performance on more fundamental questions and with a longer timeline than it has done in the past.

When might we see a version of the research plan? See Recommendation #3.

BQ5: Several comments and responses are focused on the issue of drought influences on performance. FEB's would seem to be helpful at minimizing these issues but the response opens a whole new set of simple questions such as; when, how and where will these be built? It also suggests to one reviewer a more fundamental question that has been raised to authors of other chapters. At what point do we stop utilizing structural repairs to fix a damaged ecosystem, with the very real possibility that the repair of one problem in one location just creates a new one in a new location? In this case, construction of an unnatural FEB likely damages that local ecosystem. We realize questions of this nature are probably above the District's purview to address directly, but someone needs to pose this type of question to people capable of making changes in approach to solving ecological problems. If not the managers and scientists closest to the problem who else could that be? See Recommendation #2.

BQ9: The reviewers are confused by the response comment that no standard deviations can be computed from yearly or multiple year data, though the last sentence seems to indicate that some form of statistical metric will be computed, at least for some of the reported data.

Line 555-556: The response comment is greatly appreciated. The offering of hypotheses and better interpretation of reported data is exactly what we would like to see more of in the report. See Recommendation #1.

Line 979, 1077: From the response, it is clear that these areas have some archeological significance and are culturally important to (most likely) native peoples. We can appreciate the sensitivity on several levels and the need to omit specifics in this document, but for clarity's sake could these be called "culturally sensitive areas"?

Line 1371-1467: Why would this study use STA outflow water as an input to the mesocosms if (one of?) the primary goal is to compare vegetation types in a proof of concept that native plants such as sawgrass are more effective at removing P than the currently used cattail and SAV? To this reviewer, the most logical input would be the water entering the STA, not leaving, it unless the goal is to use these plants as a final polishing step after the STA treatment. This does not appear to be the case and points to the need to complete a Scientific Plan to better coordinate research objectives into a comprehensive program.

1415: Sorry for the confusion, this comment was absorbed into the previous one, but was not fully deleted.

1468-1543: We all agree the District needs more controlled experiments and hopefully the Scientific Plan will address this. This is an example of why an ad-hoc approach (water level is being drawn down, let's see what happens to cattail density) typically leads to inconclusive results. If the cost of the study is nominal, perhaps we can get lucky and get meaningful results, but money is better spent on a more thoroughly thought out study.

Figure 5.6: To use a preceding 12 month moving average is not the typical way of employing moving averages, but so long as it is stated, the reader can interpret the results. Please consider using a more standard format in future reports. More importantly please check your calculations

of the moving averages. A rough estimate of the first value in Fig 5.6 suggests that the number should be about 70 rather than the number shown which appears to be about 90. Also see the related next comment.

Fig 5-26, 5-34: The query is only partially addressed. How are months with no flow accounted for the calculated moving averages? Is a value of zero included in subsequent averages? Does the average utilize only 11 (or less) months until the no flow data is more than 12 months old? Or does it use the preceding12 non-zero values (and hence a record of more than 13 months)? Interpretation could be considerably different depending on the approach taken.

Summary and Recommendations section: The authors deserve an apology from us for this section, as it was inadvertently left in this year's comments when last year's file was used as a template. That said, the second comment is still germane and the author's response reiterates the point that better use of controlled experiments in the new Scientific Plan is warranted.

Key Findings and Recommendations (SOW Task 3)

- The introduction of completed and (when appropriate) continuing research projects should contain a statement of the hypothesis(es) or at least the specific goal and objective the project was designed to address, follows by a very brief description of the methods and an overview of the results. More emphasis should be placed on an interpretation of the results (when projects are completed) and link that discussion to the stated hypothesis or objectives. This will allow for better evaluation of the specific project and the overall Research Plan.
- The near completion of Compartments A and B will likely affect the overall STA performance positively, especially in wet years. However the extra area that should be kept inundated will become a more challenging management feature during the dry season, especially in drought years. Structural mitigative strategies such as FEBs may offer a partial solution, but may also create new unintended negative ecological consequences. Therefore research on the effects of wetting and drying cycles on phosphorous and sulfate releases (and appurtenant methylation of mercury) should be continued and expanded. This research should be coordinated with the research efforts described in Chapter 3B.
- The District is encouraged to accelerate the development of a new overarching Scientific Plan to better coordinate the various research projects focused on the STAs. This plan should incorporate a longer range view and therefore address more fundamental questions with a more controlled experimental approach and, as suggested last year, consider organizing around a more comprehensive modeling focus which should better identify cause and effect relationships.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 6

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

Posted: 9/17/12 @ 8:40 AM EST

Dodds General Comments

This is a fairly thorough and well written chapter. The organization is clear and the overall amount of information presented is good. The document puts the current year in perspective with past years, giving strength and context to any management decisions.

Dillon General Comments

This chapter contains a great deal of useful and interesting data and is I think better organized than the comparable chapters in earlier reports that I have read. The overview provided in Table 6-1 is particularly useful and should be a feature of each annual report. Another excellent change this year is consideration of the past 4 years with respect to hydrology rather than just the past year; in my opinion, this should have been the norm as it is clear that parameters such as water table elevation are affected by longer-term hydrology than a single year. This kind of multi-year trend analysis should be used to assess the role of hydrology with respect to many other aspects related to the status of the ecosystem, both in this chapter and in other chapters.

I have two general comments about the chapter that would improve the reporting in subsequent years. The first is that the importance of hydrology, specifically drought episodes and major storm events, is apparent. These events control the most significant chemical and biological changes in the Everglades and surrounding areas. Climate change is certain to change the frequency and magnitude of these changes. It would be valuable to undertake a study to come up with some idea of what those changes are likely to be because they are almost certain to have major impacts on the Everglades. There are methods available to downscale the GCMs to local areas; the data needed for the downscaling clearly exist for south Florida. Various standard scenarios exist based on changes in emission of greenhouse gases which can be coupled with the downscaling to give an idea of what the expectations are for the future for key hydrologic parameters. Some of the targets for various ecosystem properties may need to be reconsidered in this light; they may not be feasible under future climate scenarios. In short, there is a need to be proactive rather than reactive to this issue.

