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

During September–November 2011, the peer-review panel and public posted their comments on draft Volume I on the 2012 SFER WebBoard at www.sfwmd.gov/webboards (see Appendix 1-2). This appendix includes authors' responses to comments and recommendations provided on the WebBoard. With the exception of reformatting some information for better readability, this appendix was not edited by the SFER production staff and appears verbatim as posted on the WebBoard.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 1

Stacey Ollis and Wanda Caffie-Simpson

Level of Panel Review: Background

Reviewer: J. Burkholder

Posted: 09/20/11 at 01:37 PM by S. Ollis

Comment: *The draft Chapter 1 consists of less than 1.5 pages of writing, a map, and a summary table of "Volume I key content and associated statutory reporting requirements." The excellent map (Figure 1) shows the major geographic features in the District boundaries; however, the labels are difficult to read and should be enlarged. While this chapter clearly sets the stage for major emphasis on statutory reporting requirements, as a startling, major shortcoming, no information whatsoever is included about what was actually accomplished by the District in WY2011. It would seem much more desirable – that is, much more helpful to readers – for the authors to include, at a minimum, a table summarizing these highlights. Such an excellent table was included in previous versions of Chapter 1, and made it a strong introductory chapter that was appropriately informative to readers. Lacking such information, my overall assessment is that the draft Chapter 1 unfortunately falls short of fulfilling its purpose to provide sufficient background for the 2012 SFER.*

Response: Chapter authors greatly appreciate the insightful feedback of the panel on the draft 2012 SFER – Volume I. With a strong focus on electronic reporting for efficiency, all tables and figures of the SFER can be enlarged on screen for improved readability; with an emphasis on keeping our reporting as tight as possible, editors will review graphics to determine if layout can be accommodated in larger view for the final report, as appropriate. Also, note that a high-level summary of the key 2012 SFER findings and accomplishments will be included in the Executive Summary of the final report, which will be prepared once all main report information is completed; this will be cross-referenced in the final chapter for clarification.

Appendix 1-4: CERP Annual Report (470 Report), Specific Comments

Comment: *P.1-4-9, last paragraph – Describes several non-CERP "Critical Restoration" projects that were underway when the plan was written and are necessary precursors for many CERP projects. It would be helpful to provide brief additional clarification about how these projects fit/are being accommodated into overall CERP planning.*

Response: The impacts of the Critical Restoration projects on surrounding CERP projects are evaluated as part of the CERP project development process and subsequently documented in the final Project Implementation Report and Environmental Impact and Statement of related CERP projects, respectively. A sentence will be added for clarification in the final report.

Appendix 1-6: South Florida Water Depth Assessment Tool

[Note: responses to panel comments to be provided on SFER WebBoard under Appendix 1-6.]

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 2

Wossenu Abtew

Level of Panel Review: Background

Reviewer: J. Burkholder

Posted: 10/06/11 at 11:41 AM by W. Abtew

Comments are from the Panel Review; Responses are from the authors of Chapter 2.We appreciate the time and expert advice provided on the hydrology chapter. Please consider the following Responses to Comments

Comment #1: *Metric units should be used throughout; if the authors feel that it is more advantageous to use English units (e.g. acre-feet), then Metric units should be given in parentheses.*

Response #1: After years of deliberation, we have settled on units of flow in English standard units. While small volumes are easily done in both units, large flows are not convenient in metric; one acre foot equals over 1,200 cubic meters making expressions of millions very large. If Metric unit is added in parenthesis, tables will be too large to fit pages. Additionally, regional users of the report are familiar with the English unit as acre-ft for flow measurements. In light of this history and circumstance, we elect to make no change to units in the report. However to aid readers, conversions from English to Metric units will be part of the final SFER Front Matter similar to previous years.

Comment #2: *Line 38 – by "normal," do the authors mean "long-term average"? Please clarify.*

Response #2: Yes, normal refers to "long-term average". No change would appear to be needed.

Comment #3: *Figure 2-1 – is an informative map, but the legend should explain the arrows.*

Response #3: The comment is helpful and accepted. A legend is added to Figure 2-1 stating that the arrows show WY2011 flows and arrow width is proportional to flow volume. The revised figure will be included in the final report.

Comment #4: *As the draft chapter states (line 56), seven succinct Appendices provide supplementary information to the chapter as clear graphics and tables. However, much of the text on pp. 2-21 (Water Levels and Flows) through 2-35 is repetitious, with the same appendices referenced numerous times throughout. To both streamline this chapter further, and make the references to the appendices less repetitive, the Panel suggests that a table should be added that briefly summarizes the contents of the appendices, and also summarizes the chapter references to the 2007 SFER (see last section of these comments, below). The table is needed to provide a succinct list of the contents of each Appendix. The importance of the information referred to in the 2007 SFER would also become rapidly evident, rather than being "buried" in the text. The chapter would be shorter and clearer (not so much repetitious-sounding information to wade through for each region, especially regarding Appendices 2-3 through 2-7 – see gray-shaded information in the last section of these comments) because much of the writing on pp. 2-21 through 2-35 could be removed.*

Response #4: The organization (format) of the chapter has evolved through a decade of peer review comments. The current format has been used for the last few years and has been accepted by most readers and serves their informational needs. Pages 2-21 to 2-37 cover "WATER LEVELS AND FLOWS". In this section, in addition to the text, there are: 1. Two tables (Tables 2-1 and 2-8); 2. Five figures (Figures 2-10, 2-11, 2-12, 2-13 and 2-14).

The section is not unduly lengthy. For each water body or region, limited number of lines of text are provided with appropriate references to the appendices. For this year's report, we feel that no format change is needed. A reader who needs information on a water body, for example "Lake Okeechobee", does not have to read the whole chapter. Each water body has a direct reference to the relevant Appendices.

Comment #5: *Figure 2-2 – is the upper Kissimmee basin adequately covered with weather sites? Only one weather site is shown. In case of malfunction, wouldn't another site be essential to ensure the information based needed to support the District efforts? It would be helpful for the figure legend to include explanation about the distribution of weather stations (many, closely spaced, in some areas versus very few in others).*

Response #5: Yes, the weather network is not evenly distributed. A figure legend may not be sufficient to describe the characteristics of the weather monitoring network. The District hydrometeorologic monitoring system is presented in detail in SFER2007, Chapter 2, Appendix 2-4 "The South Florida Hydrologic Monitoring System". The distribution of weather stations and other details on weather parameters monitoring are covered in this report. We believe that no change required in this year's report

Comment #6: *Lines 200-202 - The canal system in the Lower East Coast was designed to supply water to the ENP during a 10-year drought. Considering the fact that droughts appear to be more frequent than they were historically, is this adequate or does it need to be adjusted?*

Response #6: Frequency analysis that includes recent data may show some difference. However, frequency analysis of a region's hydrologic data is done once in several years and may show some tendency for changes in drought frequency and severity. The canal system is more limited by upstream water availability than by capacity. will still have capacity to supply water as long as water is available upstream.

Comment #7: *Line 484- should be Alligator Lake*

Response #7: Thanks for catching this and the changes will be made.

Comment #8: *Lines 485-486 – can the reason why stage data were only available for Alligator Lake for a few months of WY2011 be determined with more certainty?*

Response #8: Good comment and the statement will be changed to show that the reason stage data were limited was construction and repair work.

Comment #9: *Pp.2-22 to 2-36 – the main chapter needs to include one or more figures that show the locations and relationships of all of the named structures.*

Response #9: The final text will include references to previous reports where the maps are available.

Comment #10: *Appendix 2-1, Figure 1 provides basic foundation information that should be included in the main chapter rather than in an Appendix.*

Response #10: The Figure is mainly to show the groundwater wells that were used in Appendix 2-1. Figure 2-2 in the report has comparable information.

Posted: 09/16/11 at 10:15 AM by W. Abtew

From: Abtew, Wossenu
Sent: Friday, September 16, 2011 10:07 AM
To: Tears, Clarence
Cc: 'Gilpin-Hudson, David'; LaRock, Julianne; VanHorn, Stuart; Redfield, Garth; Ollis, Stacey; Pathak, Chandra; Ciuca, Violeta; Nath, Ananta
Subject: RE: 2012 South Florida Environmental Report

We will add a statement to that effect during revision of the report.

Wossenu Abtew, Ph.D, P.E., D.WRE
Principal Engineer
Water Quality Bureau
South Florida Water Management District
Phone: 561-682-6326
wabtew@sfwmd.gov

From: Tears, Clarence
Sent: Friday, September 16, 2011 9:12 AM
To: Abtew, Wossenu
Cc: 'Gilpin-Hudson, David'; LaRock, Julianne; VanHorn, Stuart; Redfield, Garth; Ollis, Stacey; Pathak, Chandra; Ciuca, Violeta; Nath, Ananta
Subject: RE: 2012 South Florida Environmental Report

Then I suggest your statement be added to this report to clarify the limitations.

Thanks Clarence

From: Abtew, Wossenu
Sent: Friday, September 16, 2011 9:01 AM
To: Nath, Ananta
Cc: Tears, Clarence; 'Gilpin-Hudson, David'; LaRock, Julianne; VanHorn, Stuart; Redfield, Garth; Ollis, Stacey; Pathak, Chandra; Ciuca, Violeta
Subject: RE: 2012 South Florida Environmental Report

Ananta,

The objective of SFER, Chapter 2, Regional hydrology, is to summarize regional level hydrology within the limited report production time. We will evaluate for next water year's report the possibilities of expanding the report to cover more details at sub-region level.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 3A

Grover G. Payne[1] and Shi Kui Xue

Level of Panel Review: Accountability
Reviewers: V. Novotny (AA), O. Stein (A)

Posted: 10/06/11 at 04:34 PM by G. Payne (FDEP)

Comment #1: *Since the data reported are specifically for the wildlife refuge, the three WCAs and the park, a map clearly delineating these regions relative to each other would be useful before presenting the site locations shown in Figs 3A 1-4. Specific confusion arises to the difference between WCA 1 and the Loxahatchee Wildlife Refuge. Are these the same thing? Some subsequent maps identify an area of similar dimensions to that shown in Fig 3A-1 as WCA 1. There should be consistency between map labels.*

Response #1: A map will be added to show all portions of the EPA including the three Water Conservation areas and the Park. For the purposes of this report the Arthur R. Marshall Loxahatchee National Wildlife Refuge (Refuge) is the same as Water Conservation Area 1 (WCA-1).

Comment #2: *TP data presented in the text on Pgs. 3A24 to 3A30 are not always consistent with data reported in Figures 3A9 and 3A10 and/or Table 3A4 (or between table and figures). Some specific inconsistencies are noted below, but a double check of all information is warranted.*

Response #2: The typographical error noted in *Comment 14* was corrected. No other inconsistencies were found.

Comment #3: *It might be useful to present and briefly analyze the ratio of OP to TP both spatially and temporally, since the biogeochemical cycling of P undoubtedly influences this ratio. Important clues to long term fate and meeting regulatory criteria might be garnered from this analysis.*

Response #3: Because the EPA is a highly phosphorus limited system, any available orthophosphate is quickly assimilated. As a result a large percentage of the orthophosphate measurements are below the analytical detection limit. For example during WY2011, approximately 87 percent of the OP measurements at interior sites within the EPA were below the detection limit while across all types of sites (inflow, interior, and outflow) 70 percent of the OP measurements were below the detection limit. With such a high percentage of OP measurements below the detection limit, an analysis of the OP/TP ratio is not likely to reveal a significant amount of additional information not apparent from the analyses currently presented in the chapter.

Comment #4: *Page 1 - Last paragraph contains a dichotomy of two statements "With a few exceptions, water quality was in compliance with existing state water quality criteria during WY2011 " and "Comparisons of WY2011 water quality data with applicable Class III water quality criteria revealed excursions for four parameters: dissolved oxygen (DO), alkalinity, pH,*

[1] Florida Department of Environmental Protection, Division of Environmental Assessment and Restoration, Environmental Assessment and Support Program, Tallahassee, FL

and specific conductance". DO is a key water quality parameter and the violation of the DO standard (criterion) usually implies that the water quality goals are not met and the designated use is not attained. Alkalinity and pH excursions could be of natural origin which could be disposed of by a Use Attainability Analysis (UAA).

Response #4: As stated in the comment, during WY2011 exceedances of state criteria were observed for only four parameters. Of the four parameters, two, alkalinity and pH, are typically limited to the interior of the refuge and reflect natural conditions that could be addressed through site specific criteria. Additionally, the specific conductance exceedances result from the operation of the canal system in South Florida which is an unabatable condition.

As described in the chapter, the DO exceedances generally occur as a response to the biological changes associated with phosphorus enrichment or result from abnormal climatic or hydrologic conditions. Additionally, unlike most other parameters, DO is not a direct pollutant. Instead, DO is a secondary response parameter that reflects changes in other pollutants or to physical or hydrologic changes in the system. In accordance with state water quality standards, the violation of any water quality criteria, including DO, can be indicative of not meeting the waterbodies designated use. In water quality assessments, exceedances of DO criteria are not given any more or less weight than exceedances of any other parameters.

Comment #5: *Page 3 lines 56- 57 statement "Only atrazine exceeded the toxicity guideline concentrations and no parameters exceeded state water quality standards" is confusing. Has Florida accepted the federal priority pollutants criteria as state standards? Which ones are violated? (Also p. 13 lines 332-336)*

Response #5: Since it was recognized that many of the pesticides for which data is available do not have numeric criteria that have been adopted by the state, a set of toxicity-based guidelines concentrations for these pesticides was developed using available acute and chronic toxicity data. The guideline concentrations as well as details concerning their development were presented in the 2001 Everglades Consolidated Report (Weaver et al., 2001) as referenced in the text. These guideline concentrations were developed based on the narrative chronic toxicity criteria which is defined by Subsection 62-302.200(4), F.A.C. as: "*the presence of one or more substances or characteristics or components of substances which: (a) are greater than one-twentieth (1/20) of the amount lethal to 50% of the test organisms in 96 hrs (96 hr LC50) where the 96 hr LC50 is the lowest value which has been determined for a species significant to the indigenous aquatic community; or (b) may reasonably be expected, based upon evaluation by generally accepted scientific methods, to produce effects equal to those of the substance specified in (a) above*." Furthermore, surface waters of the State are to be free from components which "*are present in concentrations which are carcinogenic, mutagenic, or teratogenic to human beings or to significant, locally occurring, wildlife or aquatic species*" (Subparagraph 62-302.500(1)(a)5). The pesticide guideline concentrations have not been formally adopted into rule and therefore do not have the same legal meaning as formally adopted criteria and standards. The guidelines were meant to provide a basis for evaluating the parameters detected at concentrations above their respective MDLs.

Not all of the federal priority pollutant criteria have been adopted by Florida. The State's current water quality criteria are provided in Chapter 62-302, Florida Administrative Code which can be found at: http://www.dep.state.fl.us/legal/Rules/mainrulelist.htm.

Comment #6: *Page 3 Total phosphorus data indicate that the Total P standard of 10 µg/L is generally met in the EPA Park but not in the Water Conservation Areas (WCA). With the 2015 deadline for meeting the TP standard looming what is the chance that the standard will be met by this deadline?*

Response #6: As detailed in the chapter, the TP criterion is being achieved in the unimpacted portions of all Water Conservation Areas. While the TP criteria are applicable to the Park, the assessment is performed based on the incoming TP loads known to maintain a healthy condition within the Park with the results of that analysis being provided elsewhere. The criterion is not being achieved within the portions of the WCAs historically impacted by phosphorus enrichment; however, there has been continued and significant improvement observed in the TP levels in these in these areas over the past several years. Additionally, there are continued efforts described in other chapters of the SFER (e.g., STA expansion and improvements) that are expected to result in further water quality improvements throughout the EPA; however, it is extremely difficult to accurately estimate the future effects of these efforts. Additionally, it is outside of the scope of this chapter to speculate on future occurrences, including when the TP criteria will be achieved.

Comment #7: *Page 10 - Water quality excursion analyses lines 263 – 272*

The 10% allowed excursion is generally used in the state water quality reporting to the US EPA and Congress under Section 305 (b) of the clean water Act. 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 excedences 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 is correct.

The point appears to be that a 10 percent excursion frequency is too loose and unacceptable for pesticides. Better to state that directly or if that is an incorrect assumption, a reason for why the10% excursion frequency does not apply should be given.

Response #7: As described in the chapter, the 10% excursion frequency is not applied to pesticides based on guidance from the USEPA. The reason that 10% excursion frequency is not applied to pesticides (and other selected parameters) is that exceedances of the pesticide criteria can result in more immediate and severe effects to aquatic organisms and human health. This will be clarified in the text.

Comment #8: *Page 13-14 Table 3a-1 shows much better water quality in the Park and WCA areas than that during the preceding 5year periods. Is it an expression of a consistent progress? Most likely, this reflects the drought hydrological conditions that resulted in smaller TP loads into these areas.*

Response #8: There are continued efforts, described in other chapters of the SFER (e.g., STA expansion and improvements), that have resulted in significant improvements in water quality throughout the EPA, especially phosphorus, over the past several years. Additionally, observed water quality conditions have fluctuated with changes in climatic conditions.

Therefore, the water quality improvements observed for WY2011 likely result from the combined effect of both the continued efforts to improve water quality in the EPA and the drier conditions experienced during WY2011. As described in the chapter, this conclusion is supported finding that TP loads to the EPA during WY2011 were 65 percent lower than those measured during WY2010 while there was only a 46 percent reduction in flow to the EPA during the same period.

Comment #9: *Page 16 and throughout the chapter – Be consistent with units. The unit of ppb (parts per billion) is not used in the scientific literature and should be replaced (as it was done in the preceding sections of this chapter) by µg/L. Also mg/kg is a generally accepted unit and does not have to be spelled out.*

Response #9: Changes to the units will be made in the document as appropriate and consistent with the style adopted by this report.

Comment #10: *Page 16 line 365-367- The statement that "DO is assessed as an annual station average rather than as point measures" is confusing. In the preceding section and Table 3A-1, DO was assessed by the site specific DO standard for Everglades which is a formula calculating "natural" DO for a given day and hour and the number of excursions is then tabulated. Is a trend in DO assessed by annual geometric means? A better explanation is needed.*

Response #10: In accordance with the DO SSAC, for each instantaneous DO measurement, a limit is calculated based on the water temperature and time of day for the measurement. To assess achievement of the SSAC all of the DO measurements and all of the calculated DO limits for the year are averaged. If the average of the measured DO concentrations exceeds the average of the calculated DO limits, the DO SSAC is achieved. The text will be clarified as appropriate.

Comment #11: *In the last year report review, this reviewer was concerned with relatively large percentage of excursions of the relatively low site specific DO standard in the interior of the refuge area WCA zones. For 2011 year, the situation seems to have improved. The question is whether this improvement is only temporary or a more permanent result of improvements. The entire question of the DO concentrations and compliance with the standard will be revisited below in the discussion of the Appendix.*

Response #11: It is likely that the improvements observed during WY2011 are the combined result of the abnormally dry conditions and efforts to restore the system and to improve water quality conditions. Results of future monitoring will reveal to what extent the observed improvements are permanent.

Comment #12: *It was noted that WY2011 was apparently a drought year and the low P loads can be attributed to the low inflow volume and not necessarily to the improvement or increased efficiency of the abatement programs. For flow, provide conversions from 1000 acre-ft to million m3 (or Mega m3)(1 acre-ft =1233.5 m3 or Mega-m3). The acre-ft volume unit is archaic and if the writers wish to be consistent with the units, m3 should be used as the primary unit.*

Response #12: There are continued efforts, described in other chapters of the SFER (e.g., STA expansion and improvements), that have resulted in significant improvements in water quality throughout the EPA, especially phosphorus, over the past several years. Additionally, observed water quality conditions have fluctuated with changes in climatic conditions. Therefore, the water quality improvements observed for WY2011 likely result from the combined effect of both the continued efforts to improve water quality in the EPA and the drier conditions experienced during WY2011. As described in the chapter, this conclusion is supported finding that TP loads to the EPA during WY2011 were 65 percent lower than those measured during WY2010 while there was only a 46 percent reduction in flow to the EPA during the same period.

The authors and editors of the SFER (*South Florida Environmental Report*) have developed guidelines regarding the appropriate units for use in the report. Since acre-ft is a unit of measure accepted and widely used by the South Florida Water Management District and its constituents, it remains an acceptable unit for use in the SFER. Also, the acre-ft units have been used in the SFER since its inception and its continued use provides a measure of consistency that allows easier comparison between reports. A footnote will be added that provides a conversion to the appropriate SI units. Additionally, a table with the units utilized in the report along with metric – SI conversions is included as a preface to the final SFER.

Comment #13: *224: What do the fatal qualifiers represent?*

Response #13: Fatal qualifiers are standard data qualifiers used by laboratories and/or field samplers to indicate that the quality and/or accuracy of the data may not be suitable for the intended purpose. For example, a qualifier indicating that a sample was not properly preserved or that the analytical analysis was flawed in some manner.