The second point is that the data presentation would be much stronger in many places if there were some error estimates made. For example, the nesting bird data are interesting and in some cases the year-to-year changes are so great that there can be no doubt that major alterations have occurred. In other cases, it is unclear whether the reported changes (e.g. line 448, 9% decline in great egret nesting) are within measurement error. I'm sure that methods exist for estimating the error associated with these measurements.

Specific Comments Dodds and Dillon Combined

In general the summaries avoid giving specific numbers, and might be improved with at least a few more example numbers.

Line 6: what is the difference between landscape and ecosystem ecology?

Line 20, page 6-1: "These conditions were positive for the Everglades and may indicate that local 19 meteorological influence on rainfall patterns may be beneficial despite global circulation

patterns 20 that would point toward drier conditions for South Florida." This is a strong statement that might need to be qualified or removed. Does this mean that biological and chemical influences of overall drier conditions that are predicted by regional application of global climate models may be mitigated by more intense storms?

Line 146, page 6-6: These statements are a bit on the non-technical side. What is a fantastic year? An average % population increase could put this in perspective.

Figures 6-1 to 6-7 are nice figures and really lay things out well. They also highlight the benefits to longer-term analyses of data

Line 357: What is the rationale for these exceedances of the Florida ay MFL criteria. These are largely natural events so it is not clear why it should be considered an exceedance. Is it an exceedance because measures are supposed to be taken relating to managing flows that would prevent this?

The pilot experiments using enclosures for fish movement are promising. Using enclosures of this small size could cause problems if that is what is planned for the actual studies. There should be fairly good data on ranges of movement for these species, if not, some larger scale measurements would be warranted. Given that the few fish tested moved freely throughout the enclosures, it is possible that the enclosures constrained the animals or offered cover where it would not normally occur.

The germination studies are good and well described

Line 727: again, another place where error estimates are needed to determine if this is a real change or a measurement artifact. The fact that 20 species disappeared in a year suggests that this is dependent on the intensity of sampling.

Line 958: If cattails provide refuge from predators this would also explain these results.

Line 1195: Beta diversity is simply a comparison among habitats. The index may reflect temporal patterns using space for time substitution. This needs to be a bit more clearly explained here. Also if species identities matter, then clustering methods or PCA might be a better choice for analyses.

Line 1271: It might be better to collect integrated samples over whatever the depth at the sampling site is rather than at 0.5 m only, which may not be representative of the water column at least at deeper sites

Line 1282: This is an awkward sentence.

Line 1332 and fig 6-31: would be much more useful in absolute units rather than as relative fluorescence values; I thought ground-based measurements were available that would allow calibration of the RFU?

Line 1440: Generally, sediment core incubations rarely provide unequivocal data; there is always some disturbance of the surface sediments, the overlying water, etc. The very short (4 hr) incubations may reduce these problems, or may make them worse

Figure 6-35: A panel with N to P ratios would be nice here. This might be important as it could indicate temporal changes in N and P limitations.

Figure 6-37: An analysis of correlation between ammonium flux and oxygen flux would be interesting.

Figure 6-39: The "3d" format in excel plots is not very professional.

Figure 6-45: Nicely illustrates major points.

Posted: 10/23/12 @ 7:59 PM EST

1. **Accountability Review.**

This was not tasked on this chapter**.** However, both reviewers did agree that the accountability was better when results were put into long-term perspective.

2. **Technical Review.**

Summary of Key Findings

- In general the reviewers felt that this chapter was well written and most of the suggestions for improvement were relatively moderate. We stress that this chapter is impressive in that it describes a very complex set of measurements and experiments in a limited amount of space.
- The fundamental approach of the studies is sound and the gaps or flaws are minor. The interpretations of the data seem sound and for the most part the document does not over reach given the results presented.

Recommendations

- Future work should include downscaling of global climate change predictions and relationships to management goals.
- Future reports should keep the long-term perspective in mind.
- More emphasis should be placed on error estimation and reporting specific numbers in this and future documents.
- Future work on fish movement should carefully and explicitly consider enclosure effects in experiments.
- Further experiments may be warranted to describe sediment nutrient fluxes and this is a central issue in understanding nutrient dynamics and efforts to control nutrient loading to the system.
- The results reported here are of basic scientific interest with respect to wetlands and the researchers are highly encouraged to continue publication of the results in peer reviewed literature as this will only increase their value to basic science and their power to inform decision making by local management and stakeholders.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 7

Level of Panel Review: Accountability

Reviewer: W. Dodds (AA)

Posted: 9/13/12 @ 10:12 PM EST

This chapter is well done. I do not have many comments. It seems as if SFWMD is doing what they can to control non-native species. It is especially good to see cross agency coordination on control of some species. As the chapter is mostly descriptive, there is very little to comment on here other than it is very clear and gives the information in an easy to access format. The chapter serves as an excellent reference for some of the other chapters.

Posted: 10/25/12 @ 9:07 PM EST

The document seems defensible and the information is presented in a logical and straightforward manner. The findings are clearly linked to management goals and objectives, and appear realistic about what can be accomplished given resources at hand.

Summary of Key Findings

- Invasive species are the most permanent form of pollution, as there are few if any examples of eradication of most well established pests.
- The district is commended for continuing this difficult work in a scientific and practical fashion in the face of limited resources.
- The document accurately reflects the difficulties in control, and absolute requirement for cross-agency cooperation in keeping unwanted species at bay.
- The document clearly delineates the status of these species by region with a convenient color coded system.

Recommendations

- The district should continue efforts to control invasive species introductions as well as their spread through ongoing efforts in coordination with other agencies.
- While research on biological control is costly, it is entirely essential in efforts to control these species, as in the long run it is often the least expensive control option. That said, the long list of biological control agents gone awry indicates that substantial research funding and careful study is necessary to use biocontrol effectively.
- While control of many of these species is not glamorous or easy work, finding funding to continue this control is essential to maintain the South Florida native ecosystems in conditions under which they maintain their biotic integrity, conserve endangered species, and continue to provide the ecosystem goods and services they provide to humanity into a sustainable future.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 8

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

Posted: 9/17/12 @ 08:01 AM EST

General

This chapter includes detailed and comprehensive information about measures implemented in water year 2012 under the Lake Okeechobee Watershed Protection Plan to reduce phosphorus and nitrogen loading to the lake, about the nutrient status of the lake including external loads as well as in-lake chemistry, and about the ecology of the lake with respect to submerged and emergent aquatic vegetation, phytoplankton, macroinvertebrates, fish, wading birds, herpetofauna and the Florida apple snail. There is also a minor discussion of a few aspects of the ecology of Lake Istokpoga.