Comment #14: *330-331: While it may be true that sulfate concentration lowers on a north-south gradient and in the interior stations, it should be noted that this is not necessarily a positive. It suggests that the available sulfate is biogeochemically active, perhaps being reduced to sulfide, and may be an indication of associated mercury methylation. This information should be better integrated with Chapter 3B. Note that a conflicting possibility is provided on lines 475-479, but it does appear that the data cannot separate these alternative possibilities.*

Response #14: The general north to south gradient observed for sulfate is the same spatial pattern seen for phosphorus and is the result of the fact that the source water (containing high levels of both sulfate and phosphorus) enters the northern portion of the system with the levels of these parameters being diluted and assimilated as the water moves to the south through the system. The reduction of sulfate to sulfide cannot account for the magnitude of the spatial gradient observed, especially since the highest sulfate concentrations occur in areas where the lowest oxygen levels (greatest potential for reduction to sulfide) with lower sulfate concentrations being found in areas with higher oxygen levels. Additionally, the explanation of the conditions within the Refuge provided on lines 475-479 results from a well understood and substantiated phenomenon which also explains why the interior of the Refuge continues to largely be a soft-water rainfall driven system despite the high conductivity hard-water inflows.

Comment #15: *604 vs 610: There are inconsistencies between reported values, probably line 610 should be 42 (not 62) µg/L.*

Response #15: Typographical error will be revised in text.

Comment #16: *669, 778 and others: Is there any regulatory (or other) significance to the 15 µg/L criterion?*

Response #16: Compliance with the TP criterion is assessed using a four-part test as described in previous versions of this chapter and referenced in this chapter. The four components of the test are: 1) a five-year network average geometric mean TP concentration of 10 µg/L, 2) a one-year network geometric mean TP concentration of 11 µg/L, 3) annual network geometric mean TP concentration of 10 µg/L or less for three out of each five years, and 4) a one-year single station geometric mean TP concentration of 15 µg/L.

Comment #17: *771 to 789: What defines the difference between impacted and unimpacted areas? Is it simply whether a site meets the criteria or not, or have areas been separated into these categories a priori?*

Response #17: The impacted and unimpacted monitoring networks were established *a priori* prior to the initiation of the monitoring program based on the level of historic level of TP enrichment defined primarily based on sediment TP levels. As the system continues to recover from historic phosphorus loading, impacted stations which consistently meet the criteria are incorporated into the unimpacted network, but unimpacted sites cannot become impacted sites in the future.

Comment #18: *Fig 3A6. It is noteworthy that interior levels, especially for the refuge, are lower than the "outflow" levels. This might be due to the same reason as suggested on lines 475-479 for sulfate, but it is not addressed.*

Response #18: The reviewer is correct. The explanation of the conditions within the Refuge provided on lines 475-479 results from a well understood and substantiated phenomenon which explains why the interior of the Refuge continues to largely be a soft-water rainfall driven system despite the high conductivity hard-water inflows.

Comment #19: *Fig 3A7 and 3A11: The triangle symbols are nearly impossible to read in the color version and impossible if printed in B&W. Please enlarge and make a them darker color.*

Region (WCAs park, refuge) identification should also be provided in these figures (see the comment in the general comment section).

Response #19: Specified Figures will be revised as appropriate.

Comment #20: *187-192: This long sentence is very difficult to decipher.*

Response #20: Sentence will be revised as needed to make it easier to read.

Comment #21: *332: This should be unionized ammonia criteria (the word ammonia is missing)*

Response #21: Text will be revised as needed.

Comment #22: *Of note is Appendix 3 which reports the compliance with the DO sites specific standard. The standard is a formula that calculates "natural" DO concentration at the site based on time of the day and year. It was noted that the Annual SSAC Limit allows DO concentration as low as 2 mg/L to pass the standard. These concentrations are clearly lethal to fish and greatly deviate from the federal DO criteria. The problem is magnified by the fact that the standard is compared to an undefined average of the DOI measurements. For example, on the first line of the App. A-3, the calculated SSAC limit was 2.06 mg of DO/L which let pass a minimum concentration of 1.35 mg of DO/L, both clearly in the lethal range.*

Comparing standards to average concentration is dubious at first place because average means that roughly 50% of data is worse.

Response #22: While the scope of this chapter is to use the applicable criteria to evaluate the water quality status within the EPA, not to debate whether the applicable criteria provide adequate protection to aquatic organisms, a couple of comments made by the reviewer need to be addressed.

As described in response to the peer-reviewer's comments on the 2011 Chapter, the DO SSAC was developed with public input and was approved by both the State and the U.S.EPA as fully protective of the designated use of the waterbody. Additionally, the U.S.EPA in their approval of the DO SSAC stated that:

"*The Everglades DO SSAC establishes a revised water quality criteria for the Everglades Protection Area (Water Conservation Areas 1, 2A, 2B, 3A, 3B, the Arthur R. Marshall National Wildlife Refuge, and the Everglades National Park), which remains classified and protected for all designated uses of Class III waters, including recreation and propagation and maintenance of a healthy, well-balanced population of fish and wildlife. Based on our review of the supporting information as provided by FDEP for the Everglades DO SSAC, it is the Environmental Protection Agency's conclusion that the requirements of the Clean Water Act and provisions of 40 CFR Part 131 have been met."*

Further, the reviewer provides no information to support the assertions that the DO levels allowed by the DO SSAC are unprotective and clearly within the lethal range except for a comparison to an inaccurate and outdated Federal DO criteria that was not developed for application to wetlands such as the Everglades. The comparison to the federal criteria provides no relevant information concerning the levels of protection afforded by the Everglades DO SSAC.

Comment #23: *There are no comments on the other Appendices except that the dates of the sampling periods should be included in the table.*

Response #23: The appendices represent summaries of monthly or biweekly data collected over one or five-year periods. The sampling periods applicable for each appendix are provided in the heading for the appendix.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 3B

Ben Gu and Chapter Coauthors

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

Posted: 09/15/11 at 06:36 PM by O. Stein

Overall responses

The authors of Chapter 3B appreciate the useful comments from the peer-review panel which will certainly help improve the scientific contents and presentation of this chapter. The coauthors have carefully reviewed and addressed the comments on their text accordingly. Point to point response is provided below.

Comment #1: *This chapter deals with the serious problem of high and unacceptable methyl mercury contamination of higher trophic level organisms, specifically largemouth bass (LMB); and as outlined in previous reports alligators, the Florida panther, and fish eating birds. A serious human health problem exists in that sport fish Hg concentrations are 300 – 400 % above the US EPA limit for safe human consumption. Consequently, the fish advisories must continue (p.9). 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. The Everglades region have the highest methyl mercury contamination of fish and other biota in Florida. Methyl mercury contamination is biomagnified through the food web and water column concentrations in ng/L range suffice to cause a problem.*

Response #1: We agree with the panel assessments on the status of mercury in fish from the Everglades. We will add a statement to reflect the fact that additional long-term reductions in THg are not apparent in the data.

Comment #2: *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 (SO42-) reduction without significant sulfide (S2-) formation. However, a close relationship between sulfate concentration and mercury methylation rate has not been confirmed by recent research (p. 2, line 47-49).The primary source of sulfur is the atmospheric inputs and the Everglades agricultural area.*

Response #2: The text *"However, a close relationship between sulfate concentration and mercury methylation rate has not been confirmed by recent research"* is unclear as to precise meaning, and suggested replacement text is:

"The contrary effects of sulfate and sulfide on methylmercury production are major drivers of methylmercury accumulation in aquatic ecosystems including the Everglades. Research on the role of sulfur in controlling net methylmercury production has demonstrated that the sulfate concentration at which methylmercury production or accumulation is maximal is generally between 7 and 14 mg/L. For waterbodies below that optimal sulfate level, addition of sulfate stimulates methylmercury production. Further studies are needed to better understand other factors, such as dissolved organic carbon, which influence the rate of microbial methylation of mercury in the Everglades."

The sulfur mass balances indicate that the primary source of sulfur is EAA soil oxidation and Lake Okeechobee. The contribution of agricultural sulfur amendments to sulfur contributions from EAA soil oxidation has yet to be determined.

Comment #3*: Linking methyl-mercury formation directly to only sulfate concentrations may be an over simplification. Apparently, Hg methylation is done by sulfate reducing microorganisms but these organisms also require a rather narrow suite of organic matter forms to supply electron donors. It would also presumably require methane as a source of methyl groups. However, methane production by anerobic digestion competes with SRBs so that high levels of sulfate may inhibit production of methyl groups. Additionally, the sulfide produced by SRB activity can combine with mercury ions to form insoluble mercury sulfide complexes that most likely cannot be absorbed by the biota. These observations may partly explain findings reported in this and previous reports that Hg methylation is maximized when EPA sulfate concentrations are between 1 to 5 mg/L, which are relatively small, borderline to natural conditions. This may be a simple synthesis of the panel's knowledge about this complex process which should be proven or disproven by research that could subsequently lead to better understanding of the process and abatement.*

Response #3: "Linking methyl-mercury formation directly to only sulfate concentrations may be an over simplification." This is certainly true just as linking lake phytoplankton productivity to phosphorus is an over simplification, but it nonetheless is a predictive relationship and a mechanistic one. As mentioned, more research is needed on DOC and mercury methylation. We will address this issue in the chapter revision. New findings come daily as: "Bacterial growth phase influences methylmercury production by the sulfate-reducing bacterium Desulfovibrio desulfuricans" (http://www.sciencedirect.com/science/article/pii/S0048969711006620).

Methane is not the source of the methyl group for Hg-methylating SRB, and methanogenesis is not required for Hg methylation by SRB or FeRB. Both groups of organisms produce MeHg in pure culture without a source of methane. Bartha et al demonstrated a path from lactate to MeHg in Desulfovibrio in the early 1990's (Berman and Bartha 1990; Choi and Barhta, 1992, 1994). In this organism, the methyl group derives from XX (whatever organic substrate the SRB can use), and is transferred along standard intracellular methyltrasferase pathways including Me-B12.

Hg-sulfide complex formation is a complicated topic, and the subject of much ongoing research. However, we have known for more than a decade that the Hg-S complexes that form at low to moderate (low uM) sulfide concentrations are bioavailable for uptake and methylation (Benoit et al. 1999; Drott et al. 2007). Recently the demonstration of the formation of HgS nanoparticles at these sulfide concentrations has advanced our knowledge of Hg-S complexation (Deonarine and Hsu-Kim 2009). Importantly, it is now clear that dissolved organic matter holds HgS nanoparticles in solution by capping the particles and slowing their growth (Ravichandran et al. 1999; Aiken et al. 2011). Tests of the bioavailablity of these DOM-nanoparticle complexes are the edge of the current science.

Hg methylation is maximized when ENP sulfate concentrations are between 1 to 5 mg/L. The WCA mercury methylation optimum sulfate concentration is higher as - the 2011 SFER states "Studies have shown positive correlations between MeHg production and surface water sulfate concentrations in the WCAs up to 20 ppm sulfate". It is not at all clear that 1 to 5 mg/L sulfate is "borderline to natural conditions". ENP sulfate levels distant from canal inputs (and seawater) is <0.1 mg/L; a mere 2% of 5 mg/L. We will go through the text and make sure this difference is well stated.

Comment #4: *The model presented on pages 3B-11-14 should be prefaced by pointing out that it only occurs in anoxic sediment and also if no oxygen and nitrate are present. Oxygen and nitrate are the preferred electron acceptor over sulfate. The schematic on Figure 3B-6 assumes*

simultaneous (1) formation of both acetates and methane by anaerobic digestion of organic sediments, (2) reduction of sulfates to sulfides by sulfate reducing bacteria (SBR), and (3) availability of methane and mercury for methylation. As pointed out previously, sulfides can

outcompete methane for available Hg by forming the insoluble mercury sulfide complex - cinnabar. So, the process is likely far more complex than illustrated on Figure 3B-6, nevertheless the discussion on pages 3B-11-14 realistically represents the process in simplified form.

Response #4: We agree that the model should point out that methylation occurs under anaerobic conditions –we will also note in revision that all Everglades peats/soils become anaerobic with depth – most within the top 1 cm, even in the most oligotrophic areas. See comments above on methanogenesis – it's not needed for Hg methylation.

Regarding "sulfides can outcompete methane for available Hg by forming the insoluble mercury sulfide complex", new finding are that the character of DOC matters greatly in how well HgS is held in solution, and DOC slows the growth of nanoparticles. There are similar findings on the bioavailability side. The degradation of peat in the EAA provides a very different type of DOC than what is found in the central Everglades – the much more aromatic DOC in WCA2A makes HgS much more soluble and bioavailable than does DOC from say 3A15. We will discuss more on the importance of the type of DOC from EAA in revision.

Comment #5*: This section also realistically describes the difficulties in abating and controlling the methyl mercury problem. 95% of input Hg originates from atmospheric sources (power plants, agriculture) and only 30% or so can be attributed to local sources, the rest coming from national (non-Floridian) and international sources. Thus significant Hg input reduction can only be achieved by international treaty and or regulation at the Federal level. Regardless, reduction of Hg release by coal fired power plants is the only long term solution for reducing the mercury problem. Dental offices used to be another significant source, but most dentists do not use mercury amalgams anymore. However, there may be some legacy Hg pollution in the sediments from this and other local sources.*

398-429: Several locations of the report allude to "international" (defined as non-Floridian) outputs as the source of mercury in the Everglades, and thus suggests that only international treaty could reduce inputs. However this section appears to indicate that local sources may account for between 20% and 44% (30% of 70% to 46% of 90%) of the total atmospheric flux to the system. Additionally the data presented in Figs. 3B-7 and 3B-8 on a nationwide scale would seem to indicate that much of the non-Florida sources might be generated within the conterminous US. Thus Floridians might have control over a significant portion of the total flux of Hg to the Everglades system, and much of the remainder might be controllable at the Federal level. Statements that Hg source reduction can only be affected by international treaty is an overstretch of the reality.

Response #5: We will add text to this SFER chapter to make it clearer why we believe atmospheric mercury contributions from local (Florida) sources to the Everglades are low and that international mercury sources may be dominant.

Text above "local sources may account for between 20% and 44% of the total atmospheric flux to the system", are from 1995-1996 data.

Text from the 2008 SFER chapter: "The primary air emissions sources of mercury in South Florida circa 1990 were from municipal and medical waste incinerators. Mercury emissions from incinerators of all types have since declined by approximately 90 percent. Principal reasons for this decline were pollution prevention activities that resulted in reductions of mercury concentrations in waste, as well as incinerator emissions controls (RMB, 2002; Atkeson et al., 2005)."

"Atmospheric mercury contributions from local (South Florida) sources are estimated to have declined from 51 percent of total atmospheric sources in 1991, to 21 percent in the observation period spanning from 1995 through 1996, and 9 percent in 2000 (Pollman et al., 2005b; Pollman et al., 2007)."

The 9% (year 2000) local source mercury contribution estimate is supported by subsequent REMSAD modeling which predicted that atmospheric mercury contributions to the Everglades from local sources was 11% (based on year 2001 meteorology and updated year 2001 mercury emissions). Modeling by the University of Michigan will soon give us better estimates. For the present, the statement that mercury source reduction for the Everglades can only be significantly reduced by international treaty is the best estimate of reality, and argues for sulfur mass balance research as an alternative to mercury source control to control Everglades fish mercury levels.

Comment #6*: But the far more uncertain way of abatement is to reduce the input of sulfur to the EAA. It was already pointed out that there is only a narrow 1-5 mg/L window, confirmed on Figure 3B-12 for mosquito fish, that would result in methyl mercury formation and biological uptake in the Shark River Slough. Further note that the reported literature review (pgs. 3B-33-36) suggests that methylation is optimized at approximately 10 mg/L. Presently, sulfate concentrations in the EPA are as high as 70 mg/L. So, a realistically asked question is whether the higher observed sulfate concentrations cause methyl mercury contamination or prevent it. A similar model has been recently found for the effects of nitrate on eutrophication whereby higher nitrate concentrations retard or prevent eutrophication*

1. Investigation of this hypothesis could be added to the list of recommendations for future observations and research on page 3B-15 and 3B-36-37. A possible proof of the hypothesis that medium-high sulfate concentrations in water and pore water can suppress methylation can be found on page 3B-19, describing the effect of hydraulic inflow modifications. Prior to the opening of STA-2 in July 2001, sulfate concentrations in the EPA ranged from 5 to 17 mg/L. The post-discharge average sulfate concentration in northwestern WCA-2A was 60.8 ± 1.0 mg/L and currently may be as high as 70 mg/L. Yet the THg fish contamination reported on Figure 3B-1before 2000, in the time of the lower concentrations of sulfate, were three times higher than they are today under much higher sulfate concentrations. Needless to say the review panel advances the hypothesis that reducing sulfate loads to the EPA could be actually counterproductive for reducing mercury methylation very cautiously, but this possibility must be at least explored before moving forward on management changes of sulfur loads that could exacerbate the problem.

Response #6: Regarding the comment "*It was already pointed out that there is only a narrow 1-5 mg/L (sulfate) window...*" sulfur mass balance studies will assist in determining if we can manage sulfur to achieve sulfate levels below 1 mg/L in the Everglades National Park. Natural ENP levels may be < 0.1 mg/L.

Regarding the comment "*So, a realistically asked question is whether the higher observed sulfate concentrations cause methyl mercury contamination or prevent it.*" The answer is "both" as per the suggested new text: "The contrary effects of sulfate and sulfide on methylmercury production are major drivers of methylmercury accumulation in aquatic ecosystems including the Everglades." Research underway modeling sulfate loading reductions and resultant sulfate concentrations across the EPA and resultant MeHg levels in Gambusia across the EPA, will give us some insight into this matter.

Regarding the comment "*Prior to the opening of STA-2 in July 2001, sulfate concentrations in the EPA (WCA-2A) ranged from 5 to 17 mg/L. The post-discharge average sulfate concentration in northwestern WCA-2A was 60.8 ± 1.0 mg/L and currently may be as high as 70 mg/L. Yet the THg fish contamination reported on Figure 3B-1 before 2000, in the time of the lower*

concentrations of sulfate, were three times higher than they are today under much higher sulfate concentrations." Figure 3B-3 shows LMB mercury concentrations over time in WCA-2 and there is little variation from 1997-2010. Prior to 1997, mercury atmospheric deposition rates were thought to be much higher than after that date.

Regarding the comment "that reducing sulfate loads to the EPA could be actually counterproductive for reducing mercury methylation", that will be certainly true for some locations given the contrary effects of sulfate and sulfide on methylmercury. We will discuss in revision on the possibility of increased methyl mercury production due to reduction of sulfate. As mentioned, modeling in progress will provide useful insights on this. To extend this point to an extreme, why not add so much sulfate to the Everglades so resultant sulfide represses mercury methylation? One answer is because sulfide is already at toxic levels in the northern EPA, see lines 503-506, sulfide can be over 50 times the USEPA standard.

Comment #7: *Pages 3B24-26 describe research of sulfur impacts on wetlands and mobilization of phosphates from wetland substrate. It was found that sulfate reduction in substrate had no effect on P mobilization, confirming that sulfate has the same effect on retardation of eutrophication as nitrate and, obviously, oxygen in water and upper sediment. As long as sulfate (and/or nitrate and/or oxygen) is present in the upper sediment (substrate) layer, phosphorus in the form of orthophosphate is locked with Fe(III) in low solubility iron phosphate complexes. Only after most of the sulfate is converted to sulfide, Fe (III) is reduced to Fe(II) and phosphorus is released into the solution, which is confirmed on Figure 3B-13.*

Response #7: The panel comments are consistent with the findings from the sulfur-induced phosphorus release and plant toxicity project carried out by DB Environmental Inc. We suggest the following statements be added to the key findings section:

"Laboratory and field studies were performed to elucidate the potential for high sulfate levels causing either soil P release or toxicity to cattail and sawgrass in south Florida marshes. Studies in both STAs and WCAs demonstrated no increase in either surface or pore water SRP in response to sulfate enrichment. Low substrate quality, P limiting conditions, and/or low iron concentrations in the wetlands likely contributed to the lack of observed soil P release. Similarly, to date neither cattail nor sawgrass have exhibited an adverse growth response, quantified by leaf elongation rates, to high sulfate/sulfide levels. Oxidized plant rhizospheres, and high temporal and spatial variability in porewater sulfide levels, probably contribute to the lack of observed plant growth response to this constituent."