In general, the chapter is well-written, with clear and relatively concise writing. The results are explained in adequate detail. The conclusions drawn are supported by the data presented. In summary, I thought that this was the best version of the chapter on Lake Okeechobee that I have read since I have been a reviewer of the report.

The greatest concern related to this section as remains the progress 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. I understand that the target load is a legislated requirement, but in the end, it may simply not be feasible, and if not, it may be time to, in effect, start re-negotiations. Unless a clear path to meeting this target load is apparent (and described in detail in the next year's report), then realistic scenarios for the future should be developed. This is particularly relevant since the potential for very large internal P loads makes the desired changes in water quality even more difficult. I would encourage more intensive efforts in future to quantify the nutrient load-trophic status response relationships through modeling efforts so that what to expect with different target loads could be identified clearly.

On the positive side, the lake TP concentration has shown some recent improvement. Although the values reported for 2011-2012 are still well into the hyper-eutrophic category, they are lower than previous years. The five-year moving average, however, (Figure 8-14) is somewhat less promising with the mean still higher than 15 years ago. The complex mix of hydrology and nutrient dynamics explains the high load-lower concentration situation, and I'm sure that this is/was predictable with the appropriate models. It is also a good reason to continue with using 5-year average values for many parameters.

Hydrologic events, particularly storm events and drought episodes, are clearly of great importance in determining both the external loads (and probably the internal loads) and the lake's response to these loads in terms of trophic status parameters such as chlorophyll concentration, water clarity, etc. Climate change is certain to change the frequency and magnitude of these events. It would be valuable to undertake a study to come up with some idea

of what those changes are likely to be because they are almost certain to have major impacts on the lake. There are methods available to downscale the GCMs to local areas; the data needed for the downscaling clearly exist for south Florida. Various standard scenarios exist based on changes in emission of greenhouse gases which can be coupled with the downscaling to give an idea of what the expectations are for the future for key hydrologic parameters. Some of the targets for the lake's trophic status properties may need to be reconsidered in this light; they may not be feasible under future climate scenarios. In short, there is a need to be proactive rather than reactive to this issue.

In summary, it is obvious that a great deal of high quality data have been collected on the lake and its tributaries, and that these data have been described and interpreted clearly and correctly in almost all cases. It would however be beneficial to include more in-depth analysis particularly with more conclusions drawn in the next report. A number of remedial projects have also been clearly presented; most of these have been successful to some extent. A brief indication of where these watershed projects are going in future and what their ultimate potential is in terms of TP reductions would be useful, although I accept that funding considerations largely dictate this.

Specific

Line 111. Blue green algae is an old fashioned term, cyanobacteria should be used

Line 129. Scientific names of all species should be used at least first mention in the document

Line 195. Exotic plants means non-native?

Line 338. The document jumps between metric and English units here and elsewhere, eg. acres, and should stick to metric units

Line 366. The potential for nutrient removal by macrophyte harvesting has been raised in the past. Macrophyte harvesting must be less expensive than dredging. Furthermore, particular areas with undesirable plant species could be targeted. My understanding is that repeated harvests not only remove significant nutrients but also leads to deterioration in the targeted plant's condition.

Fig 8.7. The approach is very interesting here, and seems valuable. The actual years need to be defined in the legend.

Line 565. The delay in implementation makes no apparent sense. Could the rationale for this be explained more fully in terms of what the delay could achieve.

Tables 8.1 and 8.2. There are too many significant figures in these tables. A maximum of 3 is justified here. Also, a figure demonstrating these changes would be a good addition.

Line 622. An in-lake goal for TN would be useful. While P is probably limiting in most parts of this lake, there are clear effects of alteration of TN:TP ratios, including dominance of certain types of cyanobacteria, some of them potentially toxic. If maintaining biotic integrity is a goal, getting close to historic conditions of TN as well as TP is more likely to help reach that goal.

Line 721. The huge flux during the storm leads to the need to evaluate climate change and the predictions of more frequent extreme events

Table 8-5. The TP concentrations at some sites are higher than treated sewage. Although it sounds extreme, it may be viable to treat some of these inflows as such, with full-scale tertiary treatment. It is at least worth considering the cost relative to the other methodologies that are being attempted.

Line 776. A non parametric pair wise comparison technique throws out most of the temporal information. There is also concern that running multiple pair wise comparisons is not corrected for repeated measures, e.g. Bonferroni.

Table 8-11. The TP load is still far from the target; this table shows clearly that loads are hydrologically driven which points to the use of mean volume weighted concentrations when looking at inflow contributions and changes over time

Line 867. The calculation of sedimentation coefficient is excellent; this is useful information for modeling the response of the lake to its load.

Line 972. Can this mapping be done more efficiently with remote sensing methods with some ground level controls rather than detailed ground level mapping?

Line 998. It is good to see the attempts to standardize the SAV measurements; such efforts are most useful and if there are other data sets for other parameters where this can be done, I would strongly encourage it. Ultimately, the long-term nature of the data sets for this ecosystem will be invaluable.

Line 1194. Has there been any analysis of wind with respect to bloom formation? Calm conditions should favor cyanobacterial blooms.

Line 1204. The section on phytoplankton is well done; on periphyton, a broader perspective is warranted. Questions such as how much does the periphyton contribute to the total algal biomass in the lake; is it important relative to the SAV in terms of nutrient uptake could and should be addressed.

Line 1331. If I understand the methods used for the fish trawl, this was done only once in the year. This is probably not very good data to compare year-to-year, at least not without some estimate of within-year variability.

Overall, well done!

Posted: 10/29/12 @ 07:43 AM EST

Chapter 8 was reviewed on the basis of the technical quality of the work carried out as part of the Lake Okeechobee Protection Plan and the interpretation of the results of these studies.

The central questions addressed in this review were:

Are the findings and conclusions supported by "best available information," or are there gaps or flaws in the information presented in the document? Are there other interpretations of the data and other available information that should be considered by the authors and presented to decision makers?