Specific questions and comments by line number

Comment #8: *78-86: Did different management agencies sample specifically different locations, or collectively sampled fish simply sent to different labs for analysis?*

Response #8: This text will be edited to better reflect the actual sampling programs represented, along with their analytical methodologies, by the data presented in this section. Samples are collected by FWC and reported in the SFER in Appendices 3B-1 and 5-5 as part of SFWMD permit compliance under the Everglades Forever Act (EFA) as well as for human health assessment under programs funded by FLDEP with the latter data being submitted to FL DOH for risk assessments and FLDEP to identify impaired surface waters (Chapter 62-303, F.A.C.). During WY2011, a large number of sport fish were collected for human health risk assessment from the Everglades Protection Area (EPA) and we took advantage of this additional data in this report to provide some background on the current status of the human health risks from consumption of fish from the Everglades region. This is, after all, the endpoint of meHg management in the EPA and the Everglades region in general. Different locations were sampled under the two sampling programs; however, results for EFA collections were used to supplement

human health risk assessment data submitted to FLDOH. It is important to identify that each sampling program employed different analytical methodologies, otherwise all field and laboratory procedures where similar. The SFWMD permit compliance samples were analyzed for THg in fish tissue by thermal decomposition of tissue using USEPA Method 7473 (Mercury in Solids and Solutions by Thermal Decomposition, Amalgamation, and Atomic Absorption Spectrophotometry) while the remaining fish collected under for risk assessments were analyzed by the FLDEP laboratory by USEPA Method 245.6 (Mercury in Tissues by CVAAS) after acid digestion. Since the development of thermal decomposition methods, which require less tissue are quicker and less expensive to analyze, there have been concerns over compatibility of combining data in a monitoring program using these distinctly different methods. There has been extremely good agreement between the methods for split samples (n=286) analyzed over the past 4 years and all permit compliance monitoring results were generated by the SFWMD laboratory. We will add text to clarify these issues in the chapter revision.

Comment #9:99-100: Does this imply that largemouth bass are more susceptible to ambient MeHg than other fish? The comparison of LMB to what is not clear.

Response #9: This discussion generally was focused on MeHg bioaccumulation in LMB, but all species of fish vary in mercury content across the EPA similarly. It is well understood that MeHg bioaccumulation varies both temporally and spatially across aquatic habitats, including the EPA, in response to variations in a wide range of environmental variables. These factors control the ambient concentration of MeHg that is available for LMB, as well as other species of aquatic life. It is also understood that life history parameters of an individual species (i.e. growth, trophic position and exchange rates) can influence variations in MeHg bioaccumulation habitats. Focused mainly on LMB, we feel that variations in life history parameters play a much smaller role in explaining variations in MeHg bioaccumulation as compared to variables that control the ambient concentrations of MeHg available for bioaccumulation across the EPA (i.e. cycling of sulfur and dissolved organic carbon). We will revise this section to make sure the above statements are well presented.

Comment #10: *110: "Length adjusted" implies fish length or time length?*

Response #10: Fish length. The text will be edited to reflect this.

Comment #11: *296-319: The form of the DOC would seem to as or even more important than just the overall quantity. As discussed in the report, the form of DOC controls how much Hg-OM complexation there is, but as discussed above, it probably also influences the quantity the HG that can complex with sulfide and will certainly influence the competition between methanogenic and SRB microbes. Since it would appear that both processes are required to methylate Hg, OM composition as well as quantity would seem to be an important driver of the process.*

Response #11: DOC as commented on previously is important and new publications out next month will inform this SFER chapter. Methanogenesis is not required for Hg methylation by SRB or FeRB. We will revise the key finding adding the following statement "Further studies are needed to better understand other factors, such as dissolved organic carbon, which influence the rate of microbial methylation of mercury in the Everglades". Please also see Reponses #2.

Comment #12: *536-541: Comparisons between MeHg production between the WCAs and the Shark River Slough area indicate that optimal sulfate concentrations vary between the two locations. Porewater chemistry experiments have been conducted in the WCAs but apparently not in the Slough area. A comparison of pore water chemistry between sites might shed light on the different controlling factors. Experiments should include not only sulfate/sulfide couples but also methane concentration and DOC concentration and composition as well as Hg-S complexes in*

the sediment. Microbial assays might also shed light on the competition between methanogenesis and SRB.

Response #12: We agree with the panel assessment. These recommendations are in part reflected in " Future Research Needs" and will be further addressed in chapter revision.

Comment #13: *556-603: Regardless of the effect of sulfate concentrations on Hg methylation, better optimization of sulfur amendments to various crops grown in the EAA will beneficial to better ecosystem health as well as profit margins for producers. Therefore these studies should be continued.*

Response #13: We agree with these recommendations and these studies will continue.

Comment #14: *619: Care should be taken when using the word "significant". If there was a statistical test to confirm this then a "P" value should be reported, otherwise best to not drop the word.*

Response #14: A P-value will be inserted into the document.

Comment #15: *643-647: How ports and values minimized static head differences without influencing the water chemistry by flow cross contamination is not clear.*

Response #15: It is hard to explain how the static head differences are minimized without going into a lot of detail. A detailed description of the mechanics of the water exchange process is available in the Task 4 Final Report which will be cited in the reference list.

Comment #16: *688-696: A methodology for how "new" and "old" mercury is distinguished should be provided. Was it by isotope ratio analysis?*

Response #16: During the construction of Hg mass budget, the "new" Hg referred to the Hg that was deposited from the atmosphere into the Everglades during the 2005 wet season (May through November), while the "old" Hg referred to the Hg that was accumulated and stored in the system. Since this section used the models to construct the Hg mass balance, geochemical differentiation between the "new" and "old" Hg is not necessary. The revised version will define the "new" and "old" Hg at the beginning of the section.

Comment #17: *707-712: The units on the rate constants are not clear. More importantly rate constants without mention of the specific kinetic model are not useful, since they are model dependent.*

Response #17: The data was fitted with a second-order reaction model and such information will be added in the revision.

Comment #18: *720-721: A little more detail is warranted. How does DOM concentration and composition influence photodegradation?*

Response #18: The following sentences will be added to describe the effect of DOM on MeHg photodegradation: "In addition to affecting light attenuation, DOM may control the cycling of such free radicals as hydroxyl radicals. Both processes are important in the photodegradation of MeHg."

Figure and Table comments

Comment #19: *Table 3B-1: Since spatial gradients are so important to the analysis a figure indicating the locations of the sample locations would be beneficial.*

Response #19: It is our intent to include such a figure.

Comment #20: *Table 3b-2: Please provide units for Mean THg*

Response #20: Unit (mg/kg) will be added to the table heading.

Comment #21*: Figs 3B-1-4: Why are 2011 data from the SHARK region in included in the figures, but 2011 data from other regions not included?*

Response #21: All collections, with the exception of samples from the Shark River Slough, are collected during the fall of each year. Shark River samples are collected in the spring. Both sample periods fell within the reporting year for FY11 of May 1, 2010 to April 30, 2011.

Comment #22: *In figure 4 (and other locations within the document) Is "the refuge" the same thing as WCA 1? If so please be consistent.*

Response #22: In the text "Refuge" and "WCA1" are one and the same; however, text will be edited for consistency. We will use WCA-1 in this revision to be consistent.

Comment #23: *Fig 3B-6: This a good conceptual figure but we take issue with a couple of statements on the input reduction column. First there may be some minor ability to adjust the concentration and more likely composition of DOC via plant and/or microbial assemblage modification. Further, as stated in the broad comments, there are likely more options that just international treaties to reduce Hg deposition.*

Response #23: We will revise the text to provide a better description of the atmospheric mercury contributions to the Everglades from local sources (see Response # 5). As previously stated, best estimates indicate that atmospheric mercury contributions to the Everglades from local sources are ca. 10% of total; international sources are dominant. Modifications to plant and/or microbial assemblages to alter DOC composition and reduce mercury methylation could be a controversial management action whether proposed for the EAA or the EPA.

Comment #24: *Fig 3B-7: The number for the application rate is missing in the footnote*

(Should be Table 3B-3—author)

Response #24: The number for the application rate as the Table 3B-3 footnote will be cited in the revision of Chapter 3B.

Comment #25: *Fig 3B-11: Several areas are mentioned in the accompanying text (lines527-535). These areas should be noted in the figure.*

Response #25: Area names in the text will be noted in Figure 3B-11 in the chapter revision.

Comment #26: *Fig 3B-12: Accompanying text (lines 542-555) indicates mercury methylation is optimized at this site when sulfate is between 1 and 5 mg/L and DOC is between 10-20 mg/l. However this statement may not be justifiable from the provided data. First there are very few data points outside this range and there appear to be as many (or more) points within the range with low MeHg values as with high values. We suspect that if MeHg values were regressed against SO4 concentrations no correlations would be apparent. This observation may have ramifications for the entire premise that there is a correlation.*

Response #26: Regarding the comment "*…lines 542-555) indicates mercury methylation is optimized at this site when sulfate is between 1 and 5 mg/L… there are very few data points outside this range and there appear to be as many (or more) points within the range with low MeHg values as with high values.*" There are sulfate data points below 1 and above 10 mg/L. Figure 3B-12 I believe supports the conclusion that mercury methylation is optimized at this site when sulfate is between 1 and 5 mg/L and DOC is between 10-20 mg/L. However, we agree and will state further data collection will help improve our understanding of the critical range of sulfate for optimal MeHg production.

Regarding the comment "*We suspect that if MeHg values were regressed against SO4 concentrations no correlations would be apparent*." That is certainly true; the relationship is non-linear. While sulfate does not explain all the variance in methylmercury production, many studies indicate it is significantly (non-linearly) related, and importantly, many studies demonstrate it is mechanistically related. We will review related sections and make sure this point is clearly stated.

Comment #27: *Fig 3B-18: The 10% values appear to be curt off at the bottom.*

Response #27: A new figure with the 10% values at the bottom will be used in the revision.

Comment #28: *Fig 3B-21: These data across many studies would seem to indicate that methylation is optimal somewhere in the 10 mg/L sulfate range. Reported optimal values most distant from that are typically at the highest range tested if lower of at the lowest range tested if higher, further suggesting the optimal is closer to 10 mg/L. There are very few data points from the EPA near this so the suggestion that the optimum is lower, e.g. Fig 3B-12 is suscept.*

Response #28: We agree with the panel assessment that methylation is optimal somewhere in the 10 mg/L sulfate range (likely 7-14 mg/L) based on the literature data (will appear in the Key Findings section). Figure 3B-12 is distribution of surface water MeHg versus sulfate and dissolved organic carbon (DOC) in the ENP (figure from D. Krabbenhoft). Figure 3B-21 is summary of literature reports of the effect of experimental sulfate amendments on MeHg production in sediments and soils (from C. Gilmour). Figure 3B-12 is for the ENP; Figure 3B-21 has no ENP data.

Editorial page and line comments

Comment #29: *24: Change to median mercury concentrations in largemouth bass average twice*

Response #29: Agreed and change will be made.

Comment #30: *115 In The*

Response #30: Agreed and change will be made.

Comment #31: *116: The rationale*

Response #31: Agreed and change will be made.

Comment #32: *201 Dunn's test*

Response #32: Agreed and change will be made.

Comment #33: *202: ignored*

Response #33: Agreed and change will be made.

Comment #34*: 408: Capitalize Wet*

Response #34: Agreed and change will be made.

Comment #35: *769: It is NOT the acidity per se*

Response #35: Agreed and change will be made.

Comment #36*: 787: micromoles not micrometers*

Response #36: Agreed and change will be made.

Comment #37: *801: Deltaprotobacteria group?*

Response #37: The correct spelling is Deltaproteobacteria.

Summary and Recommendation

Comment #38: *The monitoring and research on the formation of MeHg in the EPA wetlands and waters and its incorporation into a food chain represents the most comprehensive and advanced research in the world that can be compared to the coordinated research in the 1960s and 1970s following the Minamata mercury poisoning disaster in Japan. The top US agencies and universities cooperated on the research and the discoveries that are new or reconfirming the old findings are emerging not only in this report but also, hopefully, in the peer reviewed literature that will follow this research effort. It is difficult to find deficiencies.*

Response #38: Thank you.

Comment #39: *The only major suggestion to the researchers participating on this research is not to adhere to forming linear relationships of the MeHg formation in the sediments and mercury absorption into fish (and higher prey organisms) to sulfate concentrations and not mention other factors. The previous SFER reports and literature confirmed that there is a narrow "window" of sulfate concentrations at which the MeHg is formed and absorbed into the biota. It is quite possible that reducing current sulfate concentrations may have an adverse or no effects. The remedial measures are reducing Hg availability and/or locking somehow incoming Hg by sulfides and storing it forever into the sediment as nano-size cinnabar.*

Response #39: Regarding the comment "*It is quite possible that reducing current sulfate concentrations may have an adverse or no effects*." We concur with this comment and will revise the section of Future Research Needs" to include this research. In addition, ongoing modeling will help determine whether there are adverse or no effects on mercury methylation rate, but the net effect of reductions of sulfate loading on mercury methylation plus reduction in toxic sulfide is likely to be positive as is pointed out in the next paragraph. Also see Response #6.

Comment #40: *This does not mean that excessive sulfur applications onto EAA fields should continue, they could be reduced or the input of sulfur in the drainage canals diverted somewhere else, but the impact on MeHg formation and absorption may be initially minimal and, because the oxygen and nitrate concentration are often low in the system, may even cause problems with the release of phosphorus from the sediments. However, excessive reduction of sulfates to sulfide in receiving water is generally undesirable for other reasons that were not covered in the chapter. Sulfate reduction by SRBs in water and sediments creates black sulfide coatings on sediment and death of aerobic life, which is also accompanied by the release of methane and smelly hydrogen sulfide from anaerobic sediments. The problem, however, is the lack of oxygen, which is unrelated to sulfate. In the review of Chapter 3 it was pointed out that DO concentrations throughout the EPA system can reach very, near zero concentrations which then result in anoxic anaerobic substrate and sediments. There are large areas, especially in the Everglades Park, where sulfate concentrations are in the MeHg formation 1-5 mg/L window. If enough oxygen (and/or nitrate) is present, sulfate reduction does not occur and methyl mercury is not formed. Maybe, we are forgetting something in this fight against formation of MeHg and its absorption and bioaccumulation by biota.*

Response #40: We agree with the panel assessment on the importance of sulfur reduction, and the status and role of DO in sulfur cycle. However, it does not seem feasible to greatly increase DO in this wetland, and it may not wise to increase nitrate.

Comment #41: *The section on future research needs appears to recognize that new research will need to focus on a mass balance modeling approach so that predictions in measurable decreases in Hg bioavailability can be correlated with measurable changes in inputs such as sulfur loads. This a good approach, but it is clear that the model must include all the issues surrounding mercury methylation and avoid the linear thinking described above.*

Response #41: We fully recognize the relationship between sulfate concentration and MeHg production is not linear. We will review related sections to make sure this conclusion is well stated in the text.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 4

William Baker and Chapter Coauthors

Level of Panel Review: Accountability
Reviewers: V. Novotny (AA), J. Burkholder (A)

Posted: 10/05/11 at 09:04 AM by W. Baker

Comment #1: *Page 5 Table 4-1 summarizes the phosphorus loads from all watersheds. Because of the relatively dry year, some tributary canals had no flow. To be consistent with the units in this table (and throughout the report) a conversion of pounds/acre to kg/ha should be included in the legend.*

Response #1: A conversion note will be added to the table.

Comment #2: *Page 14 – Define ERP/SW and WOD acronyms on this page. ERP (Environmental Resource Permitting) is defined on p. 18. In Table 4.3 which land uses are and which are not included in the total acreage? When the percent of total acreage in column 3 and 5 are added they are more than 100%.*

Response #2: Acronyms ERP and SW are defined on page 14 lines 273 and 276, respectively. WOD is defined in the previous section on page 7 line 90.

In Table 4-3, all land use types are included in the total sub-watershed acreage which can be found in Table 4-1. The percentages shown are by sub-watershed and are not intended to be added across sub-watersheds, i.e. a sub watershed having complete coverage (100%) is still only a fraction of the overall watershed. The overall watershed coverage is shown in the last Grand Total row.

I. Northern Everglades (source control programs legislated for both phosphorus [P] and nitrogen [N] in rivers, and for only P in the Lake Okeechobee watershed - p.4-7).

Comment #3: *Unlike the source control programs for the Southern Everglades (p.4-35), the Northern Everglades' programs are not described to include education/outreach components (p.4-13). Based on previous SFERs, this description is in error; the District has engaged in extensive education/outreach efforts throughout the area under its purview. Therefore, please add information about the District's education/outreach efforts for the Northern Everglades source control programs.)*

Response #3: Educational outreach in the Northern Everglades has been mentioned in previous SFERs, mainly programs administered by the FDEP and UF/IFAS (see 2010 SFER page 4-19). This year, there is some information in the River Watershed section regarding training and education included in the FDACS Source Control Program, which applies to the Lake Okeechobee Watershed as well. This reference will be added to the Lake Okeechobee Section.

Lake Okeechobee –

Comment #4: *Lines 319-321 – "The District WOD source control program boundary has not yet been amended to be consistent with NEEPP. That is, it currently does not include the Upper Kissimmee or Lake Istokpoga sub-watersheds." Please add brief clarification: How long is this anticipated to take?*

Response #4: Clarifying text will be added: The District is currently in the process of developing performance measures, in accordance with NEEPP requirements, for the Lake Okeechobee Watershed. Once these measures have been developed (estimated to be completed in 2012), the District will request approval from the State of Florida's Office of Fiscal Accountability and Regulatory Reform to amend Chapter 40E-61, F.A.C. Rule amendment efforts will be subject to the directives provided under executive orders and any further requirements from the Office of Fiscal Accountability and Regulatory Reform.

Comment #5: *Lines 331-333 – Explain more clearly for readers – what are the current discharge target concentrations, and why are they "outdated"?*

Response #5: In efforts to streamline the SFER in 2012, discussions that are in previous versions of the SFER, or other regulatory documents have been referenced. A note will be added to the final chapter to direct the reader to these references with more information. A summary of the current rule can be found in *Chapter 4 of the 2011 SFER, STATUS OF SOURCE CONTROLS IN THE LAKE OKEECHOBEE WATERSHED, BACKGROUND*. The current discharge target concentrations can be found in 40E-61 F.A.C.

Comment #6: *Table 4-4 – Explain why only ~24% of agricultural areas in the Upper Kissimmee are enrolled, and specific plans, if any in addition to the general plans previously described, to increase that percentage. Also, please explain the value of 134.2%.*

Response #6: Two notes will be added to the table: The enrollment acreage for the Upper Kissimmee Sub-watershed is lower than others due to the initial focus on priority sub-watersheds within the Lake Okeechobee Watershed. The Fisheating Creek/Nicodemus Slough Sub-watershed shows an enrollment greater than 100% due to the inclusion of cattle grazing BMP NOIs on forestry lands that are not classified as agriculture use.

Comment #7: *Line 346 – Briefly explain the incentives.*

Response #7: In efforts to streamline the SFER in 2012, discussions that are in previous versions of the SFER, or other regulatory documents have been referenced. A reference will be made to the FDACS site http://www.floridaagwaterpolicy.com for information regarding their incentive-based program.

Caloosahatchee Watershed –

Comment #9: *The PM for this watershed is nutrient loading at the watershed level. Please explain why concentrations are not also considered as a PM.*

Response #9: Performance measures (PM) for source control are under development. Upon completion, a preliminary proposal will be made on whether concentration, load, or combination-based performance measures are advisable.

Comment #10: *Lines 383-384 – How is it "being ensured" that biosolids do not contribute to watershed nutrient loadings?*

Response #10: Present and future activities to ensure that biosolids do not contribute to watershed nutrient loading include:

For FDEP, the implementation of the Biosolids Rule, Chapter 62-640, F.A.C. prohibiting land application of Class B biosolids by January 13, 2013, unless a nutrient balance demonstration is completed,

For FDACS, continuing enrollment in Notice of Intent that include nutrient management BMPs, and conducting BMP implementation assurance activities to verify that Class AA biosolids are applied at agronomic rates for phosphorus and nitrogen, and

The District will implement a regulatory source control program including development of performance measures and on-going tributary water quality collection and analysis to verify that implementation of collective source controls, including those for Class AA and Class B biosolids, are reducing nutrient loading.

Comment #11: *Lines 401-402 – How is progress being tracked to implement the plan? – please clarify.*

Response #11: This information is detailed in the 2012 Protection Plan update, as described in the Appendices to Chapter 10.

Comment #12: *Tables 4-5, 4-6 - Further explanation about these tables would be helpful: What is the overall goal of the percentage of sub-watershed acreage, 100%? Is there a timetable for achieving the overall goal for the Caloosahatchee and the St. Lucie watersheds? Why are some percentages so low? (e.g. Coastal Caloosahatchee, 11%)*

Response #12: The goal for implementation for the Surface Water and Environmental Resource permits (SW/ERP permits) is that 100% of the projects involving the alteration of surface water flows are covered. However, not 100% of a sub-watershed acreage may need to be covered, e.g., non-irrigated pasture areas do not require SW/ERP permits.

Regarding the percentage of acreage covered and low levels, please note that the percentage coverage presented in Tables 4-5 and 4-6 only includes District-issued permits per the operating agreement concerning regulation under Part IV, Chapter 373, F.S., between the District and FDEP. For instance, in coastal areas (like in the Coastal Caloosahatchee sub-watershed), FDEP is responsible for issuing ERP/SW permits to projects that may involve wetland resources (dredge and fill), docking facilities, boardwalks, shore protection structures and piers, etc. which are more prevalent in this sub-watershed.

Comment #13: *Lines 447-448 – It would be helpful to provide a page number here.*

Response #13: Clarifying text will be added.