Chapter 8 includes detailed and comprehensive information about measures implemented in water year 2012 under the Lake Okeechobee Watershed Protection Plan to reduce phosphorus and nitrogen loading to the lake, and about the nutrient status of the lake including external loads as well as in-lake chemistry. It also addresses the ecology of the lake with respect to submerged and emergent aquatic vegetation, phytoplankton, macroinvertebrates, fish, wading birds, herpetofauna and the Florida apple snail. The chapter is well-written and is technically sound. The conclusions drawn by the authors are well-supported.

In addition, the issues raised by the reviewers were addressed very thoroughly by the chapter's authors in their reply to our comments. They have done an excellent job of addressing the concerns that were raised.

Key findings

1. Several projects under the Lake Okeechobee Watershed Construction Program have been advanced significantly and are, or are expected to, contribute significantly to reductions in total phosphorus loading to the lake.

2. Several other research and assessment projects have been carried out successfully in 2012, and have generated valuable scientific information. Some of these are continuations of ongoing projects while others are relatively new initiatives.

3. Despite all of the positive steps taken, many at great financial cost, there has been little progress towards reducing the TP load to the lake nearer to the target value of 140 metric tons.

4. However, despite lack of reductions in loadings, the phosphorus concentrations in the lake have declined, although the lake remains in a hypereutrophic state.

5. The importance of significant hydrologic events including drought and major storm events in governing nutrient loadings and the lake's response to loadings was apparent.

6. Although the phosphorus level remains very high, algal blooms were infrequent, with cyanobacteria generally inconsequential.

7. The fishery appears to be in good shape and recovery following the hurricanes of 2004-2005 continues.

8. Wading bird foraging and nesting success rates were poor in 2012.

9. Submerged aquatic vegetation (SAV) increased in areal coverage although this was due largely to increases in an undesirable macro-algae species, *Chara*. SAV is being replaced with emergent vegetation in parts of the lake, which is unlikely to be beneficial to the ecosystem.

Key Recommendations

1. The greatest concern related to this chapter remains as the lack of progress made towards reducing the TP loading to the target value of 140 metric tons/year. Despite very substantial efforts, the load is similar to what it has been for the past several years. A clear plan for meeting this target load is needed and should be described in detail in the next year's report.

2. There is a significant potential for very large internal P loads occurring in future that will make improvements in water quality in the lake even more difficult to achieve. More efforts to quantify the potential for internal loading and the factors that affect it are needed. Additional investigations into the feasibility of in-lake sequestration of phosphorus should be explored in more depth.

3. Improved nutrient load-trophic status response relationships through modeling efforts are needed. FDEP and SFWMD should consider updated trophic status models that include, among other factors, the likelihood of formation and persistence of cyanobacteria blooms. While nitrogen is not the primary management target, efforts to control N, particularly if they help P control, should be considered valuable as increases may alter biotic integrity even in the absence of stimulation of total algal biomass.

4. Hydrologic events, particularly major storm events and drought episodes, are clearly of great importance in determining both the external loads and the lake's response to these loads. Climate change is certain to change the frequency and magnitude of these events. Because of their potential to have major impacts on the lake, a study should be undertaken to develop an understanding of how often and of what magnitude those events are likely to occur.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 9

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

Posted: 9/23/12 @ 6:17 PM EST

General

This chapter is divided into major sections that focus on the technical details of the Kissimmee River restoration project, on hydrologic conditions in the year reported on, and on the Kissimmee River Restoration Evaluation Program including consideration of hydrology, chemistry and a few aspects of the biology of the system. There are also minor sections on the Kissimmee Basin modeling studies and the Upper Kissimmee Basin studies.

The chapter provides a succinct overview of the activities related to the restoration of the Kissimmee River to a pre-channelization condition as well as the Basin's current ecological health. The premise is that ecological health has been severely compromised by channelization prior to approximately 1970 and that removal of the middle section of the C38 canal and restoration of the original river channel will reconnect the restored channel to its associated floodplain. It is intended that this will allow the associated biological communities to revert to something similar to those of the pre-channelized basin.

In general, this chapter was significantly easier to follow than the comparable chapter in previous annual reports. Nevertheless, there could still be some improvements. Table 9-3 provides a very valuable overview of what is being monitored and where to find information in previous reports. However an additional table using a similar format that spells out what each expectation number is would be equally valuable as these expectations very succinctly define the goals of the restoration project. The chapter is well-written, with clear and concise writing. The results are nicely organized and explained in adequate detail. Most of the conclusions drawn are supported by the data presented.

One general problem is that there appears to be a potential conflict between some of the expectations of the Kissimmee River restoration project, specifically between expectations for the period of floodplain inundation (Expectation #3) and dissolved oxygen (Expectation # 8). The data from this year's high runoff events clearly demonstrates that high flows after low flow periods decreases dissolved oxygen concentrations to below that for fish survival, a pattern seen previously (lines 339-352). Yet that diversity in the period of inundation is exactly what the project is trying to achieve. Are these expectations incompatible? If so does the District have the authority to alter some?

Specific questions and comments

Line 16-17: What are the long-term implications of the funding shortage and the reduction or loss of many of the non-KRRP projects?

Line 73: Climate change is almost certain to have major impacts on precipitation and temperature in south Florida. What are the consequences in terms of meeting expectations for the seasonal flow pattern and staying within the desired water levels? Some general comments on this would be valuable, although a detailed analysis probably requires a separate additional project.

Line 86: The oxygen results are very promising. This is a critical parameter in terms of many the biological communities in the basin, and it is important that the oxygen-related criteria were met or almost met despite the hydrologic conditions not being optimal at all times.

Line 107: The floodplain storage of phosphorus is very important. The measures being implemented to reduce the Lake Okeechobee phosphorus to its 140 metric ton target are not fully working and are not going to be successful. Unless some new approach or some different process that hasn't been considered takes effect, the loading target will not be met. This floodplain storage may be the only way the target can be met and so must be fully explored.

Line 112: The different response of nitrogen compared with phosphorus to the hydrologic events indicates different sources for the two nutrients. Some discussion of this is warranted.

Line 154: Are the newer reduced ranges in water fluctuation due to lower peak stages, higher low stages or both? Also see comment on lines 204-207.