Comment #14: *Line 496 – Briefly explain why these discharges are considered negligible.*

Response #14: The draft chapter indicates that discharges from the S-4 sub-watershed to Lake Okeechobee through the L-D1 culverts are considered to be negligible. This assessment is based on the historical data analysis conducted by Goforth et al. (June 2011) as part of District contract No. 4600002337 (Performance Measures Methodologies for Collective Source Controls in the Lake Okeechobee and Caloosahatchee River Watersheds). The analysis indicates that the L-D1 culverts can discharge water in both directions and serve to equalize water levels between the Lake and the S-4 sub-watershed. However, the culverts have limited discharge capacity and are only opened when the lake stages fall below 13.0 ft, NGVD, thus it was assumed that the flows and loads are negligible. There are no flow or water quality data available for these structures. A reference to the document will be added, as described next (underlined text):

"Nutrient discharges from private structures in the S4 sub-watershed into the East Caloosahatchee Sub-watershed (Disston Water Control District Structure 3, and the Nine Mile and Flaghole canals, which are not monitored and flow into Lake Hicpochee). Note, however, that discharges

from this sub-watershed to Lake Okeechobee through the L-D1 culverts are considered to be negligible (Goforth et al, 2011)."

Comment #15: *Lines 497-502 – Explain how these will be evaluated to see if network improvements or hydrologic modifications will be needed (lines 484-485).*

Response #15: The relevance of the nutrient discharges from the East Caloosahatchee Sub-watershed into the West Caloosahatchee Sub-watershed via private canals connecting canals 3 and 2, in relation to flows through S-78 is being determined based on review of the water control plan required pursuant to Chapter 298, F.S., any surface water or environmental resource permits and supporting documentation, MikeSHE modeling support documents associated with the 2009 Protection Plan update, field verification and inquiry with land owners, as needed.

Comment #16: *Line 505 – Please clarify – are Marshpoint and Courtney Canal significant or have they been estimated to be small/minor? Basis?*

Response #16: The SFER describes the stations that are monitored by the District or others. We are not aware of the basis for which monitoring is not conducted at Marshpoint and Courney Canal, while it is for other streams. This is an item for follow-up as part of the development of a representative water quality monitoring network.

St. Lucie R Watershed –

Comment #17: *A substantial part of the watershed (130,000 acres) is described as not represented by monitoring, which seems to be a sizeable gap, and (lines 519-520) mentions the need for 12 to 20 additional water quality or flow locations that should be permanently monitored. Please explain the substantial range in the number of additional monitoring stations that have been identified as needed to adequately assess the St. Lucie watershed.*

Response #17: The reason for the substantial range in the number of additional locations (12 to 20) derives from the potential use of downstream monitoring locations that can capture various tributary streams, instead of monitoring them individually. However, because downstream monitoring is subject to tidal conditions, technical issues related to the accuracy are currently being investigated.

Please note that the 12 to 20 locations estimate assumes that flow and concentration monitoring in 100% of the tributary acreage is needed to adequately measure the performance of source controls. As the performance measure development process continues, alternative options for adequate monitoring may be developed.

Comment #18: *Table 4-7 shows extremely small percentages of agricultural lands enrolled in BMP programs for the tidal and coastal Caloosahatchee sub-watersheds. Please add explanation about why these percentages are so low. Because these lands are in such close proximity to the estuaries, participation in BMP programs by agricultural entities would seem especially important.*

Response #18: Participation in the FDACS BMP programs is voluntary. However, efforts to increase enrolment can be found in the Florida Department of Agriculture and Consumer Services Nutrient Source Control Programs section of Chapter 4. As the District's development of a regulatory source control program with performance measures progresses in this watershed, BMP implementation is also expected to increase.

Comment #19: *Table 4-8 – Similarly, add brief explanation as to why the percentages of agricultural lands enrolled in BMP programs so low for Basins 4-6 and South Fork in the St. Lucie watershed.*

Response #19: Participation in the FDACS BMP programs is voluntary. However, efforts to increase enrolment can be found in the Florida Department of Agriculture and Consumer Services Nutrient Source Control Programs section of Chapter 4. As the District's development of a regulatory source control program with performance measures progresses in this watershed, BMP implementation is also expected to increase.

Comment #20: *Lines 758-759 – Do the authors mean, "considered and adequately accounted for"? Please clarify.*

Response #20: The lines referenced indicate that one of Florida Department of Environmental Protection (FDEP) objectives is to coordinate with applicable authorities on septage disposal to ensure sufficient nutrient loadings are considered in the St Lucie and Caloosahatchee watersheds. The agency responsible for implementing septage disposal requirements is the Florida Department of Health (FDOH), specifically, that application is conducted in accordance with an agricultural use plan that limits applications based on upon nutrient loading pursuant to statutory requirements. The phrase proposed does not reflect FDEP's objectives.

Comment #21: *Table 4-11 – Please clarify: how much/what nutrient monitoring is being done, e.g. for the substantial number of CAFOs in the St. Lucie watershed?*

Response #21: Clarifying information will be added as a footnote to the table. Monitoring for phosphorus and nitrogen in surface water discharges, groundwater, and land application effluent is included as a condition of the permits. In general, surface water discharges are monitored per discharge while groundwater and land application effluent are monitored at a quarterly or semi-annual basis.

Comment #22: *Lines 206-207 – add brief explanation about why the present evaluation method does not allow agencies to meet this important objective [i.e. to make appropriate changes as needed].*

Response #22: A brief explanation will be added: The current evaluation method within 40E-61 is based on numerical limitations for parcels developed from the 1989 Surface Water Improvement and Management Plan (see 2011 SFER Status of Source Controls in the Lake Okeechobee Watershed for further details) which was produced prior to the establishment of the Lake Okeechobee TMDL. Also, the current method does not evaluate performance and progress of the collective source control program for the watershed. Therefore, the evaluation method is currently being revised to enable the coordinating agencies to meet this objective and to account for the more recent water quality requirements.

II. Southern Everglades (source control programs legislated for P in Everglades Construction Project [ECP] basins; another agency issues long-term compliance permits to the District to regulate P discharge by non-ECP basins

ECP basins –

Comment #23: *An important point in Appendix 4-2 (p.4-2-3) that should be made in the main chapter is that "To interpret TP measurements taken at inflow and outflow water control structures defining the boundary of the EAA Basin, it is important to recognize that water leaving the EAA Basin through these structures is a combination of EAA farm and urban generated runoff and water passing through the EAA Basin canals from external basins. This pass-through water includes discharges from Lake Okeechobee and 298 District conversion areas."*

Response #23: A footnote will be added to Table 4-14 that makes this point.

EAA basins –

Comment #24: *Pp. 4-37, 4-45 – The target loads (predicted reduced by 25%) are estimated based on the 50th percentile confidence level for predicted loads. Limit loads are based on the 90th percentile confidence levels. The authors explain that the different confidence limits are used to accommodate for possible statistical errors in the regression model. It seems clear that the target loads are imprecise, at best. Are steps being taken to improve the regression model so that tighter confidence limits for the target loads can be used? If so, briefly explain these steps.*

Response #24:[2] The cited text will be revised in the final chapter to clarify: "The target loads are calculated based upon the predicted loads reduced by 25 percent." The regression equation target is analogous to a median in that 50% of the data used to calculate will be above and 50% below and future data collected under the exact same conditions would yield a similar pattern. The three year test reduces the theoretical potential for a false positive to 12.5% (50%x50%x50%=12.5%). A false positive being a result of non-compliance when the basin was in fact within the range of base period performance. One of several statistical strength indicators is the Coefficient of Determination (R-squared) value. The current regression equations for the EAA and C-139 Basins have R-squared values of 90.8% and 74.2%, respectively. No efforts are currently being made to improve the regression models.

Comment #25: *Lines 1074-1078 – This important clarifying information should also be repeated as a footnote of Table 4-14.*

Response #25: A footnote will be added to the table with this information.

Comment #26: *Figure 4-9 – This excellent figure should include the information related in lines 1078-1082, to help readers.*

Response #26: This information is currently included in the figure legend and left axis title. The authors are concerned that additional text will decrease readability of the figure.

Comment #27: *Lines 1098-1107 – It would be helpful to include brief information about the frequency of monitoring required; or refer readers to Appendix 4-2 for this information.*

Response #27: The paragraph has will be modified to clearly refer readers to Appendix 4-2 for permit level water quality and quantity data. In addition, a sentence will be added to Appendix 4-2 describing the required frequency of monitoring.

Comment #28: *Lines 1120-1122 – It would be helpful to direct readers to more information specifically about the floating vegetation effort, or to clarify that this information can also be found, in detail, at the above-mentioned website if that is the case*

Response #28: Additional information on the floating aquatic vegetation research can be found in the BMP Research section that follows this paragraph. The referenced paragraph will be revised to clarify.

[2] **Footnote for Final Response to Comment #24:** The 25% reduction is not a margin of safety, but a source control water quality performance goal developed through a stakeholder process and based upon an estimated BMP effectiveness considering factors such as EAA soils, crop types, and implementation assumptions. A detailed description of the percent reduction determination procedures can be found in Chapter 40E-63, F.A.C., Appendix A3 (web-posted on 11/8/2011).

C-139 basin –

Comment #29: *The authors describe discharges from the C-139 basin as not exceeding the targeted limit and meeting the PM, despite the fact that the discharges were above the predicted load from the pre- BMP baseline period adjusted for rainfall. Further explanation/clarification would be helpful.*

Response #29: The annual observed loads are expected to fluctuate above and below the annual target load predicted from rainfall. Three consecutive observed annual loads above the target load, as defined under Rule 40E-63, approximate an 87.5% confidence level of exceeding base period levels. The limit represents the 90% confidence limit load for any single year. The following text will be added to the paragraph to further explain and clarify the C-139 Basin WY2011 phosphorus load performance results: "Due to the rule amendments described above, WY2011, WY2012, and WY2013 observed phosphorus load are only comparable to the limit load under the performance measure methodology and the three-year target test will not be applicable until WY2012 through WY2014 results are available."

Comment #30: *The information stated in lines 1290-1293 should also be given in the legend (or footnoted) of Figure 4-11.*

Response #30: Information regarding STA 5 operations will be added to the figure in the final chapter.

Comment #31: *Figure 4-12 – This excellent figure should include the information related in lines 1078-1082, to help readers.*

Response #31: Figure 4-12 presents C-139 Basin outflow concentrations by water year. The information related in lines 1078-1082 refers to EAA load reductions and is therefore not applicable to this figure. The figure legend clearly defines the line and symbols.

Comment #32: *P.4-50, Integrated Permit Compliance – These valuable efforts are to be commended. The writers mention four follow-up consultations, representing 27.6% of the basin, that were conducted in WY2011. Please provide brief description of the issues and what was resolved, considering that this is a major proportion of the basin.*

Response #32: The purpose of this project is to verify that permittees in the C-139 Basin are up to date with the requirements of surface water, environmental resource, water use, and everglades works of the district permits, and assist them to comply with any outstanding requirements. In that regard, the follow-up consultations conducted aimed to resolve pending issues related to the environmental resource permits. The issues identified and resolved included inconsistencies between the as-built and the permitted surface water management system, and pending engineering and environmental reports (certification, wetland monitoring reports, schedule, etc.). Text will be added describing these issues.

Comment #33: *P.4-50, C-139 Basin Monitoring Network – appears to be a very valuable effort. The Panel looks forward to learning more in future SFERs about the insights gained from this monitoring network about P and flow.*

Response #33: As data is gathered and evaluated, results will be presented in future SFERs.

Comment #34: *P. 4-51, lines 1395-1397 – This comparison will be instructive. Please clarify when it will be available (the next SFER?).*

Response #34: Results are expected in September 2011 (see lines 1477 – 1480) and will be available for the 2013 SFER.

Comment #35: *P.4-51, lines 1419-1425 – When is it anticipated that these plans regarding the Watershed Assessment Model (WAM) will be accomplished? This clarification would be helpful.*

Response #35: This work was completed in April of 2011. The tense of the text will be revised in the final chapter to reflect completion.

Comment #36: *P.4-51, lines 1429-1430 – The goal of balancing annual climate patterns with flood, natural resources (wetlands) protection, and water availability seems very important. It would be helpful to clarify when it is anticipated that this will be achieved.*

Response #36: The purpose of the C-139 Basin Regional Feasibility Study is to identify a suite of alternatives planned to meet this objective. Alternative evaluation is scheduled to be complete in WY12. However, the timeline of implementation would depend highly upon the resources required to plan, design, and construct any specific improvements identified by the study.

Comment #37: *P. 4-52, lines 1447-1449 – Because the drought substantially limited data collection. Will it be possible for the District to extend the study so that this important information can be obtained?*

Response #37: Additional funds for demonstration projects were approved for FY2012 (October 2011-September 2012).

Other ECP basins – C-51 West and L-8 –

Comment #38: *Lines 1496-1501 – add brief description of the number of stations and frequency of monitoring.*

Response #38: This information can be found in Appendix 4-3. A sentence will be added to the paragraph directing readers to this appendix.

Comment #39: *Lines 1507-1509 – This land use/drainage study will yield valuable insights. Please clarify when it is expected that the study will be finished and available.*

Response #39: The study is expected to be completed in WY2012. Clarification will be added to the C-51West and L-8 Basins Study bullet below this paragraph.

Comment #40: *Lines 1524-1532 – It is encouraging to see that the recommended synoptic water quality testing program for the C-51West and L-8 basins is planned to be implemented in WY2012. Given that Lake Worth Lagoon, known for poor water quality, the key water body Lake Okeechobee, and STA-1W and STA-1E are affected by the drainage from these basins (p.4-53), the planned synoptic water quality program should be a valuable addition in source control efforts.*

Response #40: The authors agree with this comment.

Non-ECP Basins (the seven non-ECP basins contribute 12% of the P discharged to the EPA)

Comment #41: *District efforts in these basins understandably are described as more limited than in ECP basins, and mostly consisted of estimating P contributions and continuing demonstration, research, and construction projects to improve water quality in discharges to the EPA. Appendix 4-3 provides detailed information about regulatory source control programs in these basins, including data summaries for flow, TP loads, and flow-weighted mean TP concentrations from WY1998 to WY2011.*

Response #41: No response requested.

Comment #42: *Lines 1579-1581 – This helpful information should be moved toward the front of the chapter.*

Response #42: For the 2012 SFER, we have shortened the Chapter significantly. The information being referred to ("The non-ECP basins have historically contributed approximately 12 percent of the TP load discharging to the EPA compared to the 88 percent contribution by the ECP basins"),

if moved to front of chapter, would not be consistent with information currently in the summary page or the WY2011 nutrient source controls highlights.

Comment #42: *Lines 1620-1625 – Please clarify when it is anticipated that the other three water reserve areas will be constructed.*

Response #42: The project is partly funded by the USACE and it is assumed that completion dates are uncertain due to economic conditions. As information becomes available it will be included in future SFERs.

Comment #43: *Lines 1788-1789 – Add brief explanation as to what this action will accomplish in helping with source controls.*

Response #43: The paragraph will be modified to state that flows will now be treated by an STA prior to discharge to the EPA. Please refer to the C-139 Annex Diversion paragraph on page 58 for a complete description including BMP implementation.

Comment #44: *Lines 1682-1684 and 1689-1691, WY2011 activities in the C-111 basin – Two projects, construction of the North Detention Area and the Combined Structural and Operational Plan, were described as being "on hold." It would be helpful to add clarification as to when it is anticipated that the projects will be able to proceed.*

Response #44: Negotiations between the Corps of Engineers and the District for a new Project Cooperative Agreement for the North Detention Area are currently underway with a target signing at the November governing board meeting. The text will be revised to reflect a projected restart in WY2012.

The Combined Structural and Operational Plan (CSOP) has become the Combined Operational Plan (COP). The project scope is currently under review, but it is expected that this project will also restart in WY2012. The text will be revised to reflect this change and anticipated start timeframe.

Editorial Suggestions

Comment #45: *P.4-4 – It would be helpful here to identify the EAA and C-139 as ECP basins.*

Response #45: Clarifying text will be added to the headings.

Comment #46: *Lines 88-91 – should clarify (as per line 1046) that the 79% decrease was derived after adjusting for hydrologic variability associated with rainfall. Otherwise, because WY2011 was a drought year, readers might consider that this reduction was simply weather-related and not due to (considerable) management efforts.*

Response #46: The sentence will be revised to add clarification.

Comment #47: *Line 100 – …area-specific…*

Response #47: A hyphen will be added to the sentence.

Comment #48: *Lines 125-126 – suggest rewriting*

Response #48: The sentence will be revised.

Comment #49: *Lines 206-207 – =….The evaluation method within 40E-61 is currently being….*

Response #49: The sentence will be revised.

Comment #50: *Line 422 – …Current ERP Program rules require activities to be…*

Response #50: The sentence will be revised.

Comment #51: *Table 4-2, footnote 7 – please rewrite; does not accurately state what is meant.*

Response #51: The footnote will be rewritten.

Comment #52: *Line 426 – …demonstrate that the proposed…*

Response #52: The sentence will be revised.

Comment #53: *Tables 4-9, 4-10 – it would be helpful to include percentages of the total.*

Response #53: Percentages of the will be added next to the acreages.

Comment #54: *Line 921 – …Lucie watersheds, anticipated to….*

Response #54: The sentence will be revised.

Comment #55: *Lines 965-966 – "The District is required by permit…" – please clarify. Does the statement relate to the "master permit" information given on p.4-43 (BMP research)? If so, readers could be referred to this information.*

Response #55: The sentence will be revised to clarify. The reference is to the ECP and non-ECP permits discussed in the paragraph.

Comment #56: *Lines 1127-1129 – It would be helpful to move this definition up to where BMPs are first discussed.*

Response #56: This definition was developed specifically for the EAA and is appropriately placed in the EAA discussion. BMPs are broadly defined as "on-site measures that prevent or reduce pollution at its source" in the second paragraph of the chapter.

Comment #57: *Line 1393 – …if the water is flowing….*

Response #57: The sentence will be revised.

Comment #58: *Lines 1435-1436 – …(4) the contract was executed and Phase II was initiated.*

Response #58: The sentence will be revised.

Comment #59: *Line 1454 – …14 kg…*

Response #59: The sentence will be revised.

Comment #60: *Line 1793 – SFERs, …*

Response #60: The sentence will be revised.

Appendix 4-1 – Lake Okeechobee Watershed

Comment #61: *P.4-1-1 to 2 – The authors aptly state that because source control programs are not fully developed and implemented for the Lake Okeechobee watershed, it is essential to accurately track their implementation rates; and that once PM methodologies are developed and adopted, they will be used in conjunction with implementation rates to evaluate progress toward achieving water quality goals. It would be helpful to give readers an approximation of when it is anticipated that the process of developing and adopting PM methodologies will be complete.*

Response #61: A note will be added in Appendix 4-1 regarding the PM development timeline: Development of Performance Measures is anticipated to be completed in 2012. The District will then request approval from the Stat of Florida's Office of Fiscal Accountability and Regulatory Reform to amend Chapter 40E-61, F.A.C. Rule amendment efforts will be subject to the directives provided under executive orders and any further requirements from the Office of Fiscal Accountability and Regulatory Reform.

Comment #62: *P.4-1-3 – Of the nine sub-watersheds, four have "intermediate areas within their boundaries where the annual TP load is measured," or "summary basins." Please explain this*

approach more clearly, and why it is used. From the description it would seem that the entire TP loads from these sub-watersheds are not considered - clarification is needed.

Response #62: Summary basin data are supplementary to the sub-watershed level data and presented to further define the source of phosphorus loads in the Lake Okeechobee Watershed.

Comment #63: *P.4-1-5 – In cases such as the Indian Prairie sub-watershed and the Fisheating Creek/Nicodemus Slough sub-watershed, where monitoring stations are moved, add explanation about how differences in the data collected before vs. after the move are interpreted. (The changes also affect the West Lake Okeechobee sub-watershed.)*

Response #63: In the cases where monitoring stations are moved, or boundaries are changed, explanations will be added in Appendix 4-1 regarding how differences in data collection before vs. after the move are interpreted.

Appendix 4-2 – The Two Main ECP Basins

Comment #64: *Tables 3 and 4 – The various locations (inflow and outflow points) should be shown on an accompanying map. Chapter 4 and Appendix 4-1 maps show only some of these.*

Response #64: Text will be added to Appendix 4-2 referring readers to Figure 4-8 in Chapter 4.

Comment #65: *P.4-2-11 – Baseline TP loads have been calculated for only three of the five Lake Okeechobee diversion basins. Explanation would be helpful about the status of baseline TP load calculations for the other two.*

Response #65: Clarifying notes will be added to the table in Appendix 4-2. For the diversion basins that have baselines, historic permit level data was available for the portion of the diversion basin that discharged to the EAA prior to diversion and was extrapolated as a baseline to the entire basin. For the remaining diversion basins, there was no historic discharge to the EAA and therefore baseline data is not available.