Line 204-207: Assuming flood control management limited maximum stages, increased storage would increase the extent of littoral zones. It is a little harder to envision how increasing minimum stage increases the littoral zone extent. Increasing storage by raising either maximum or minimum stage is dependent on increasing storage in the rainy versus dry season. Since two goals are to have discharge for 365 days per year in the restored lower section (Expectation #1) and to keep that section inundated for at least 180 days per year (Expectation #3) it would appear that more storage will be required in the dry season corresponding to higher minimum stages in the upper lakes. A little more explanation of how the new Headwaters Revitalization Schedule will increase the extent of upper lake littoral zones is warranted.

Line 259: What is the range of "Zone B"? Could it be shown on Fig 9-7B?

Line 246-267: While not explicitly stated it appears the new Headwaters Revitalization Schedule has not yet been implemented even though all the construction of the S65 gate (and apparently all up-gradient improvements) has been completed. Why has the schedule not yet been implemented and when does the District anticipate starting that schedule?

Line 341: Low dissolved oxygen occurred after the high rainfall event of July and it is suggested that this has occurred on occasions where high rainfall follows droughts. What is the mechanism for the loss of oxygen following these events?

Lines 353-359 and Fig 9-8: One assumes that the desired water level in PC61 (and all other piezometers) is to be above the ground surface for at least 180 day (Expectation #4), but is there a schedule for that period of inundation?

Line 440: The hydrology section with each expectation listed and the degree to which it has been met discussed is a very nice approach; all parts of this section would have benefited from this layout.

Lines 550-588: It is not clear why DO was not sampled with depth in the reference streams but was during the baseline period within stream segments of pools A and C. Is it because mixing due to flow makes for a more homogeneous DO profile in the reference streams, a mechanism not available in the stagnant pre-restored stream segment of the Kissimmee River? However if this is true, why do expectations for DO include a depth profile or why is a depth profile apparent in the restored segments of Pool C that contain flowing water.

Line 550-588: There is clear improvement of DO concentrations in the post-restored Pool C reaches, but the improvement appears to be more dramatic in the dry season while the most critical times for fish health in during the wet season. Much of this improvement might be due to simply have flowing water in the channel, rather than some improvement due to a period of inundation. As mentioned in the general comments section, the compatibility of expectations may need to be addressed.

Line 607: It has never been clear why there is no explicit expectation with respect to phosphorus. What is the rationale?

Lines 664-710 and Figures 9-17 to 9-20: Since the purpose of these graphs (based on the discussion) is to evaluate the long term trends in N and P concentrations it would be beneficial to have five groups of bars where each group represents a specific location and each bar represents a year. As currently presented the most obvious comparison is the general increase in load (and concentration) as water flows through the lower basin, which really is an intuitive conclusion (at least for load). Considering the number of bars in the proposed format why not use a line graph with year as the X axis?

Line 780: The survey of sediment and soil samples for phosphorus analysis is very important and it should be a priority to complete the analysis and reporting of this work in the coming year.

Lines 854-856 and 893-984: A formatting error has caused double printing.

Lines 852-934: There was a dramatic decrease in wading bird abundance in 2008. Was this a response to a hurricane? A 3-year moving average (Table 9-7) loses some information by minimizing this drop. While a small decrease is apparent in waterfowl numbers it is wading bird numbers that changed most.

Figure 9-23: Why was an unmistakable increasing trend in the first few years after rehabilitation (2002-2006) dramatically lost in all subsequent years? Was this a response to a major event? The data in this figure don't seem to reconcile with the data in any of the tables.

Lines 903-906 versus 923-924: The ranking of relative species abundance is inconsistent between these locations.

Figure and Table comments

The fonts in several figures (mostly legends and axis labels) are too small to be clearly legible when viewing at normal magnification. This is especially true of figures 9-8, 9-11, 9-12, 9-14, and 9-16 through 9-21.

Posted: 10/29/12 @ 7:45 AM EST

Chapter 9 was reviewed on the basis of the technical quality of the work carried out as part of the Kissimmee River Restoration Project and other related basin initiatives, and the interpretation of the results of these studies.

The central questions addressed in this review were:

Are the findings and conclusions supported by "best available information," or are there gaps or flaws in the information presented in the document? Are there other interpretations of the data and other available information that should be considered by the authors and presented to decision makers?

The authors of chapter 9 have provided detailed and reasoned responses to our comments, and have provided additional information concerning subsequent activities and reports. In general, this chapter provides a clear and scientifically-supported overview of activities in the KRRP and a few other initiatives related to the restoration of ecological integrity to the Kissimmee River and its floodplain.

A few issues concerning the comments made about the report remain:

Comment #2:

The comment here was more general than the authors interpreted. The correlation between rainfall after drought (comment #12), and the impacts on DO levels is also evident in the STAs. There is a *potential* conflict between expectations, and this possibility should be driving the district to explore the causes of low DO more fully and recognize that initial inundation of the floodplain might be a factor.

Comment #11:

The text gives the reader the impression that all construction is complete, but never really says either way. Perhaps the authors could add a sentence or two indicting what construction remains to finish the job and not the expected finish time.

Comment #21:

The authors did not address the most important part of this comment, namely that use of a 3-year moving average blurs important variation of the data. It is recommended that annual data, not 3 year moving averages be used.

Key Findings

1. Although the total rainfall was below average in the hydrologic year reported on, an extreme storm event was a key driver for some of the processes that occurred in the system.

2. The expectation for the seasonal flow pattern was not met because of the below-average rainfall in the hydrologic year which led to extended periods of low discharge.

3. Although the floodplain stage met fluctuation targets at both the upper and lower parts of the basin, targets for floodplain inundation were met at the lower sites but at only one of three upper sites.

4. Restoration targets for dissolved oxygen are on track, both in terms of average values and minimum values.

5. Although total phosphorus load continued to decline, the Kissimmee River is still a very major source of TP to the downstream system. There is some evidence that inundation of the floodplain led to trapping of significant amounts of TP.

6. Wading bird and waterfowl abundance showed positive signs of recovery.

Key Recommendations

1. The hydrology of the system is impacted greatly by extreme events, namely drought and major storms. Climate change will almost certainly affect the frequency and magnitude of these events. An analysis of the potential consequences on the Kissimmee Basin is essential for adequate future planning, as changes in hydrology including stage and inundation, oxygen levels and phosphorus transport are certain.

2. The observed floodplain storage of phosphorus should be investigated in depth. The measures being implemented to reduce the phosphorus loading to Lake Okeechobee will not result in meeting the 140 metric ton target by 2015 unless some new approach or some different process that hasn't been considered takes effect. The possibility of significant floodplain storage of TP may be the only way the target can be met and so must be fully explored.

3. The mechanism driving the loss of oxygen after high rainfall events that follow droughts should be investigated as this scenario will be repeated, and may, as a consequence of climate change, occur more frequently.

4. The survey of sediment and soil samples for phosphorus analysis is very important and it should be a priority to complete the analysis and reporting of this work in the coming year.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 10

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

Posted: 9/20/12 @ 7:26 AM EST

General Comments

This WY2012 Chapter 10 is possibly the first comprehensive, cohesive and highly professional chapter on the estuaries the district has presented to the public and this reviewer had an opportunity to see. The chapter is focusing on two important estuaries, the St. Lucie (SLE) and Caloosahatchee (CRE) Estuaries. Annual reporting on the status of the both estuaries is mandated by the Northern Everglades and Estuarine Protection Plan (NEEPP). Incidentally, Lake Okeechobee feeds both estuaries as is the largest contributor of fresh water. The lake outflows have a profound effect on the salinity, circulation, water quality and the health of the biota in the estuaries. The lower sections are impacted heavily by the Gulf of Mexico (CRE) and Atlantic Ocean (SLE) tides. The first part of the chapter focuses on the current monitoring programs and other ongoing efforts to improve water quality and biota, including grasses. Because releases of water from Lake Okeechobee play the major part in the hydrology and ecology of the estuaries, the second part describes the Adaptive Protocol study geared towards developing guidance for the discharges.

In general, the chapter is well written and the flow of writing is logical and relatively easy to follow. The authors should be commended for this. A minor and rectifiable problem is the use of acronyms which are only sparsely identified or not identified at all. The authors must make sure that the acronyms are periodically defined throughout the chapter. The second problem, as it is typical for some but not all other chapters of the 2013 SFER, and the previous Water Year reports, is mixing of metric (SI) and US units and switching between the US and metric units without conversions. As pointed out in the previous reviews, this report after completion will be read and studied not only by the SFWMD staff and Florida agencies but by a wide national and international audience. Most of the US government reports and all scientific and professional papers are today using SI units and metric system is also the system taught at universities. Hence, consistence with the units is important. Specifically, it is proposed that the authors adhere to one set of units (preferably metric) and provide conversion factors in the parentheses or legends of tables and figures. It was noted that metric SI units used in this chapter include meter (m), kilometer (Km), tons (mt), hectares (ha), mg/L, µg/L, m2, oC and US units used in this chapter as acre-ft, feet, acres, inches, etc. Often both systems of units appear in the same table. The unit of salinity in the chapter is not defined and does not have a dimension. The customary units of salinity are per mile (o/oo), grams of salt/kilogram of water (same as total dissolved solids), conductivity or PSU which is a conductivity ratio of sea water to a standard solution of KCl. With exception of conductivity, all of them could be considered as dimensionless but the unit base must be defined because PSU is not numerically the same as per mile or grams/kg. On one or two graphs in the chapter *psu* was identified as salinity unit.

Specific Comments

Page 10-1 to 10-4. Both estuaries in WY 2012 were hydrologically impacted by reduced discharges from Lake Okeechobee. The lake exhibits higher phosphorus and nitrogen concentrations and has been eutrophic. As a result of the reduced release the N and P concentrations in both estuaries dropped, not a result of BMPs but simply by shutting off the lake source of nutrients. This has positively impacted the oyster densities. The fresh water discharges are important because the section of the estuary affected by them is apparently the most productive part of the estuary.

Because there is little information on the effects of low releases and reasons for them and connectivity to the Everglades system which are also fed by the Lake Okeechobee discharges, the district is developing the Adaptive Protocol. The legislation also requires development of the Watershed Protection Plans for the estuary watersheds. Each plan will have three components: (1) Pollutant source control program; (2) The Construction projects, (3) Research and water quality monitoring program. These programs appear to be similar to the other watersheds which drain into Everglades.

For the monitoring and assessment the authors divided the water year into dry (November and April) and wet (May to October) seasons. It could be pointed out that the SFWMD water year (WY) timing is similar to standard hydrological year which starts in October.

Page 10-7 lines 251 and 293/294. Provide conversions between US and SI units (in parentheses) for cfs and m3/s and acres and hectares.

Pages 10-7 and 10-8 list local water quality and restoration projects in SLE watershed. In comparison to the total area of the watershed these projects are minor and some, such as sewer connections and dredging for navigation would have been done with or without the Everglades Program. Provide conversions of feet to meters and acres to hectares in parentheses. These projects have not been completed yet.

Pages 10-8 till 10-10 list four construction projects in the CRE watershed.

One is an aboveground reservoir (provide conversion from ac-ft to m3). A similar (same?) project evaluated in Chapter 4 reported very low efficiencies of such reservoirs. The second project is conveyance attenuation of pollutants in a creek and ditch. The third project is to test strategies for nitrogen and other pollutants removal using wetlands, focusing on removal of dissolved organic nitrogen (apparently ammonium). The estuary is nitrogen limited as it was proven by a TMDL prepared for the watershed. The fourth project is the development of the Caloosahatchee basin storage/treatment alternatives. All four projects are in their initial phases of implementation, some are waiting for the funding to become available.

Pages 10-10 to 10-23 describe the SLE hydrology, water quality and aquatic habitat monitoring program. This program has been apparently initiated recently because the last year report provided only a cursory mentioning of the monitoring program and reported mostly the status of grasses. This report was withdrawn. This chapter on the other hand provides an excellent delineation of the program and presentation of preliminary results. As more data will be gathered this program has a potential to become a gold mine for marine scientist and will enable the SFWMD and state planers to identify quantitatively the stressors and their impact and develop good plans. The TMDL for the SLE watershed has been already prepared and the rule was adopted by the state. The TMDL goal for the SL estuary is 0.081 mg/L of Total P and 0.72 mg/L of Total N, respectively. One may argue that the allowable P load is high and would not guarantee a good status (better than eutrophic) of water quality in the estuary. However, the subsequent monitoring results show that the estuary is nitrogen limited rather than phosphorus limited; hence, both N and P should be reduced but N reduction may provide more immediate

results. The studies also identified salinity limits that will dictate the fresh water releases from Lake Okeechobee. The salinity has apparently a significant effect on oyster densities.