Comment #66: *Figures 5 and 7, of flow-weighted mean TP concentrations and TP loads, respectively, in the EAA basin for WY2011, will be very helpful when they are finalized.*

Response #66: WY2011 data is now available and will be added to the Appendix 4-2 tables and figures.

Comment #67: *P.4-2-22, discussion of observed TP load versus the target – Still does not seem sufficiently clear. Also, please remind readers about what the PM is.*

Response #67: See response to comment 29 above. Also, the performance measure for the C-139 basin is detailed in Rule 40E-63 Appendix B2 that states that the entire C-139 Basin is to maintain discharges at or below the collective average annual phosphorus loading based proportionally on the historical rainfall during the baseline period. Clarifying text will be added to Appendix 4-2.

Comment #68: *P.4-2-22, submittal of permit-level data – It would be helpful to add brief explanation here about what is being done to encourage BMP permit holders in the C-139 basin to request the optional farm-level compliance level; otherwise this data gap will continue, despite the District's many efforts to develop strong source control programs.*

Response #68: The 2010 amended rule includes incentives for permit-level monitoring through the option to participate in demonstration projects with measurement of BMP effectiveness and conditions under which permittees may claim that no additional BMPs are practicable, given full BMP implementation and permit basin monitoring demonstrating no increasing trends. Clarifying text will be added to the final appendix. Also to encourage permit holders in the C-139 Basin to implement the optional permit-level monitoring, permit application fees were reduced from

$1,880.00 to $250.00 for new and/or renewal of existing permits including the Permit Basin Discharge Monitoring Program, and permit-level monitoring can be added to existing permits though a letter modification at no additional cost.

Comment #69: *P.4-2-27 – It is very encouraging that the demonstration projects have resulted in long-term improvements that have increased on-site retention/treatment and reduced the TP in runoff.*

Response #69: The authors agree with this comment. No response is requested.

Comment #70: *P.4-2-29 to 30 – The C139D monitoring project (eight automated samplers in an upstream monitoring effort) is very promising and will yield valuable data on water quality (TP, total dissolved P, and soluble reactive P) and flow from sub-basins in the C-139 basin. Please clarify when the additional required efforts will be completed to enable use of the flow data for load and FWM TP concentration calculations.*

Response #70: Data processing activities are continuing and it is expected that the flow and water quality data at the C139D monitoring sites will be used for load and FWM TP concentration calculations in WY 2012. However, the data refinement and identification of areas for improvement will be an ongoing effort.

Appendix 4-3 – The Seven Non-ECP Basins

Comment #71: *This appendix includes data summaries for flow, TP FWM concentrations, and TP loads at eight discharge structures for the seven non-ECP basins during WY1998-WY2011. Historical water quality data for the Acme Basin (which has discharged to the C-51 West canal and is now an ECP basin) is also included. As for Appendices 1 and 2, it would be helpful to add a table which shows the number of samples / sampling frequency per year for each structure.*

Response #71: For reporting consolidation purposes, in lieu of a table please refer to upstream total phosphorus (TP) monitoring maps, Figures 8 to 17, for the number of samples for WY 2011. For the non-ECP "into" structures, please refer to Volume III, Appendix 3-2.

Appendix 4-4 – The Caloosahatchee and St. Lucie River Watersheds

Comment #72: *P.4-4-1 to 2 – As for the Lake Okeechobee watershed, it would be helpful to give readers an approximation of when it is anticipated that the process of developing and adopting PM methodologies will be complete.*

Response #72: Please refer to the "Strategies for Moving Forward" in the "Status of Source Controls in the Northern Everglades Watershed" section, the District is completing a preliminary proposal of performance measures methods to determine the collective performance of source control programs at the sub-watershed level, upon which will be brought for peer and technical review by stakeholders approximately in 2012.

Comment #73: *P.4-4-2 – It would be helpful to remind readers here that the main Plans for these two watersheds are in appendices to Chapter 10.*

Response #73: Please note that the introduction to this appendix indicates that: "The three-year update to the Caloosahatchee and St. Lucie River Watershed Protection Plans, Construction Projects, and Research and Water Quality Monitoring Program is provided in Appendices 10-1 and 10-2 of this volume, respectively."

Comment #74: *Figures 1 and 7 – some of the labels are very difficult to read and should be enlarged.*

Response #74: These figures will be revised to ensure readability.

Posted: 10/20/11 at 02:09 PM by W. Baker (on FDEP comments)

Comment #1: *Table 4-2 under FDEP replace "Domestic Wastewater Residual" with "New Biosolids Rule Chapter 62-640"*

Response #1: The table will be revised.

Comment #2: *Table 4-3 #8 replace "Domestic Wastewater Residual" with "Biosolids"*

Response #2: The table will be revised.

Comment #3: *Line 267. Replace FDEP manure application… with FDACS manure application*

Response #3: The text will be revised.

Comment #4: *Line 356. This legal language may no longer be appropriate due to a recent ruling*

Response #4: This text and all other references to OFARR will be revised based upon input from SFWMD Office of Counsel.

Comment #5: *Line 376. Replace "possible" with "practicable"*

Response #5: The text will be revised.

Comment #6: *Line 409. Replace "were" with "are" (we still are granted authority)*

Response #6: The text will be revised.

Comment #7: *Line 581. Replace "district" with "FDEP" FDEP is tasked with writing the monitoring plan.*

Response #7: The text will be revised to reflect Section 373.4595, F.S. and to read “FDEP or the District”.

Comment #8: *Line 804. Replace "conditions of a TMDL" with "conditions of a Basin Management Action Plan (BMAP)"*

Response #8: The text will be revised.

Comment #9: *Line 807. Replace "TMDL" with "BMAP"*

Response #9: The text will be revised.

Comment #10: *Line 812. ...submit to the Florida Department of Health (FDOH)…"*

Response #10: The acronym FDOH is defined earlier in the chapter (line 385).

Comment #11: *Table 4-1. 0091 Mantee Packet…ERP… the letter P is missing*

Response #11: The text will be revised.

Comment #12: *Line 1067. The map depicts a huge EAA reservoir, it appears to include FL crystals land, is this accurate?*

Response #12: The outline of the EAA reservoir shown on the map is based upon the SFWMD ArcHydro Database. The partially constructed reservoir footprint can be verified on Google Earth.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 5

Delia Ivanoff and Hongjun Chen

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

Posted: 10/07/11 at 03:53 PM by D. Ivanoff

Comment #1: Broad questions and comments. *The considerably more streamlined chapter in this year's report has removed much of the information that might be considered extraneous from the perspective of a technical reviewer, such as detailed reporting of water quality parameters (DO etc.) that are not directly relevant to the primary mission of the STA, namely phosphorous removal. Assuming this information is available to the regulators concerned with such issues (perhaps in Volume III Appendix 3-1, which is not familiar to the panel reviewers), this could be considered a positive improvement to the chapter. However information that is directly related to the primary mission and/or should be available for scientific scrutiny has also been eliminated. For example, the performance section states that STA1E and STA5 are out of compliance for the interim effluent limits; these limits are not, in general, provided. Additionally, previous reports included graphs of long-term trends that allowed an easy visual to spot anomalies and ascertain variations relative to effluent limits.*

Response #1: All SFER authors were asked by their agency to continue streamlining and focusing the content of the chapters. While this effort has resulted in substantial reductions in report size and required funding, it may have come at a cost regarding the STA performance and optimization reporting. For Chapter 5, significant strides were made in both the 2010 and 2011 SFERs to move material into Appendices and this approach continued in the 2012 SFER with the use of Volume III to publish data relevant to permit-required reporting. This includes graphs showing long-term trends for the STAs. We regret that Volume III was not available during the review process. Although Volume III would not have satisfied all of the Panel's concerns, it would have provided some additional information important to the review process. In the future, the District will make Volume III and all Appendices available during the Panel review process. As will be detailed below, we will make multiple changes to specific parts of the chapter in an initial effort to provide more 'information that is directly related to the primary mission and/or should be available for scientific scrutiny' as requested by the Panel.

Comment #2: Broad questions and comments. *Previous reports also provided at least updates on research in progress. This year, not one research project is even mentioned (unless one considers mandated and periodic sampling as research). Was none conducted or just none made available for review?*

Response #2: In an effort to streamline the SFER preparation efforts for this chapter, it was decided to include only findings from research projects that have been completed within the water year to avoid redundancy across years. For the huge constructed wetlands with limited operational controls, the District must also focus primarily on empirical studies and analyses, and has very limited actual controlled research efforts in progress. A description of the empirical studies was blended within the Chapter, e.g. STA-3/4 PSTA Implementation Project, water quality transects (Lines 215-362), and monitoring the status and effects of vegetation enhancements (Lines 997-1059). Peer-reviewed publications were also completed recently, including. Chen, et al, 2010; Chen, et al, 2011; and Juston and DeBusk, 2010, to report on some

STA research findings. During revision, we will be including a discussion of these and include a description of on-going studies.

Comment #3: *As usual, this chapter and the associated appendices present a rather impressive data set for water quality, avian nesting, vegetation changes and the quantity and speciation of soil phosphorus. The effort required to collect and summarize all this information in a comprehensible format should be appreciated by all. However, there is little to no attempt to analyze these data or to make interpretations as to how they will be used for management decisions. The second question above asks whether there are other interpretations of the data, but the panel is hard pressed to find any interpretation of the data, reducing the document to data reporting rather that scientific analysis of the data. Therefore, it is almost impossible to evaluate this chapter on a technical basis unless the panel formulates their own interpretations. Is it the District's intention to declare research on the STAs complete and to convert the intent of this chapter's review to one of Accountability? Considering that 2 of 6 STAs are out of compliance this year, two new areas are about to be brought on line, and correlations between factors which influence performance are still elusive making management decisions (other than water level management which is necessarily in response to variation in hydrology) difficult at best, the panel believes that STA management is still very much in a Technical phase.*

Response #3: As stated earlier, it is not the District's intent to declare research on the STAs complete nor to view STA management as out of the Technical Phase. The District's FY2011 research and STA optimization budget was over $7 million and while mostly not experimental in nature, this effort will provide data that can be used for better understanding of performance and for operational refinements. The focus of this work was applied research including numerous ongoing data collection and data analysis efforts inside the STAs as well as multi-year mesocosm studies focused on improving STA treatment performance. In the interest of streamlining the chapter, only those studies with results this year were included in this year's chapter. It is important to note that the District continues to operate, monitor and optimize its full-scale PSTA demonstration project located in STA-3/4. The 100-acre PSTA Cell has been operated for the past four years and has routinely achieved outflow TP concentrations between 8 and 12 ppb (Table 5-2). The District is in the process of implementing some structural and operational improvements in the PSTA cell to obtain improved operational and performance data before determining the feasibility of further large-scale PSTA implementation. Results of the on-going PSTA demonstration project will continue to be provided in future reports

It is also important to note that the ongoing data collection and data evaluation is a form of empirical studies which we consider at this phase to be the more pragmatic and realistic aspect of STA optimization. We agree that while the chapter contains a rather impressive data set for water quality, vegetation changes and soil phosphorus, the chapter lacked sufficient data interpretation. We will be including further interpretation of the presented data in the revised chapter as time permits and will provide more detailed information in later reports. Over the next few months, we will continue evaluating the available data sets and additional data interpretation will follow in next year's report. Realistic expectations are important in all of these efforts; however the data variability seen in the scatter plots in Figure 5-11, for example, reflects reality in the large constructed wetlands being operated within a water management system.

Also, there have been separate internal reports (e.g., comprehensive analysis by UF-WBL, 2009), technical publications, and peer-reviewed journal publications that synthesized some of the historically gathered information, and efforts are continuing to closely look at these data to keep learning about how the different STAs function in terms of phosphorus removal and treatment sustainability. This involves both short-term and long-term data analysis, and appropriate findings are considered in short-term and long-term STA management decisions. For example, knowing that inflow load directly influences outflow concentration, we have incorporated a weekly review

of the phosphorus loading information by flow-way and routinely consider that information in STA operational discussions. A second example is that knowing that the species of phosphorus that remains at the end of the STA treatment flow-way is primarily dissolved organic and particulate P (transect data presented on pages 5-9 to 5-19), we are now focusing our research effort in finding ways to further reduce those phosphorus forms in the water column. An example of this is a mesocosm-scale study that is underway, in which we are evaluating different species of plants suitable for downstream cells that could help further lower total phosphorus at the outflow locations, including finding ways to convert residual organic P into soluble forms, which could then me assimilated through microbial consumption.

Comment #4: *As mentioned in previous reviews it would probably be much easier to synthesize and interpret results if they were presented on an STA and flow/cell basis as a first level and by the parameter of interest as a second level. Data in this chapter is consistently reported with these levels reversed. To draw a conclusion for performance of a specific cell, one must flip between water quality, soil, vegetation, water level and other possible controls in various sections of the document. Granted data is collected and initially organized by parameter type, but analysis requires integrating all these data to determine controls for a specific STA, flow way or cell. But this issue goes far beyond data reporting. We fear that meaningful analysis leading to better management has remained elusive, in part (recognizing that controls are multiple and interconnected), because the District remains focused on data reporting and has not adopted a mindset of effective data interpretation.*

Response #4: We will make an attempt to re-organize the chapter to present the key information on an STA basis within the time constraints for chapter revision. However, some information may have to remain in their current sections and sub-sections to avoid redundancy, facilitate ease of locating information, and economize on the chapter length.

Specific comments questions by line number

Line #127-131: *This text states that effluent P from STA1E was 22 ppb but data in Table 5-1 states that a FWM was somewhat greater than 45 ppb. These would seem to be incompatible statements.*

Response: Line 127-131 pertains to the annual flow-weighted STA effluent P concentration, while Table 5-1 pertains to individual flow-way effluent P concentration. The differences between STA outflow and flow-way outflow TP concentration could be affected by various factors, such as seepage, particulate settling, mixing of flow-way outflow with standing canal water, and the different level of accuracy in flow data between larger outflow structures and smaller flow-way structures. We will clarify this in the revised chapter.

Line #139: *It is curious that the PSTA project is being decommissioned when previous reports indicated that effluent form these experimental cells was superior to cells planted with EAV and even SAV. What will happen to cell 2 after the project is decommissioned? What about the District's PSTA cell in STA 3-4.*

Response: The US Army Corps of Engineers is currently in charge of the STA-1E PSTA project and made the decision that the project was not providing enough useful information to justify its continuation in light of costs and operational constraints associated with the project. The Corps is currently evaluating options on the design and construction for this area. The District's STA-3/4 PSTA project is continuing, and the District is currently enhancing its scientific evaluation for this project.

Line #164-362: *This entire section begs the question: What will be done with all this data that was collected? Will next year's report provide a more comprehensive analysis of this data? That*

seems unlikely even though the report makes statements in several locations that sic, "this requires further analysis".

Response: Please see also responses under Comments 5-3, Comments to Lines 363-383, and Summary and Recommendations. We will include further discussion and interpretation on the results at many locations throughout the chapter. As mentioned earlier within this response document, further analysis will continue, including parsing the data at logical levels and categories, in order for us to better interpret the vast amount of information collected over the years for the STAs.

Line #363-383: *Clearly any correlation between the reported variables is extremely weak. Considering covariates (or separation by various treatments such as vegetation type) might produce more meaningful results. Hopefully this is not the best that can be done to predict performance after 10+ years of operational data collection.*

Response: Please see also the responses to Comments 5-1, 5-3, and Summary and Discussion. The authors are currently re-visiting analyses presented in Figures 5-11 and corresponding discussion. Previous attempts to analyze data comprehensively proved difficult due to the complexity of the STAs. As mentioned in other places within this response document, the District is re-focusing its efforts in analyzing the large amount of information and parsing them into more meaningful categories to yield information that could aid in better understanding of the STA treatment mechanisms and performance, and allow us to use those as basis for better management and operation of the STAs.

Line #492-506: *The nature of the environmentally sensitive areas of Compartment C is never mentioned. What needs to be preserved?*

Response: These are Native American cultural resource areas that need to be preserved accordingly. Due to the sensitivity of this issue, we are not privileged to provide any further information.

Line #521-550: *Is it possible to construct small elevated islands within the appropriate STA as nesting sites for avian wildlife to minimize the impact of birds on operational objectives to manage P removal. Would snail kites use these preferentially? They appear to preferentially use the berms from the data supplied in the appendix.*

Response: Our understanding of Everglade snail kite biology is that they preferentially nest directly over water, likely the reason why we have never seen them nest in willows on STA levees. Based on this, it is not likely that building islands that rise above the water-line would give the snail kites safe nesting locations. Having raised areas below the water-line would still be problematic to STA operations because the US Fish and Wildlife Service would still require maintenance of at least a foot of water below the nests to protect the nests from predation. Raised areas would likely encourage more nesting for ground-nesting migratory birds (such as black-necked stilts and killdeer). It does not ensure that stilts would not nest on both the islands as well as the drier portions the STAs. Having multiple or large raised islands can also be problematic in terms of short-circuiting of flow within the affected areas.

Line #627: *Why is the organic P determined from the difference between two extractable P fractions? The data in the associated figures (5-17 & 5-18) provide values for both extractable fractions and Po. Seems like this is double accounting, even though the Po fraction is very small. Any information about the forms of the residual P would be appreciated.*

Response: There was no double accounting of any of the fractions. The Po value presented in Figures 5-17 and 5-18 were from subtraction of (NaOH) extracted inorganic P from (NaOH)

extracted total P. Neither of the NaOH- extracted TP nor NaOH-extracted Pi was included in the graphs (Figures 5-17 and 5-18).

Figure and table comments

Fig 5-3: *Text is too small to read. As another example of the lack of appropriate analysis, the significant differences between cells 5 and 7 are never explored. Both are EAV, of similar size yet performance is markedly different. Looking at the vegetation data in Table 5-12, one finds that the better performing cell has less EAV. While in this case the comparison may be counterintuitive, these are the type of correlations which must be (and are not) made to effectively improve management decisions. (In this case a logical question would be why did a better vegetated cell perform less well if vegetative cover is considered a positive? What else is going on?*

Response: Text size for Figure 5-3 will be increased for better readability. The discussion of the results presented will be enhanced, with the inclusion of further interpretation about the cell condition and performance.

Figs 5-7, 8, 10: *Comparing concentrations down a gradient of a wetland incorporates what is invariably variations of inflow parameters as well as potential spatial variation of performance. The panel can only assume that these data were collected to make a determination of spatial variation, but a) this is never mentioned b) considering the variability of the inflow volume and perhaps TP concentration, probably cannot be made independently from this variation. However, while ideal plug flow is clearly not achieved in any cell, use of basic reactor hydraulic modeling e.g. the intermediate hydraulic residence times of the transects relative to the total HRT and the inflow parameter history would help tease out some of the inflow controls on these data. The (positive) fact that data are averages of transect sample locations helps to minimize the short-circuiting issues associated with this approach.*

Response: The data were collected to determine the treatment pattern along the flow path during various flow scenarios, and to determine which form of phosphorus is prevalent at the key points along the flow path. For example, in Figure 5-7, the top graph (12/17/2009), which was from data collected during a low flow event, show higher level P (less effective net treatment) than the bottom graph (higher flow event, 8/13/2010). We will include clearer discussion and further interpretation on these results in the revised chapter. These data are used in STA performance evaluations. A more comprehensive analysis that includes this and related data is underway, however, due to time and page constraints, that detailed analysis will not be in the 2012 SFER. Some of the data have also been published in peer-reviewed journal, such as the one by Juston and Debusk, 2010.

Fig 5-11: *Given the very weak correlation, why was the data log transformed to make a regression equation? A linear fit would produce an equally poor fit but at least be easier to interpret.*

Response: These data analyses are being re-visited. The authors are currently evaluating different options in presenting and analyzing available data. Note that there was also an error made for Figure 5-11a (inflow data was used instead of outflow data).

Table 5-2: *Looking at the cell to cell variation and exceptions to full-time operation noted in the footnotes, one cannot be impressed by the difficulties the District (or for that matter, this review panel) faces when trying make meaningful interpretations from the data collected. Comparing the accumulated knowledge base to all the time and effort, hence money, that goes into data collection in any one year across all the STAs and multiplying that by the number of years this has occurred, one must be disappointed. This is not a negative reflection on the effort or even the*

overall approach considering the inherent variability of the STA system, nor is it a condemnation of what has been learned which is impressive, but it does seem that more should be known for the effort expended.

Response: Table 5-2 specifically aims to compare the performance of Periphyton Stormwater Treatment Area (PSTA) with submerged aquatic vegetation (SAV) cells. The District recognized the insufficiency of the amount and quality of data collected thus far for the PSTA cell, and is now making amends by modifying the structures to obtain better quality flow data and strengthening on its scientific evaluation of the PSTA treatment technology. We anticipate that, provided we get good amounts of flow in the next two years, that we will have a more meaningful evaluation soon after.

Fig 5-13 & 5-15*. Text is too small to be legible.*

Response: Text size will be adjusted in the revised chapter.

Table 5-6: *What does NE mean? Please put in a footnote or in the caption*

Response: NE means "not established." This pertains to the stages not being maintained specifically for bird nesting, since those dates were before or after nesting was reported present. The revised chapter will include this clarification.

Table 5-7: *How is floc and soil physically separated?*

Response: Intact cores are collected using a stainless steel corer. Floc is physically poured into a graduated butyrate tube and depth is recorded. Remaining soil is then extruded into a separate butyrate tube, and then sectioned further to appropriate sampling depths. The revised chapter will include this explanation.

Fig 5-17 & 5-18. *Residue should probably be Residual*

Response: Edits will be applied in the revised chapter.

Editorial comments by line number

Line #231; *change have to to must*

Line #250: *contributing to slightly enhanced Cell 1 performance*

Line #545: *change depth to stage*

Line #696: *intervals*

Line #906*: discharge occurred ?to or? from any*

Response (for editorial comments): Edits will be applied, as appropriate, in the revised chapter.

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.

Response: In addition to earlier responses, we here seek to correct a general misimpression by the Panel that the District is not committed to both research and in-depth data analysis for STA management. During the period from 1994 to 2003, the District and its partners supported an extensive experimental research program to test concepts in advanced treatment technologies and optimization of STA performance (Jorge et al. 2002 and references cited therein). This $30 million effort framed the quantitative basis by which STAs are managed today and guided the development of a comprehensive plan to further improve STA performance known as the Long-Term Plan, briefly discussed at the end of Chapter 5. Research and data evaluations are a huge component of the Long-Term Plan which also lays out a process by which the best available information is used to develop and expeditiously implement incremental improvement measures consistent with informed and prudent expenditure of public and private funds. A key aspect of the Long-Term Plan revision process is the public involvement component which allows stakeholder input throughout. The application of this in-depth, open and iterative planning process to STA design and operation underlies the recent improvements in the performance of all of the STAs, in particular, STA-3/4 and STA-2, both achieving long-term discharge concentrations below 20 ppb. However, this past effort must not be interpreted to mean that research and monitoring data interpretation are no longer a District priority. Adaptive management using an on-going stream of research and monitoring information is a vital part of STA management; the STAs will be in an empirical technical phase for the foreseeable future.

We recognize the need to focus our effort on data collection and further data synthesis, hence, moving forward, the goal will be to further analyze and interpret the comprehensive database of operational, flow, water quality, soil, topographic and vegetation data.

Literature Cited

Chen, H., Zamorano, M.F.., and D. Ivanoff. 2010. Effect of flooding depth on growth, biomass, photosynthesis, and chlorophyll fluorescence of Typha domingensis *Wetlands,* Jan. (2010): 9p.

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Jorge et al. 2002. Chapter 4: Stormwater Treatment Areas and Advanced Treatment Technologies. Page 4-1 to 4-33 in: SFWMD. 2002 Everglades Consolidated Report. South Florida Water Management District, West Palm Beach, FL.

Juston, J. M., and T. A. DeBusk. 2011. Evidence and implications of the background phosphorus concentration of submerged aquatic vegetation wetlands in Stormwater Treatment Areas for Everglades restoration, Water Resources Research, 47, W01511, doi:10.1029/2010WR009294.

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RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 6

Fred Sklar and Chapter Coauthors

Level of Panel Review: Technical
Reviewers: J. Burkholder (AA) and P. Dillon (A)

Posted: 10/07/11 at 12:37 PM by T. Dreschel

Draft Chapter 6 emphasizes key findings in Everglades research and evaluation within five subject areas – hydrology, wildlife ecology, plant ecology, ecosystem ecology, and landscape. A new, surprising addition to this year's chapter is the inclusion of the hydrology and ecology of Florida Bay.

Comment #1: *While the Summary is nicely done, it would be helpful to include explanation about why Florida Bay was moved to Chapter 6, when it is part of the Coastal Ecosystems and has previously been included in that chapter (in the 2012 SFER, Chapter 10).*

Response #1: The following sentence will be added to the Summary: Fl Bay science has been transferred from Chapter 10 to the Everglades chapter to develop a more regional understanding of upstream impacts on Fl Bay and facilitate a more watershed overview and synthesis.

As in previous SFERs, Table 6-1 presents a helpful summary of the major highlights in progress during WY2011 in relation to operational mandates. The various experiments include helpful explanation about the relevance of the findings to water management. The supporting statistics for the experiments are clearly described, and statistically significant results are emphasized. Overall, the draft chapter is ***excellent*** *in technical caliber, and even better written than last year's chapter. It is also easier to follow the linkages between the various projects and sub-projects; the results of a given study are clearly related to the others. For each main section, the writing below summarizes the important contributions and then indicates questions and comments.*

Comment #2: *Please note, as a general comment, that the Panel recommends that the units used in Chapter 6 should follow the Metric system; but if the authors feel that it is more helpful to readers to use English units (e.g. line 328), the chapter should include metric units in parentheses after English units.*

Response #2: It will be done.

Hydrology

This outstanding section is technically sound, nicely written, and accompanied (as in previous SFERs) by excellent graphics. Figures 6-1 to 6-7 follow the same, extremely helpful design as in previous years, including color-coded arrows to assist readers in determining (red) poor conditions from recession rates that are too fast; (yellow) fair conditions due to poor foraging depths, slow recession rates, or rapid recessions; and (green) good conditions. Deviating from previous SFERs, the authors included three water years rather than two, and in so doing nicely highlighted the return of a second major drought in only three years. Drought year WY2009 was a great year for many species of wading birds, whereas abnormally wet WY2010 was a bad year for them. WY2011, similar to WY2009 hydrologically, was not a good year for wading bird nestlings. Among the various areas in the EPA that were described, it was especially interesting

to note that the extreme effects of the drought on WCA-3A resulted in no nesting at the Alley North colony, which is usually a major nesting area. Birds moved to another local colony instead, but nest numbers were much reduced except for – as good news – a large number of roseate spoonbills (Ajaja ajaja). Another surprise (p.6-14) was the "marked disconnect" between hydrologic conditions in the southern versus the northern EPA, wherein Florida Bay and the mangrove transition area apparently did not sustain drought effects until midsummer and salinities remained near-average. Tributaries to Florida Bay had nearly average flow as well. Salinity assessment bay-wide was assessed using high-speed synoptic mapping technology. The data indicate that there can be a months-long lag between seasonal rains and decreased salinity or "freshening" of Florida Bay. WY2011 was a drought year like WY2009, but water levels were normal or higher. Although this seems counter-intuitive at first, the explanations are very helpful – a lag time following the high precipitation of 2010 and drought resistance of peat in terms of water storage are reasonable causes of this observation. Perhaps this could be viewed analogously to residence time: a lag in water level response indicates a long enough water residence time to accommodate annual variations in input (precipitation).

Comment #3: *Line 134, "the often meaninglessness of arithmetic means" – excellent point here!*

Would it be possible to map salinity bay-wide more frequently than once per quarter? It would seem advantageous to have monthly bay-wide assessments to assist in interpreting various changes in plant and wildlife ecology in Florida Bay.

Response #3: It would be desirable but due to budget constraints the previous higher frequency sampling effort has had to be cut back.

Comment #4: *Water levels by the end of WY2011 were below the tolerance level for peat conservation, and in most of the areas were at their lowest in three years. This seems very important, but not much is made of the observation. The Panel suggests that the authors add a sentence indicating that long-term climate change will surely affect the peat.*

Response #4: The following will be added: Water year 2012 is expected to be another drought year due to La Nina conditions. If it is, then the District will need to evaluate the possible connection of droughts to climate change and the serious impacts it could have on peat fires, oxidation and conservation in the Everglades.

Comment #5: *There is great emphasis on the linkages between hydrology and wading birds, understandably, but it would also be helpful to include some brief explanation about the role of hydrology in the life histories of other important classes of organisms.*

Response #5: The following will be added: The emphasis on wading birds in these hydrographs highlights their importance in terms of government efforts to restore the Everglades. Other organisms strongly impacted by hydrology include crayfish, fish, deer, rodents, and reptiles, as well as, the productivity and competitive interactions of all plants and microbial communities. However, their relationships to hydrology has not been as well defined as those for wading birds.

Wildlife Ecology (3 projects highlighted)

Wading bird nesting was not successful for most species during WY2011; readers are referred to Appendix 6-1, which unfortunately is not available for Panel review. The extensive, prolonged drought conditions played "an important but poorly understood role" in nesting success, which was low despite excellent foraging habitat, lack of hydrologic reversals, and smooth/continuous recession rates.

Work toward resolving relationships between hydropattern and prey production – Crayfish are important food for white ibis as well as large-bodied fish. WY2011 was the second year of a two-year experiment at the Loxahatchee Impoundment Landscape Assessment (LILA) facility, which

assessed effects of drought on crayfish; large-bodied fish and wading birds were also tracked. It had been noted that wading bird "super colonies" develop 1-2 years after strong regional droughts, but the underlying mechanisms are not known. The crayfish density study was well done and yielded interesting results with important practical applications. Crayfish abundance increased in cells with drought and fish reduction, followed by rehydration and better bird feeding success. The data support the hypothesis that drought-related reductions of predatory fishes allow higher crayfish recruitment and higher abundance in the following year, in turn providing better foraging for birds such as white ibis that feed on crayfish.

Comment #6: *Lines 41-48 – In the Summary information about this section, the authors hypothesize that cues delivered in past years may control spoonbill populations. Monitoring efforts indicated that fish in mangrove habitats are susceptible to cold snaps and hydrologic patterns from previous water years: In WY2010, few fish were available to wading birds because of cold and high water; in WY2011, despite rapid rebound of fish, many spoonbills left the region. Are efforts planned to test this interesting hypothesis, considering that spoonbills are an important indicator of restoration?*

Response #6: Unfortunately, no efforts are funded or planned to test this hypothesis.

Comment #7: *Lines 424-426 – Add clarification that the data from the experiment suggest that smaller fish are not being enhanced, as well as crayfish, after large-bodied (predatory) fish reductions.*

Response #7: This section is the brief introduction to the study and includes relevant information from the literature about relationships among large predatory and small fish populations. The authors did provide evidence that densities of small fish populations were not affected by the reduction in predatory fishes but these were stated in the Results/Discussion section (lines 482-485), as well as graphically (Fig. 6-16A – no change in dried vs. control densities from the before to the after period).

Comment #8: *P.6-20 – Brief description should be added of the macrocosms and outlines the experimental design.*

Response #8: Studies conducted at LILA are regularly featured in the SFER and descriptions of the macrocosm, etc., have been described previously. Due to the need for brevity, the authors have not included the requested description will included an appropriate reference. Further details of the experimental design will be added to the text.

Comment #9: *Lines 428-443 – To assess baseline conditions prior to imposing drought in two of the four macrocosms, traps for smaller fish and crayfish were placed in both deep and shallow slough habitats, but there were nearly three-fold more deep sloughs than shallow sloughs (n=11 and n=4, respectively). The authors should comment on how this difference was handled statistically to avoid bias.*

Response #9: The sample sizes reported here are not the total number of deep and shallow sloughs (there is only one of each slough type per macrocosm); instead they are the number of throw trap samples per slough (i.e., n=4 in the shallow slough and n=11 in the deep slough), per macrocosm. The large difference in sample size between the two sloughs reflects the three fold greater spatial area of the deep slough relative to the shallow slough. We have attempted to clarify the text accordingly.

It should also be noted that our primary interest was the response of crayfish/small fishes at the level of the macrocosm, not the spatial variation between the two sloughs within a macrocosm. Thus within-macrocosm spatial variation was not modeled explicitly, and as such the sample size differences do not incur bias. We did include a term for slough type (deep vs. shallow) in the

model, but this was simply to account for potential differences in density between slough types - crayfish densities are generally thought to be higher in the shallower and more densely-vegetated shallow sloughs. This expectation was largely correct in the analyses (in three of the four macrocosms the shallow sloughs consistently have higher crayfish densities); the only reason that wasn't true in the fourth macrocosm was because both sloughs had low densities throughout the time series. However it is also important to remember that the experimental design/analyses are focused on changes through time (before vs. after) in impacted (dried) vs. control macrocosms and that each macrocosm was sampled equally at each sampling event (no macrocosm or treatment was biased in time by spatial positioning of traps). We felt it was important to include both habitat types so that our conclusions could be applied to entire wetlands (i.e., whole populations and communities of prey).

Comment #10: *Lines 424-426 – Briefly explain the evidence that smaller fish are not being enhanced, as well as crayfish, after large predatory fish reductions.*

Response #10: See response #7 above (The authors did provide evidence that densities of small fish populations were not affected by the reduction in predatory fishes but these were stated in the Results/Discussion section (lines 482-485), as well as graphically (Fig. 6-16A – no change in dried vs. control densities from the before to the after period).

Comment #11: *Line 446 – The authors' definition of "large-bodied" fish (5 cm length or greater, standard length) seems questionable. They state in the Summary and in this section that large-bodied fish compete with wading birds for crayfish prey; can a 5-cm-length fish consume adult crayfish? Would juvenile crayfish be their prey? Additional clarification/explanation is needed.*

Response #11: Predatory fishes > 5 cm length are thought to consume young-of-the-year crayfish thereby cutting off recruitment of adult crayfish (i.e., bird prey). We have provided additional details to the text.

Comment #12: *Lines 471-477 – Please explain why large-bodied fish were "somewhat more abundant" (actually, according to Figure 6-15, significantly more abundant) in the drought treatments after ~eight months.*

Response #12: The significant difference in predatory fish CPUE in the drought treatments vs controls in March 2011 was primarily a function of the relatively high CPUE in Macrocosm 3. The drought treatment and fish removal efforts were less effective in M3 than in M4 so it is probable there were more large fish present in M3 than M4 shortly after the drought in July 2010. However, it is unclear at this point why the CPUE in M3 subsequently increased to a level greater than in other macrocosms.

Comment #13: *Lines 478-481 – Please add brief explanation/thoughts about why the two "dried" macrocosms were markedly different in predatory fish abundance. It would be helpful to point out that despite this difference, the general response of large-bodied fishes in the two "dried" macrocosms was similar, according to Fig. 6-15.*

Response #13: Each macrocosm has a deep distribution pond where water is added through a gated culvert from a header cell, and some smaller deepwater (> 2 m deep in summer) habitats scattered around the perimeters. These deepwater habitats could not be dried completely and supported large populations of predatory fishes through the driest part of experimental drydown. Having no natural analogues in the historical Everglades, nor an analog in the remaining contiguous ridge-slough landscape we were forced to neutralize the refuge function of these habitats by removing fish manually in order to study the effects of drought-induced predatory fish reduction. We were more successful at removing fish from M4 than from M3 during this removal process. We will provide additional details in the Methods and Results sections related to this procedure.

Comment #14: *Lines 514-515 – The second major implication asserted from the findings in this experiment is that "large-bodied fishes appear to compete with wading birds for food resources." However, this is a well-known phenomenon in general, and is a "given" as stated in the Summary (line 36). Therefore, this second point should be omitted. The hypothesis that periodic droughts are natural disturbance events in the Everglades that are critical for white ibis super colony formation merits more emphasis. The Panel strongly encourages the District to examine this hypothesis more fully because of its important implications for wading bird populations.*

Response #14: The authors do not agree that competition among large fishes and wading birds for food resources is a well-known phenomenon in general. The strength of predator-prey interactions and predator control of prey populations and community structure varies in space and time, as well as among communities. While it is known that large fishes and wading birds can consume similar aquatic prey species, we know of no published evidence that links the population dynamics of large fishes to wading bird foraging or reproductive success. Indeed, while our results suggest a potential link between sunfish populations and those of the crayfish-eating white ibis, they also suggest that sunfish populations will have little effect on those species that primarily consume small fish species (e.g., snowy egrets and wood stork). We have adjusted line 36 to better reflect this view.

The authors thank the review panel for the support for this project. The data presented here are only a small component of the overall results of this study and we are learning a great deal about the interactions among hydrologic disturbances, food web interactions and wading bird foraging responses, with important implications for Everglades restoration and management. We hope to continue this study for another two years.

Comment #15: *Florida Bay salinity transition zone prey base (Audubon of Florida monitoring effort in WY2010, compared to data including WY2011) – The basic finding was that the el Niño effects of WY2010 depressed salinities in the southern Everglades National Park (ENP) and led to increased abundance (and size per individual) of the freshwater oligohaline prey fish community in WY2011.*

Response #15: These data are not yet finalized nor interpreted; we only have status reports from Audubon to say anything about the WY2011 conditions as discussed in other portions of this chapter (& we thought it was important to have some tie-in).

This subsection appeared to be strictly observational. It would be helpful for the authors to clarify the long-term relevance of these observations.

Comment #16: *Figure 6-18 – should indicate significant differences.*

Response #16: While we did not explicitly test this, there would be no significant differences (as tested by a t-test or Mann-Whitney test) between conditions in WY2010 vs each site's respective long-term mean prey fish density or biomass. We will add this statement to the caption.

Comment #17: *Lines 565-567, 572-574 - The authors stated that these observations resemble expected outcomes under restoration scenarios examined for the C-111 Spreader Canal Western Project. However, again (see lines 60-61, lines 76-78, p.6-5 – Table 1 (box 2), lines 336-338; and see lines 874-876, 1179, 1201- 1203 etc.), this project was not clearly explained or defined.*

Response #17: Additional text will be added to clearly explain the goals of this restoration project.

Comment #18: *The C-111 Spreader Canal Western Project was also briefly discussed in lines 584-586 – it is expected that the project will lower salinity, improve habitat, and lead to increased fish production and a return of roseate spoonbills to Florida Bay. Please explain this*

project up front in the Summary section to provide appropriate background for readers (suggest moving lines 1123-1126 up to that section).

Response #18: Additional text explaining the project will added to the summary.

Comment #19: *P.25 – Under "Relevance to Water Management" in this subsection on the prey base of the Florida Bay salinity transition zone, readers "suddenly" are informed about roseate spoonbills. Much of the writing here seems misplaced. The roseatte spoonbill issue likely will be covered well in the appendix to this chapter, but it is not yet available. Regardless, it may be helpful to add one-two sentences referring to the roseate spoonbill issue to the first part of the "Florida Bay salinity transition zone prey base" subsection as background, and to move some of this writing (e.g. 1st paragraph, 2nd to last sentence) to the appendix.*

Response #19: Sections will be reorganized. The WY2011 spoonbill results will be moved to the appendix and we will add text to the first paragraph describing how the Audubon network of hydro/SAV/fish sampling on mainland is used to gauge spoonbill foraging conditions.

Comment #20: Plant Ecology *(3 projects highlighted in the Summary, but 5 projects in the text)*

Response #20: The three Florida Bay SAV projects were combined in the summary and table, this will be clarified.

1) An improved, Selective Herbicide for Cattail – This excellent sub-section has a helpful introductory paragraph to orient readers. The major progress reported was a very promising evaluation of an herbicide, imazamox (ClearcastTM) as highly selective (at a certain application level) in minimizing non-target damage while maximizing cattail removal from phosphorus (P)-enriched areas of the Everglades. The sub-section contains much more experimental detail than other parts of the chapter, because this is a new undertaking. The experimental design is technically excellent, the supporting statistics are nicely explained, and the data are carefully interpreted, including comparison with another published study and possible explanations for differing results regarding non-target pickerelweed (lines 700-709). In addition to "Relevance to Water Management," this sub-section contains some thoughtfully conceived "Conclusions." The authors point out that control of cattail in marginally infested marsh ridge and slough mosaic could slow its rate of invasion, while also acknowledging that control of this noxious plant in nutrient-enriched, hydrologically altered wetlands is (line 740) addressing the symptom rather than the underlying cause – that restoration of an oligotrophic landscape ultimately will be needed. While this study is encouraging, as excellent recommendations the authors advise additional study to assess imazamox sensitivity of additional plant species that are common in this habitat, cattail recolonization rates, and imazamox degradation rates and breakdown products before this herbicide is applied on a large scale.

Comment #21: *The Panel suggests that the authors should clarify that they saw no evidence of toxicity of this herbicide to fauna. Also, it would be helpful to clarify what is known about the mobility of imazamox in water – can it move from site to site once applied?*

Response #21: Concur. While no herbicide-related wildlife impacts were noted, toxicity to fauna was not directly measured in this study. Research in support of EPA registration found that imazamox concentrations must be three orders of magnitude greater than the maximum application rate before toxicity effects were observed in fish (rainbow trout), aquatic invertebrates (Daphnia), waterfowl (mallard duck) and honey bees. This research also confirms that imazamox does not bioaccumulate in aquatic organisms. The half life of imazamox in water ranges from 5 to 15 days with the length dependent upon water clarity, depth, and available sunlight. Dilution and photolytic breakdown are the primary means of dissipation in water. As discussed in the report, aerial applications in this study would result in very low concentrations in the water. These points will be included in more detail in the final draft of the report.

Comment #22: *Lines 682-683 versus Figure 6-19 – the text states that there was a highly significant difference in change in cattail cover between the 0.28 kg/hectare rate and other treatment levels, but the upper left panel does not seem to show that. Please clarify.*

Response #22: Thank you for catching this error. As suggested from data presented in Figure 6-19, the text describing the significant difference is incorrect. The text on lines 682-683 will be corrected to reflect the fact that the Tukey's honest significance test estimates that the control is significantly different from all treatment plots. This result was also observed using a Dunnett's test (done after the draft report was submitted). This section will be corrected accordingly.