On *page 10-10*- provide conversions for cfs and identify units of salinity.

*Page 10-15*needs conversions from inches to mm (millimeters) or cm. Although previous section already identified dry and wet season (November to April and May to September) this identification should also be included into Table 10.1. The chapter correctly states that annual rainfalls are also affected by long term climatic changes (El Niňo). However, have any projections been made on the effects of the ongoing long term climatic changes on the future rainfalls in the South Florida region?

On *page 10-16* clean up the units in Table 10-2. Since four columns report the results in metric tons (mt) change million acre-ft to 109 m3 (106 acre-ft = 1.233 Km3). Note that 1 Km3 = 109 m3. Provide conversion between the US and SI units. Provide conversion from cfs to m3/sec on Figure 10-5 and throughout the rest of the chapter.

Page 10-17. When referring to salinity provide units (even though salinity may be dimensionless such as PSU). A sentence such as "… a preferred salinity envelope of 8-25 has been …." does not make much sense to those outside of the estuary team of SFWMD. Flow unit cfs on Figure 10-6 is usually not capitalized unless all lettering is in capital letters. Provide a conversion to SI units.

Page 10-18 and 10-19. Provide salinity units in Table 10-3. Table 10-4 and Figure 10-7 indicate that (a) phosphorus concentrations are very high that would normally be in eutrophic or even hyper-eutrophic range, (b) luckily the estuary is nitrogen limited. Figure 10-4 shows that algal blooms might have occurred in the past WYs as documented by spikes of high Chl-a concentrations.

Page 10-20. What is POR? The definition of this acronym was not found. It is a good idea to identify rare acronym more than once in the chapter. The chapter also reported at several places that reducing fresh water inputs into the estuary improved oyster densities. This improvement is not seen on Figure 10-8 which shows decreasing densities in the last three years.

Page 10-22. Figure 10-8 also contradicts the "Significant Findings" on page 10-22, reporting increases of oyster densities. The fact that the estuary is overloaded with P but is limited by nitrogen could be added to the findings.

Pages 10-24 to 10-37 describe the same programs for the CRE.

Page 10-26 lines 729-730 has acres (US) and m2 (SI) next to each other. It is suggested to use hectares instead of acre with conversion to acres in parentheses.

Page 10-28. Provide conversion of inches to cm or mm. Again it would add clarity if the months of wet and dry season are included in table 10-5.

Page 10-29. Same issue with conversions from million acre – ft to more convenient Km3 or 109 m3 and cfs to m3/sec, add units for salinity.

Pages 10-30 to 10.32. Lake Okeechobee was found to be a major source of nutrients also in the CRE watershed. In Table 10-8 the WY 2012 has the smallest Lake Okeechobee water release and, accordingly, the N and P loads were small (there is an issue with units). Figures 10-16 and 10-17 and Table 10-9 show chlorophyll-a concentrations between oligotrophic and mesotrophic water quality. The Chl-a spikes into mesotrophic/low eutrophic water quality are related to increasing phosphorus levels which could lead to concluding that the estuary is P limited. The N/P ratio is also higher than that for SLE . The releases of the eutrophic Lake Okeechobee water may be the cause of elevated Chl-a concentrations. Meaning of the acronym POR on line 845 should be identified.

Pages 10-38 to 10-54 describe the Adaptive Protocol Study. The goal of the study is to develop understanding of the Lake Okeechobee releases on the quality of the estuaries. The study begins with the formulation of the hypothesis expressed on Figure 10-39. Then it describes in-situ monitoring and cruises and present preliminary results. The model presented on Figure 10-20 specifies that the most productive sections, the most intensive primary productivity, occur in the fresh water/brackish sections of the estuary. The primary productivity is phytoplankton algae and plant growth which are grazed by zooplankton and progressing to higher trophic levels, i.e., fish and fish eating fowl and mammals. Lines 948 to 950 emphasize that increased rates of primary production may not be manifested as increased biomass (increased Chl-a concentration) because of predation by zooplankton and filter feeders. Harmful Algal Blooms (HAB) occur when the balance between primary production and predation is disrupted.

On *page 10-38* define LSZ.

Pages 10-40 to 10-44 describe methods. The scientific reasoning for pulse investigation described on *page 10-40* is not clear. What was obtained by these investigations? Also report pulses in m3/sec. On *page 10-42* provide a conversion from knots to km/hr (note that all distances in this chapter are reported in kilometers) and miles/hr (in parentheses). On *page 10-45* add units of salinity. On the same page, the reported increased salinity was caused by small discharges from Lake Okeechobee.

Table 10-11 page 10-46. This table needs salinity units. The DO maxima in the fifth column must be in error, 3.8 mgl/L is probably not a maximum but a minimum. The other values ranging from 15 to 19.7 mg/L would represent a gross oversaturation which is erroneous based on the CHL-a concentrations reported and mean and standard deviations in the table.

Figure 10-47 presents a sharp increase in turbidity in the downstream sections of the estuary even when the concentration of Chl-a was decreasing. Page 10-48 attributes the turbidity increase to waves stirring sand by boat traffic and wind. In this figure salinity is expressed in psu (for the first time in this chapter?). It should be reported so through the chapter. The legend on Y-axis should report chlorophyll as Chl-a, not chla.

Table 10-12 and Figure 10-26 on pages 10-50 and 10-51 indicate that, unlike SLE which is clearly nitrogen limited, the CRE might be phosphorus limited. Has it been clearly established? The blue Chl-a spikes to mesotrophic/eutrophic range at three upstream sections (stn 1,3,5) on Figure 10-51 correspond to blue P spikes, hence, CRE may be phosphorus limited. The authors on page 10-54 also attributed the elevated chl-a concentrations to hydrologic conditions and stratification. Apparently, there is a great potential for algal blooms when there is not inflow from Lake Okeechobee and temperature is greater or equal 27°C.

Summary Comments

In general, the chapter presents a credible and comprehensive plan of monitoring that would lead to an efficient management of two estuaries and development of Adaptive Protocols for releases of flow from Lake Okeechobee which is their main source of water and nutrients. The lake is currently overloaded by phosphorus. A complete stoppage of Lake Okeechobee inputs is apparently not possible. The fresh water input is needed to stimulate primary productivity but the stimulus must be balanced and not results in phytoplankton blooms and disappearance of species, grasses and habitat.