Comment #23: *Lines 685-687 versus Figure 6-19 – describe only slight leaf yellowing and spotting; most plants continued to maintain undamaged leaf tissue and exhibited new growth. Yet Figure 6-19 shows an average decrease in cattail percent cover by ~50%. Please clarify.*

Response #23: While the mean cover of Typha was substantially reduced at that treatment rate, we did observe plants with remaining live leaf tissue and new growth emerging from the shoots with little to no herbicide symptoms. The authors will rewrite this sentence to better communicate the point that the plants appeared to have mostly suffered loss of some leaves and appeared to be recovering.

2) Hydrodynamics of Tree Islands in Early Development (LILA study) – The tree island project characterized the effects of tree growth and transpiration on the local groundwater hydrology by monitoring stage levels in wells. This subsection was interesting and well written.

Comment #24: *The methods are clearly explained, except that it would be helpful to include more explanation about the groundwater evapotranspiration method of White (lines 805-806).*

Response #24: We will add another sentence to explain the White method (and the reference).

The White method uses the specific yield sediments, the sum of the change in storage per day and the net recovery rate of the water table between midnight and 4:00 am to estimate the daily groundwater evapotranspiration rate.

Comment #25: *Although the inference from this study is that, as would be expected, the vegetation and underlying geologic conditions greatly influence the hydrologic conditions of tree island, a major "deliverable" from this study is a two-year, greatly detailed, valuable database on groundwater levels, surface water stage, groundwater evapotranspiration, and hydraulic gradients in tree island systems.*

Response #25: Although an effect of vegetation and geology on hydrology of tree islands is expected, what was not anticipated was the establishments of the measurable hydraulic divide on each of the tree islands as the tree biomass doubled, and deeper water table depression on the limestone islands. This leads to an explanation of a major cause, not previously demonstrated, for the high levels of nutrients and other minerals found on tree islands in the southern Everglades compared to those in the northern Everglades. Much appreciated is the reviewer's comment that a two-year detailed, valuable database is provided from this study. We believe that this study has provided important new data on how tree islands develop and function.

Comment #26: *Another interesting finding was the rapid response in the water table with growth of juvenile trees, and the creation of a "hydraulic divide" along the edge of the islands after tree biomass doubled. This hydraulic divide resulting from tree growth helps to prevent accumulation of nutrients – a mechanism explaining how tree islands help to maintain oligotrophy in Everglades marshes.*

Response #26: We agree that the response was rapid but disagree with the statement that the hydrologic divide would prevent the accumulation of nutrients on the islands and will add the following text to clarify our findings:

The presences of the groundwater table depression indicated the tree islands acted as a hydrologic sink, capable of aiding in the concentration of nutrients deposited on the islands over time. Furthermore, the hydrologic divide between the surface water and groundwater at the edges of the islands indicated a lack of surface water inputs, but does not speak to the interaction between the slough groundwater and tree island groundwater.

3) Macroalgal and Seagrasses Monitoring in Florida Bay – These ecosystem components are monitored to gain insights about the effects of water management on wetland and estuarine ecosystems. The information is expected to support future updates of the Florida Bay Minimum Flows and Levels (MFL) rule, and to assess effects of the CERP C-111 Spreader Western Project.

Increased seagrass (submersed aquatic vegetation, SAV) community diversity is an identified "operational and restoration target. The introductory summary states that widgeongrass (Ruppia maritima) and shoalgrass (Halodule wrightii) SAV expanded in nearshore bays and ponds, and in many Florida Bay basins, respectively, during WY2011 (but see comments below about shoalgrass).

***Comment #27:** P.6-4, Table 6-1 – states that increases in macroalgae, as well as seagrasses, "may represent a continuing rebound from a period of dense phytoplankton blooms in 2005-2008…." This writing implies that the authors are pleased to see a major increase in benthic macroalgae. However, high abundance of benthic macroalgae can wreak havoc on beneficial seagrass meadows by shading the underlying SAV so that they die out. This important point should be considered and addressed.*

Response #27: This point is more fully considered in the actual text of the chapter but text of the table will be altered. While some of the algal species present are considered beneficial habitat, a caveat about the potential for harm by increasing macroalgal abundance will be added.

***Comment #28:** Lines 596-597 – The description of this third study under Plant Ecology is inaccurate; both SAV and macroalgae were assessed. Please alter accordingly.*

Response #28: The text will be altered to make this point clear. The word "macroalgae" will be added.

***Comment #29:** Lines 845-856 – Readers are referred to Chapter 12 of the 2011 SFER for further information about these various methods for monitoring seagrass (SAV) abundance. However, the Panel requested last year that information be included about cross-calibration – or not – of these methods. Please add that information here.*

*Moreover, "SAV" is defined as seagrass in the draft Chapter 6. However, there is no mention in the "Methods" about how macroalgae were assessed. Please ensure that SAV here is **not** being used to refer to both SAV and macroalgae, and alter the writing accordingly.*

Response #29: The term SAV will be removed to avoid confusion in the text. The programs undergo an annual intercalibration and brief text will be added to indicate method and results. However, as the same method is used by both monitoring teams and each location is only compared to itself, the intercalibration is not critical to the results on a per station basis.

***Comment #30:** Methods need to be added for macroalgae; or the writing needs to be modified to include both SAV and macroalgae. As the Panel wrote in last year's comments (about Chapter 12 of the 2011 SFER), the panel remains concerned about use of the term submersed [submerged] aquatic vegetation (SAV) to include both macroalgae and seagrasses. Seagrasses, not seagrasses + macroalgae, are a VEC identified by the District in evaluating the success of its upstream restoration efforts. The draft Chapter 12 writing inadvertently included an excellent illustration of the importance of distinguishing between seagrasses (SAV) versus macroalgae: On p.12-101 the authors described an investigation of the Lake Surprise causeway cap (over the footprint of*

the original causeway) "to document the status of SAV recruitment" since completion of an excavation and filling project by FDOT. A two-year duration was to be allowed for SAV to naturally recruit into the cap area in order to determine whether transplanting efforts (of seagrasses, not macroalgae) would be necessary. The authors misstated that "SAV is recruiting well on the cap footprint" because most of the recruitment was described to have been by macroalgae, not seagrasses, and seagrasses are the beneficial species that will need to be transplanted. The authors' description of "at least one species present in 97.3% of observed quadrats," coupled with their observation that macroalgae were more frequent and in higher density than seagrass, indicate the opposite of what they asserted: Seagrasses are not recruiting well and seagrass transplanting will clearly be needed. This writing illustrates the serious problem created by "combining" macroalgae as "beneficial SAV." While some macroalgae can be beneficial, macroalgal overgrowth is a known, common cause of seagrass decline and failure to re-establish in disturbed areas. It is important that the writing of this year's Chapter 12 be altered accordingly."

The authors of draft Chapter 6 aptly note (lines 897-900) that the bay-wide substantial increase in macroalgae may be related to "higher than normal total nitrogen (TN)" or to the wetter-than-normal dry season of WY2010; and that macroalgae should be watched carefully since they can be an indicator of elevated water-column nutrient concentrations.

Lines 897-900 - The TN increase is worrying. In addition to the role of precipitation/runoff, is it possible that warming of the water as part of a climate change signal may be partially responsible? (Such temperature changes in some freshwaters are now being invoked as a reason for increased frequency of algal blooms.)

Response #30: An interesting possibility for further research and modeling.

Comment #31: *Lines 858-866, 878-886 – The species of macroalgae is an important point that needs to be addressed, and their impacts on the (beneficial) seagrasses also need to be addressed. Are these species good for faunal habitat? Do they shade light and stress underlying beneficial seagrasses? The writing should be altered to include this information.*

Response #31: The majority of the species are small individuals with little shade footprint. However, these can be harmful to seagrasses at high abundance and text will be added to reflect this. In the nearshore areas where *Chara* is present with high cover it has been shown to be beneficial as faunal habitat. The metric discussed within the text is "total macroalgal cover" which is scored based upon the bottom occlusion by all macroalgae within the quadrat, regardless of species. Individual species are also scored, but this information was not described in the report due to space limitations.

Comment #32: *Lines 867-876 – The increase in Halodule wrightii (shoalgrass), while encouraging, is very small, from only ~1% in 2005-2009 to only ~11% in 2010. These percentages are to the 10th of a percent; it is doubtful that the precision of these percent cover estimates is that good, so the percentages should be rounded to the nearest integer.*

Response #32: The percentages reported are not percent cover but are the percentage of quadrats that contained at least 25 percent bottom occlusion of shoalgrass. Since there are 240 quadrats per basin, this allows a precision of <0.01 of a percent. The removal of the extra decimal places would not hinder the message and we will do so.

Comment #33: *Figure 6-23 – Is the increase in H. wrightii in Madiera Bay at the expense of Thalassia testudinum (turtlegrass)? Please include clarification/ explanation.*

Response #33: Increases in shoal grass seem not to have negatively impacted turtle grass. The figure shows increased turtle grass frequency and/or density in each of the basins. One of the

goals of restoration is to enhance the species richness of seagrasses in the bay and so the increase in *Halodule* is viewed favorably. We will add text to convey this point.

Comment #34: *Lines 903-905 – please clarify the meaning of this sentence*

*4) Manipulating submer**s**ed aquatic vegetation (widgeongrass) density in Joe Bay (northeastern Florida Bay) – Experiments were conducted directly south of the C-111 canal, wherein SAV density was manipulated by clearing selected plots and measuring the rate of recruitment into these plots.*

Response #34: Text will be altered to: "The recent macroalgal response observed in WY2010 may be related to TN recycled from the earlier phytoplankton bloom." A clearer description of the methodology will be provided.

Comment #35: *Lines 930-939 – the authors have confused macroalgae with macrophytes (= submersed vascular plants): Najas marina is a submersed vascular plant (macrophyte), not a macroalga.*

[Note: this effort was not mentioned in the introductory Summary section, and a sentence or two about it should be added under Plant Ecology]

Response #35: This will be corrected and short text will be added to the introduction.

Comment #36: *5) Submersed Aquatic Vegetation Physiology [suggest changing to Seagrass Physiology] – The underlying rationale for this preliminary study is compelling, as are the data shown in Figure 6-24, but the extremely limited information given does not enable evaluation of technical merit. How were measurements made, on how many plants, and how frequently over what duration? Was there an assessment of variance? This sub-section either should be expanded so that technical merit can be evaluated, or it should be omitted for the 2012 SFER and presented at a later date when more information is available. If the sub-section is retained, then the findings should be briefly mentioned in the introductory Summary under Plant Ecology.*

Response #36: Within the space limitations of the document, brief explanation of the methods and analysis will be provided. In the event that this cannot be accomplished in the space available, this ongoing study will be more fully profiled in next year's SFER when additional data and analysis are available.

Comment #37: Ecosystem Ecology *(2 projects highlighted in the Summary, but 5 projects in the text)*

Response #37: This chapter is at its page limit, but the following summary will be added if it is possible:

This section focuses on two regions; one is the areas of Water Conservation Area-2A in the Everglades that have been severely impacted by nutrient enrichment. Here, results of maintaining openings in dense cattail stands for ecosystem restoration will be presented. In addition, results from the C-111 Spreader Western Project monitoring program in the coastal communities of Florida Bay will be reported. A major CERP restoration project, the C-111 Spreader Western Project, was under construction during WY2011 and is expected to be operational in WY2012. Reporting on this C-111 region includes; 1) Fl Bay water quality, 2) Sediment-water nutrient fluxes in the Central Lakes Region, 3) Phytoplankton surveys, and 4) "White-zone" soil salinity transects.

1) *Update on the Cattail Habitat Improvement Project (CHIP) – This project is technically sound and excellent in caliber. The authors aptly note (lies 982-984) that restoration of P-impacted regions in the Everglades "requires not only a reduction in P loads and concentrations, but also active management to reduce the resilience and resistance inherent to the cattail*

regime." They succinctly, clearly describe this large-scale, impressive project, its main goal and two major objectives, and its overall hypothesis. The report describes assessment of carbon, N, and P storage and cycling, and changes in trophic dynamics from samples taken in WY2010. The analyses indicated that openings in cattail habitat, attained using herbicides (and fire, in other years), supported higher soil nutrients than surrounding cattail habitat, and changed the carbon (C) and nutrient cycling from macrophyte/floc-dominated to periphyton/SAV dominated. This, in turn, supported increased prey (especially fish) biomass within P-enriched and transitionally P-enriched areas. As expected, wading birds also significantly increased (both in biomass and species richness) in open areas. Thus, the hypothesis was confirmed that food quality in the open enriched plots is superior to food quality in adjacent cattail-dominated plots. The study also showed that these created openings restore substantial environmental benefit to areas that would otherwise have very limited ecological value. Consideration of stoichiometric relationships (C:N:P ratios, reflecting trophic origin or functional groups, and food quality) supported the above findings by providing direct evidence of connections between altered nutrient cycling and trophic components of the food web. The authors nicely explained this approach; elemental composition of the consumers is a proxy indicating nutrient "needs" (demand), and the proportional elemental composition of the food represents the "supply." Enriched control plots (cattail) had a "decomposition signature" of low-quality, recalcitrant food resources (e.g. the tough, lignified tissues of cattail; C:N:P > 933:48:1), and energy flux and nutrient transfer to consumers was mostly through omnivorous invertebrates. In contrast, enriched open plots had C:N:P ratios indicating improved food quality from more labile SAV/periphyton production, and energy flux/ nutrient transfer was mostly through herbivorous and omnivorous fish. The improved-quality prey were also accessible to wading birds. Work is planned to conduct these analyses in transitional and unenriched sites for comparison.

Comment #38: *Line 983 – please define resilience.*

Response #38: A definition will be included.

Comment #39: *2) Florida Bay Water Quality – This project was referred to as the C-111 Spreader Western Project in the Summary section, which confusingly described it as a "monitoring program" that was "under construction" during WY2011 (please alter the writing accordingly). The monitoring effort indicated good water quality conditions in WY2011, with no major phytoplankton blooms and low P concentrations. The subsection is well-written, with excellent supporting graphics: that clearly convey the water quality conditions both in WY2011 and in comparison to previous years*

Response #39:. Reference to a monitoring program will be rectified and C111 SCWP will be summarized.

Comment #40: *Lines 1130-1131 – states that the data record began in 1991, but Figure 6-30 shows the data beginning in 1992. Please clarify?*

Response #40: Water quality in Florida Bay and the other southern coastal systems has been monitored since 1991 (WY1992) to ensure that District operations and projects protect and restore the ecosystem to 1132 the extent possible.

Comment #41: *Lines 1161-1170 – it would be helpful to add interpretative comment about why the TP trend is the opposite of the TN trend as mentioned.*

Response #41: Text will be added**:**

Total phosphorus (TP) concentrations in all regions, except the western region, continued the trend of being less than the long-term median and often less than or equal to the 25 percent of the interquartile range for the entire water year (**Figures 6-28** and **6-29**). Interestingly, in nearly all

areas except Barnes Sound, TN had the opposite trend of TP, with TN being greater than the long-term median and often greater than the 75th percent interquartile range for the entire water year (data not shown). The reasons for these trends are unclear, but not unprecedented in the region. Abbott et al. (2005) evaluated long-term (1991–2003) water quality data at 13 sites throughout the Biscayne Bay watershed and found that nitrogen concentrations were generally increasing and TP concentrations were declining over this period. Annual average TP and chlorophyll *a* concentrations in all regions, except the western region, which showed a slight increase in the last two water years, have been declining since about WY2006–WY2007 (**Figure 6-30**), which included the disturbance of three hurricanes in 2005. Long-term patterns of TP and chlorophyll *a* point toward the importance of such disturbances, with peaks following Hurricane Irene in 1999 and the 2005 storms.

New reference to be added: Abbott E, Alleman R, Barnes T, Bauer M, Bennett R, Chamberlain R, et al. Chapter 12: Management and restoration of coastal ecosystems. 2005 South Florida Environmental Report. South Florida Water Management District; 2005.

Comment #42: *Figure 6-30 – the long-term patterns shown are promising, especially the suggested decrease in P. These data would seem to warrant at least preliminary statistical trend analysis*

Response #42: No change made due to space limitations.

Comment #43: *Note: Studies 3-5 below should be mentioned in the introductory Summary section.*

Response #43: Text will be added to introduce these studies in the summary.

Comment #44: *3) Central Lakes Region Sediment-Water Nutrient Fluxes – the lakes region between Seven Palm Lake and West Lake (western boundary of Taylor Slough) is described as a poorly known but critical area for restoration efforts, especially the success of the C-111 Spreader Canal Western Project. This study continued from WY2010 to quantify present (pre-restoration) rates of benthic nutrient and metabolic gas fluxes from the sediments of these lakes, based on the premise that nutrients released and transported downstream could support algal blooms in Florida Bay. The data thus far indicate that the lake sediments are releasing large quantities of ammonium, which is a preferred N source for many algae. Where benthic microalgae were abundant, however, there was strong retention of N. In general, net fluxes of TP and TN were highly variable. The rationale explained by the authors seems sound, but the technical merit of this project unfortunately cannot be evaluated because of insufficient information.*

Response #44: The following text will be added:

CENTRAL LAKES REGION SEDIMENT-WATER NUTRIENT FLUXES

As reported in the 2011 SFER – Volume 1, Chapter 6, the District undertook studies on the dynamics of the western boundary of Taylor Slough (the lakes region between Seven Palm Lake and West Lake), which is a little studied area that will be critical to evaluation of CERP restoration projects, especially the C-111 Spreader Canal Western Project. An objective of the lakes study program is to quantify current (pre-restoration) rates of benthic nutrient and metabolic gas fluxes from sediment cores taken from Seven Palm Lake, Middle Lake or Munroe Lake, Terrapin Bay, West Lake, Long Lake, and Garfield Bight. It is important to understand the water column and sediment processes that will be affected by increased freshwater input to the transition zone because these processes have the potential to release nutrients to the overlying waters where downstream transport to the bay may cause algal blooms. Intact sediment cores and bottom water were collected and incubated in a temperature controlled incubator in the lab under

dark and light conditions for ~ 4 hours each. At 7 time points, 3 in the dark, 1 at the dark/light transition, and 3 in the light, samples were collected for a variety of nutrient and gas samples. Sediment-water exchange rates are calculated from the slope of the change of the chemical constituent concentrations in the overlying water. Results from the first of two studies (Owens and Cornwell, 2011) indicate sediment recycling of inorganic N results in relatively large effluxes of ammonium (**Figure 6-31**). In most cases denitrification was relatively small.

Comment #45: *4) Lakes Phytoplankton Study – This study was not clearly explained, but evidently was conducted to provide baseline information about phytoplankton assemblages and the nutrient conditions that presently characterize selected lakes in the Central Lakes Region. The information is to be used to develop predictions about how the C-111 Spreader Canal Western Project will affect these lakes and how such changes, in turn, will affect downstream Florida Bay. The authors "jump" from a confusing introductory paragraph to "results," wherein they mention various pigments with very little supporting explanation, and some confusing information about the relative abundance of various phytoplankton groups. Evidently, bioassay incubations and nutrient addition experiments were conducted, but readers have no idea how. Thus, the technical merit of this study cannot be evaluated, based on the extremely sparse information given. It is also difficult to make sense of what the authors are trying to convey as major points: Apparently, of potentially noxious groups, (i) the cyanobacteria had highest dominance at upstream sites, but were abundant at most sites near the end of the wet season, especially in waters that were high in dissolved organic N (DON) and dissolved organic P (DOP). (ii) Highest relative abundance of dinoflagellates occurred in waters with low DOP: DIP ratios (dissolved organic P : dissolved inorganic P). The "Relevance to Management" did not clearly convey how/why these findings are important to management.*

Response #45: Within the space limitations of the document, the section will be reorganized and the methodology and background will be added.

Comment #46: *Lines 1245-1248 – states that cyanobacteria are able to outcompete other phytoplankton groups when P availability is comparatively low. What is the basis for this statement (supporting references)?*

Response #46: A reference and brief text will be added.

5) Soil Salinity Transects – Also for baseline information prior to the C-111 Spreader Canal Western Project, north-south transects were established to assess soil salinity dynamics, including in the "white zone" area where decreased freshwater input over decades has led to saltwater encroachment and reduced productivity of inland wetlands. This project should add valuable information to the pre-C-111 Spreader Canal Western Project baseline.

Landscape *(3 projects highlighted)*

Comment #47: *Note: Although three projects are highlighted in Table 6-1, only two are described in the text of the introductory Summary section. This discrepancy should be corrected.*

Response #47: All three projects are described in the introduction but two are designated as science monitoring and analysis projects, the third being the description of the publication of the Predrainage Everglades book.

This exciting section is technically sound, well written, and supported by informative graphics. The diverse topics are nicely integrated. The innovative scientific approaches, supporting rationale, and relevance to management restoration efforts are clearly explained. The main points are summarized as follows; we have no questions or comments that the authors need to address, only some highly positive remarks, except for one comment regarding project #2.