It was pointed out by the reviewer in his Chapter 4 review document that the high phosphorus loads to the lake may be, in addition to the “traditional” agricultural and urban sources, caused by the effects of the Bone Valley phosphate mine which appears to be in a dangerously close vicinity of the Kissimmee River watershed, which is the main flow and nutrient source to the lake and might also be one of the sources of the atmospheric P deposition to the lake, estuaries and

Everglades. This is an observation; nevertheless, it implies that the reduction of phosphorus inputs may be a long term goal rather than a short term possibility.

Regarding the specifics of Chapter 10, the results of monitoring agreed with the conceptual model depicted on Figure 10-20 and the monitoring effort and protocol development are on the right track and have already brought good quality data. It has been already established that SLE is nitrogen limited, hence, less sensitive to phosphorus discharges from the lake and the watershed. CRE may be more sensitive to phosphorus loads and there is greater potential for harmful algal bloom development in the upper reaches of the estuary. Extreme eutrophic concentration of Chl-a exceeding 60 μg/L were observed in the upper reaches of the estuary. These findings and hypotheses may lead to somewhat different protocol for controlled water discharges from the lake.

The authors of the chapter suggest that their future monitoring should expand and include fish larvae as a biological end point. This is commendable but from a managerial stand point, the monitoring must produce also data for development, calibration and verification of computer models of the estuaries that, in addition to further TMDL N and P load refinement, would lead to a (real time?) management model for operators regulating the discharges from the lake and proportioning them between the storage in the lake, releases into the two estuaries (discharging to Atlantic Ocean and Gulf of Mexico) and the Everglades which need the lake waters too.

It is hoped (expected?) that in the next 2014 report the district scientist will also report on other important coastal water bodies such as Biscayne Bay.

Posted: 10/24/12 @ 1:13 PM EST

General Comments

WY2012 Chapter 10 focuses on two important estuaries, the St. Lucie (SLE) and Caloosahatchee (CRE) Estuaries. Annual reporting on the status of both estuaries is mandated by the Northern Everglades and Estuarine Protection Plan (NEEPP). Incidentally, Lake Okeechobee feeds both estuaries as the largest contributor of fresh water. The lake outflows have a profound effect on the salinity, circulation, water quality and the health of the biota in the estuaries. The lower sections are impacted heavily by the Gulf of Mexico (CRE) and Atlantic Ocean (SLE) tides. The first part of the chapter focuses on the current monitoring programs and other ongoing efforts to improve water quality and biota, including grasses. Because releases of water from Lake Okeechobee play a major part in the hydrology and ecology of the estuaries, the second part describes the Adaptive Protocol study geared towards developing guidance for the discharges.

In general, the chapter is well written and the flow of writing is logical and relatively easy to follow. The authors should be commended for this. The authors submitted an extensive response document and this reviewer is pleased that in many substantive responses there is an agreement between the reviewer and the authors. This includes General Comment 2 and Comments 1, 5, 10, 17, 21, and Summary Comments 1 to 5.

Several reviewer's comments dealt with the inconsistency of units. These included Comments 2, 3, 6 to 10, 13 to 16, and 19. These comments were constructive and the authors in all cases included in the reports unit conversions which improved the clarity of the chapter.

Specific comments and responses:

Comments 3 and 4: The authors satisfactorily explained the difference between the aboveground storage basins in the estuarine watershed described in Chapter 10 and those described in Chapter 4. The former much larger basins are designed for providing storage while the latter are for quality control.

Comments 9, 11 and 21: The authors clarified a small ambiguity with the nutrient limitations of the estuaries. It appears that SLE is clearly nitrogen limited but the limiting nutrient for CRE has not been sufficiently qualified or established for CRE where a combination of hydrologic conditions and temperature may also account for elevated Cal-a concentrations.

Comment 10 and 11: The authors clarified the definition of POR (Period of Record). In general, the reviewer recommended that acronyms are periodically redefined in the chapter. The authors also clarified the reviewer's note of ambiguity regarding reporting of oyster densities.

Comment 18: While the authors in their response to this comment satisfactorily addressed the issue of units they did not respond to the reviewer's concern about the lack of clarity of description and reasoning of the purpose of the pulse tests.

Comment 20: The authors satisfactorily explained the difference between the sharp increases of turbidity during low Chl-a concentrations and edited for consistency Figure 10-24.

Key Findings and Recommendation (Phase III)

In general, the chapter presents a credible and comprehensive plan of monitoring that would lead to an efficient management of two estuaries and development of Adaptive Protocols for releases of flow from Lake Okeechobee which is the main source of water and nutrients. The fresh water inputs are needed to stimulate primary productivity but the stimulus must be balanced and not result in phytoplankton blooms and disappearance of species, grasses and habitat.

In the response to the Summary Comment #2 the authors identified two key elements of the District's core mission with respect to the two estuaries which they will include in the SFER. These are: (a) operation of District infrastructure and (b) quantifying environmental water supply to support natural systems, They will include description of the progress on these fronts in the SFER which may include:

o Implementation of the Lake Okeechobee Regulation Schedule and Operation, and

o Water Protection Rule Development

The chapter described a conceptual model of the estuaries and nutrient balances therein and conducted monitoring cruises to support the model. The monitoring must produce also data for development, calibration and verification of computer models of the estuaries that, in addition to further TMDL N and P load refinements, would lead to a (real time?) management model for operators regulating the discharges from Lake Okeechobee and proportioning them between the storage in the lake, releases into the two estuaries (discharging to Atlantic Ocean and Gulf of Mexico) and the Everglades.

The author's work found that while SLE is clearly a nitrogen limited estuary, both N and P may be limiting for CRE. This may lead to a requirement for equal weight controls of both nutrients and, consequently, to a modification of the abatement programs which may be different from that being implemented for the Everglades National Park. For example, riparian wetlands provide controls of both N and P. The work on identifying the limiting nutrient for CRE must continue.

The reviewer supports the authors' suggestion to expand their future monitoring and include fish larvae as a biological end point.

This chapter was a first progress document in the SFER known to the reviewer which in a cohesive and scientifically defendable manner described the environmental issues pertaining to the two estuaries managed by the District. This work provided valuable and needed information. The authors also presented a good and defensible plan for future activities.