1) Paleoecological and Imagery Analysis of Tree Island Acreage Over the 20th Century – Results are presented from a pilot project which, for the first time, applied high-precision 14C AMS dating to Everglades soils. Paleoecological analysis (macrofossil seeds and other proxies) and aerial imagery analysis were used to quantify vegetation changes and resolve rates of tree island change over the past century as a function of water management versus natural climatic forcing. The macrofossil (seed) data enabled vegetation reconstructions from 1960-1970 (period of construction of the L-67 canal and levee), when aerial photos were not available. As important findings, the timing of increased abundance and aerial expansion of water lily and sawgrass into the island interior in the early 1960s, indicated by the macrofossil record, supported the premise that the initial phase of L-67 construction reduced tree island area by raising water stages and increasing hydroperiods. Moreover, in the dry years of 1989-1990, the flood-tolerant woody species wax myrtle expanded 25 meters into the slough, indicating a positive expansion of tree island area in comparison to the 1940 tree island boundary. It was suggested that such hydrologic conditions could provide operational targets for tree island restoration.

2) Floccometer Transport Studies (toward preserving/restoring the ridge and slough landscape) – this project is using a remarkable new, automated, complex robotic monitoring/research platform to quantify with amazing precision, at macro- and microscales, processes that maintain the Everglades ridge-and-slough landscape. Floc is clearly defined in this sub-section as a layer (thickness < 30 cm) of coarse-grained detritus or gyttja at the bottom of the water column. The new platform includes instruments for met data (meteorological conditions) as well as time series of the precise elevation of the floc-water interface; water-column and floc layer profiles of dissolved oxygen, pH, temperature, light penetration, conductivity water stage/ depth/slope, water velocity, and elevation of the peat surface beneath the floc layer. In addition to the significant contributions that this instrument will make in advancing Everglades science and restoration, the project will contribute to public education/outreach considering that a publicly accessible webcam allows remote monitoring of the floccometer and slough. An enormous amount of valuable data is being collected by this instrument, including ridge-and-slough landscape data, floc thickness, peat elevation, hourly DO and temperature depth profiles, and net community carbon balance in both the water column and the floc layers. This breakthrough technology is highly relevant to water management (lines 1615-1617): It will help guide operations and restoration planning by quantifying the field conditions needed for preservation of sloughs through downstream carbon transport."

Comment #48: *Lines 1520-1537, and Figure 6-37 – Rather than briefly describing the instrument and devoting a figure to where it was placed in the field for this study, the authors should include a detailed diagram of this remarkable, very exciting instrumented platform and its tracking capabilities. Figure 6-37 does not seem to provide much helpful information and could be omitted in the interest of saving space (report streamlining).*

Response #48: We thank the reviewers for their positive comments on this project. Due to lack of funding, this project has been put on indefinite hold. We will replace Figure 6-37 with a simple diagram, as this research platform was still in the developmental stage and there are no detailed diagrams available at this time.

3) Milestone – a new book, "Landscapes and Hydrology of the Predrainage Everglades," by McVoy et al. (2011 – the authors are District scientists) provides, for the first time, a well-documented "base condition" for Everglades restoration (soils, vegetation, geomorphology, and hydrology prior to dredging of the first canals in the 1880s). It quantitatively estimates Predrainage water depths and hydroperiods, maps pre-drainage patterns of Everglades water flow, and documents changes that occurred between the first drainage (1880s) and the first system-wide scientific mapping attempt (1940s). As the authors point out, this reconstruction "provides validation tools for regional models, a yardstick for evaluating current conditions, and

reference conditions to guide restoration." This pioneering work is an exciting, very valuable contribution to Everglades restoration efforts and, more broadly, to the science of the Everglades.

Editorial Changes

Comment #49: *Panel Comment (in italics; copy verbatim from web posting)*

Please change "submerged" to "submersed" aquatic vegetation throughout.

Response #49: This will be done.

Comment #50: *Lines 7-8 – shouldn't the writing say five areas, including hydrology?*

Response #50: This will be done.

Comment #51: *Lines 29-40 – under Wildlife Ecology, this description of the LILA experiment and its purpose are not clear. The authors should add some brief explanation. The writing here also seems to conflict somewhat with p.6-19, lines 389-393: The Summary states that the purpose of the LILA experiment was to test the effects of drought on large-bodied fish; p.6-19 states that the purpose of the experiment was to test the effects of drought on crayfish. Please clarify and alter the writing for consistency.*

Response #51: The text will be amended accordingly

Comment #52: *Line 36 - …on large-bodied (predatory)*

Response #52: The text will be amended accordingly.

Comment #53: *Line 39 – by "measure of control," the authors apparently mean that large-bodied fish would have been eliminated in areas with extreme drought? Please clarify*

Response #53: The text will be amended accordingly

Comment #54: *Lines 91-92 – first mentions the period from 1960-1970, and then the period from 1960-1963. Please clarify.*

Response #54: This will be done.

Comment #55: *Lines 60-61, 76 – mention the CERP C-111 Spreader Western Project but, unlike the CHIP, it is not described. The Summary section should include a brief description of the project to orient readers. As a suggestion, the information from lines 336-338 could be repeated in the Summary section to help readers.*

Response #55: This information will be added.

Comment #56: *Table 6-1 – the Wildlife Ecology section should include monitoring of fish populations in mangroves, to be parallel with the summary information.*

Response #56: The text will be amended accordingly.

Comment #57: *Table 6-1, box 2 – should be: ….reduction of large-bodied fish populations in the early wet season, which would be expected to accompany…*

Response #57: The text will be amended accordingly.

Comment #58: *Table 6-1, box 2 – change to: …in crayfish densities following the drought.*

Response #58: The text will be amended accordingly.

Comment #59: *Table 6-1, box 5 – please improve the description of findings about the tree islands experiment conducted at LILA. The purpose of the experiment should be briefly stated*

(e.g. the objective given on lines 781-782), and the significant findings clarified. (The fact that overlying vegetation and underlying geology play a large role in hydrologic conditions of tree islands and concentration of soil nutrients seems to be a well-known generalization – what is new and exciting from the experiment that advances tree island science?)

Response #59: This will be done.

Comment #60: *Lines 131-132: …below-average precipitation….would also have been below…*

Response #60: This will be done.

Comment #61: *Line 139 - …Both of these…*

Response #61: This will be done.

Comment #62: *Figure 6-8 – the wording in the legend needs to be enlarged as it is not clear unless the page is enlarged to at least 125%.*

Response #62: A more readable legend will be provided

Comment #63: *Lines 413-415 - …All of the evidence for this "pulsed production" of wading birds… [is this what the authors mean? – that is, the pulsed production here refers to wading birds? – otherwise, please provide appropriate clarification in the writing]*

Response #63: The text will be amended accordingly.

Comment #64: *Lines 459-460 - …and their densities were analyzed using…*

Response #64: The text will be amended accordingly.

Comment #65: *Lines 495-497, poorly written – please changed to: …a significant but modest reduction in largebodied (predatory) fishes in the early wet season was achieved through a simulated drought, as also would be expected during natural droughts…*

Response #65: The text will be amended accordingly.

Comment #66: *Line 519 – should change sub-section title to: Prey Base of the Florida Bay Salinity Transition Zone [note: these findings were not mentioned in the Summary, and a sentence or two about them should be added under Plant Ecology]*

Response #66: Title will be changed and text will be provided.

Comment #67: *Line 521 - …at which this organization collects..., performs bimonthly submersed aquatic…, and samples…*

Response #67: This will be done.

Comment #68: *Line 557 - …suggest that numbers…*

Response #68: This will be done.

Comment #69: *Lines 595-596 - …transpiration on the local groundwater hydrology of tree islands by… [is this what the authors mean? – please clarify accordingly]*

Response #69: This will be done.

Comment #70: *Lines 649, 912, 940 – change submerged to submersed*

Response #70: This will be done.

Comment #71: *Line 710 – …were found too infrequently to…*

Response #71: This will be done.

Comment #72: *Lines 805-806 – please add a supporting reference for "the White method."*

Response #72: This will be done.

Comment #73: *Line 826 – islands, which…*

Response #73: This will be done.

Comment #74: *Line 926 – …rate of recruitment into these…*

Response #74: This will be done.

Comment #75: *Lines 912-939 – this subsection needs an ending "Relevance to Water Management" for parallelism with other subsections.*

Response #75: This will be added

Comment #76: *Line 930 - …widgeon grass, another submersed vascular plant – Najas marina, and the microalgal species Chara hornemannii can maintain percent cover when salinities increase to mesohaline conditions…..N. marina and C. hornemannii decline. Widgeon…*

Response #76: This will be done.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 7

LeRoy Rodgers

Level of Panel Review: Accountability
Reviewers: J. Burkholder (AA) and O. Stein (A)

Posted: 10/07/11 at 01:15 PM by L. Rodgers

Comment #1: *Consistency with earlier versions - In comparison to last year's coverage of nonindigenous invasive species, the following five points about general chapter content merit further consideration: (i) Gone from this "streamlined" chapter is the remarkable, compelling historic information about invasive nonindigenous species in this region. (ii) Also absent, even for the eleven identified priority species, are the outstanding indicator-based stoplight tables that were/are so "user-friendly" for public understanding. Retention of both features would not require much space, and would greatly improve the chapter. (iii) Previous versions of this chapter included a photo for each of the priority species, an excellent feature that strengthened readers' interest and understanding. This feature should also be retained. (iv) Last year's chapter covered 24 priority species, including 12 plant taxa and 12 animal taxa. This year's chapter clearly identifies 11 priority plant species and 5 priority animal species; important species from last year's priority list, such as the green mussel, are not on this year's list. Explanation should be included about this major "decrease" in the District's priority species. (v) Unlike versions posted for Panel review in previous years, many paragraphs, and all of Table 7-2, lack final data (instead, a substitute phrase is given, "# available in final version only"), which impedes comprehensive review.*

Response #1: The reviewers correctly point out that several elements of previous annual reports were omitted from this year's report (and we appreciate the reviewer's compliments on those aspects of previous reports). As the reviewers are aware, this year's report is intended to be more

streamlined while meeting reporting needs and legislative mandates. In accordance with this directive, the authors attempted to focus on presenting only status updates where new data was available for the reporting period. As appropriate, background information and species updates where little or no new information is available are is cross-referenced with earlier reports. The authors agree that the one-page synopses containing the indicator stoplights are an excellent communication tool and will certainly consider them for future reports. In many cases, however, the status of some species does not change annually (or the information is unavailable). Therefore, the authors chose to present new information about priority species when it was available and to do so in a more compact format, including reduced numbers of photos. For this reporting period, significant new information was available for only 11 of the 24 priority species. To avoid confusion about the omission of other priority species, the authors will revisit the text on Lines 24-27 and Lines 96-106 that explains the reduction of content in this year's report and clarify that this report only provides updates where new data is available.

As with all previous draft manuscripts, data for Table 7-2 was not included because it is unavailable at the time of report submittal. This data becomes available in October when the final treatment data for the fiscal year is entered into the WEEDDAR database. Unlike previous years, and in accordance with previous reviewer comments, the authors attempted to include more quantitative information about the District's invasive species management programs throughout the chapter. As such, there were numerous sections where placeholders were inserted for this data, which is also unavailable until mid-October.

Comment #2: *Some activities that appear to be new initiatives in Table 7-1 (e.g. use of Asian grass carp on hydrilla) are not addressed subsequently in the chapter (see lines 185-204), despite the fact that the stated objective is to highlight new activities. Please carefully check this table against the chapter text and resolve such discrepancies.*

Response #2: The purpose of Table 7-1 is to compile all invasive species management activities undertaken by the District during the reporting period. These activities include both new and ongoing management activities. As reported in previous years, use of sterile Asian grass carp is an ongoing weed management program for isolated canal systems. Due to space limitations in this volume, we only provide information for programs and/or species for which there are notable updates to report. In the case of weed control with carp, there are no significant updates to report for this year.

Comment #3: *Species are divided into priority and (not designated, so assumed as) non-priority categories. The chapter addresses only the priority species. A secondary classification is recommended for consideration: New Invaders (hopefully susceptible to a rapid response treatment), Species Targeted for Eradication (justifying a more aggressive approach; this category would include species that are still feasible to control by eradication), and Established Species (species that have become endemic, wherein control is used in an attempt to attempt to limit continued expansion and/or monoculturalization). This proposed classification would seem to more clearly indicate the District's goals than "priority" and "all of the rest."*

Response #3: The authors agree that a secondary classification system based on management approach helps to communicate the District's current goals for each species. Indeed, the District and other agencies informally utilize a similar classification system for strategic planning purposes. In previous years, species identified for early detection and rapid response were presented in a separate section. Due to space limitations in this volume, all priority species, for which there was notable new information to report, are presented in the "Progress toward Management and Control" section. The authors attempted to identify each species' management status in each species' update, but it appears that this information is difficult to discern in the current format. The authors will explore options to better identify species according to their

management status. An earlier version of this draft subordinate was structured by species kingdom and management status, but the authors felt that the organization of information might be confusing. The authors will reevaluate the structure this section to address the reviewer's concerns.

Comment #4: *Overall comment – it is recommended that the units used in Chapter 7 should follow the Metric system; but if the authors feel that it is more helpful to readers to use English units, the chapter should include metric units in parentheses after English units.*

Response #4: Concur---the authors will use metric units and include English units where appropriate.

Comment #5: *Lines 23-24 – state that the District spent $18 million in FY2011 on invasive species prevention, control and management. While this seems substantial, a major point that is quietly made throughout the draft chapter is the need for much more funding for nonindigenous invasive species control; successful control programs are only in place for a handful of priority species. (e.g. see lines 129-130). It might be helpful to clarify the proportion that that sum represents, of the total that was spent by the District in FY2011 for restoration/management activities. Hopefully, when the nation and the state finally emerge from this difficult economic*

time, the District will be able to direct more resources to nonindigenous invasive species control, considering that the chapter authors identify that more resources are critically needed to combat these formidable enemies, some of which threaten the success of the District's restoration efforts. The Panel hopes that the District can become even stronger in efforts to control nonindigenous invasive species, given its leadership and excellence in programs such as controlling melaleuca.

Response #5: The authors appreciate and agree with the reviewer's comments regarding current and future expected funding levels for invasive species management. A persistent concern among invasive species biologists and land managers is the lack of sufficient, dedicated funding for early detection and rapid response. A major obstacle for involved agencies is developing a mechanism to efficiently share resources across jurisdictions. This is a critical need when attempting rapid response efforts to contain and eradicate newly-established species. Developing a regional or statewide program that effectively integrates pooled resources to facilitate rapid responses in key locations would be a major achievement for Florida. However, this has proven to be a difficult task given budget constraints, conflicting missions, and limited authorities of agencies. This point was emphasized by an inter-agency team of scientists and land managers at a 2010 presentation to the South Florida Ecosystem Restoration Task Force (see Lines 903-911). Without strong leadership from key agencies, however, an integrated rapid response program is unlikely to materialize. As stated in the report, dedicated funding is also insufficient or non-existent for widely established species that exert significant pressures on the Everglades ecosystem (e.g. Old World climbing fern), particularly on federal and private lands.

Comment #6: *Line 168 – Is there a known reason why fire appears to aggravate infestations of downy rose myrtle?*

Response #6: Experience has shown that downy rosemyrtle rapidly resprouts following fire events, possibly facilitated by abundant carbohydrate reserves in the root system, but the authors are unaware of published research that examines these mechanisms. Interestingly, this plant is being evaluated as a potential vegetative firebreak in the Himalayans. Once the species forms dense thickets and outcompetes fine fuel species (e.g., grasses) the area becomes fire resistant. Obviously, this type of alteration of fire regimes is a serious threat to Florida's pyric plant communities.

Comment #7: *Lines 193-198 – The authors aptly point out that despite public perception that hydrilla is beneficial to duck hunting and fishing, this noxious plant must be managed in lakes with high-priority uses and infrastructure.*

Response #7: Noted. Thank you.

Comment #8: *Lines 319, 320 – are disjunct; please correct.*

Response #8: This will be done

Comment #9: *Lines 329-331 – are confusing because the writing seems to suddenly "switch" from discussion of shoebutton ardisia to focus on downy rose myrtle. This appears to be a mistake - please correct.*

Response #9: Mention of downy rosemyrtle is an error. The sentence is meant to refer to Ardisia. This will be changed.

Comment #10: *Line 347 – …is required for successful.*

Response #10: This will be done.

Comment #11: *Line 358 – …of the invasive plant list.*

Response #11: This will be done.

Comment #12: *Line 361 – …has found that it produces*

Response #12: This will be done.

Comment #13: *Lines 652-654 (also see Table 7-1, last row at bottom of p.7-4): Are these permanently established transects or ad hoc routes? How are routes established?*

Response #13: The routes, which are primarily levees and unimproved roads throughout the survey area, are resampled at each survey event. The description of the routes and the survey methodology will be rewritten to clarify this point.

Comment #14: *Pp. 7-19, 7-20 – it would be helpful to add information about when Burmese pythons and Nile monitors were first detected in South Florida habitats.*

Response #14: This will be done.

Comment #15: *Line 797: Laurel wilt is under a "Priority Animal" heading, but this is a fungus, not an animal (although it is introduced to host plants by a beetle). An additional heading such as "Other Priority Species" seems warranted.*

Response #15: This will be done.

Comment #16: *Lines 900-901 – mention that an update about the invasive mangrove species Lumnitzera racemosa is to be provided in this chapter. However, the only other mention of this species appears to be on lines 1046-1048, "Prompter cooperative action to eliminate…the invasive mangrove species Lumnitzera racemosa also appear[s] to be successful. If the authors meant to highlight this species and provide an update about it, then more is needed. If not, the wording on lines 900-901 should be altered.*

Response #16: The final version of the report will include an update on Lumnitzera racemosa.

Comment #17: *Table 7-1 What percentage if wetland acreage in the Biscayne Bay Coastal Wetlands do the 25 acres that were treated comprise?*

Response #17: This information will be included in the final version. The authors will also clarify that the 25 acres of treatment represents the area of infestation but that a much larger area was swept for these species.

Comment #18: *P.7-4, last entry, middle column, 3rd line - …Encounter rates ranged from…*

Response #18: This will be done.

Comment #19: *P.7-5, last entry – cattail is a native bioinvasive species, not a nonindigenous species. Clarification is needed, considering the title of this chapter.*

Response #19: Duly noted. This entry will be removed from the table in the final draft.

Comment #20: *P.7-5 – should the U.S. Fish and Wildlife Service also be listed as a partner? (e.g. line 884)*

Response #20: Yes. This will be done.

Comment #21: *Table 7-2*

Unfortunately, this table is basically missing because all that can be gleaned from it in its present, incomplete version is the top eleven priority species for the District. The table legend should include more explanation about how these species were selected as the "top eleven" priorities. Quantitative information should be added, if available, about the amount of affected area in each land management region (i.e. by module and system-wide).Text accompanying Table 7-2 – should explain whether these eleven top priority nonindigenous invasive species are a subgroup of the 24 top priority species that were identified in Chapter 9 of the 6 2011 SFER; and to explain this major difference in prioritization from one year to the next (also see p.1 of this Review, "Consistency with earlier versions").

Response #21: Table 7-2 is intended to summarize District expenditures for control of priority plant species during the fiscal year. The authors will revise the text on Lines 119-120 to make it more clear that the table only represents expenditures by the District in accordance with existing funding levels. This chapter attempts to provide a comprehensive update on regionally significant invasive species, regardless of the District's direct involvement in managing the species Therefore, there are many species discussed in the report that are not presented in this expenditure table. The authors also wish to point out the expenditure level does not necessarily correlate with priority level. Unit costs for invasive species management across species are extremely variable. For example, control of incipient populations of Old World climbing fern in remote portions of the WCA's is a very high priority for the District but the unit cost per acre for control is orders of magnitude lower than that of shoebutton ardisia control in disturbed marl prairies of the eastern Everglades.

Comment #22: *Table 7-2 concerns plants only. If possible, for consistency, it would be helpful to add a similar table for the four priority animal species (Burmese python, northern African python, Nile monitor, Argentine black and white tegu) and the fungus that causes laurel wilt.*

Response #22: The authors agree that a comprehensive treatment on control costs for all priority taxa would be consistent (and very informative), but reporting expenditures from other agencies would go beyond the scope of this chapter. On more local levels, there are efforts underway to compile this information. For example, the Everglades CISMA gathers much of this information on an annual basis. The Everglades CISMA recently collaborated with the South Florida Ecosystem Restoration Task Force to compile this information into a central database (ECOSTEMS). Unfortunately, staffing resources are currently insufficient to accomplish this goal. Hopefully, this effort will be revived in the future.

Comment #23: *Line 872 – should mention the type(s) of training.*

Response #23: Agreed. This will be done.

Comment #24: *Lines 878-879 – should describe the specific location of the Everglades CISMA (e.g. see lines 915-916*

for the Treasure Coast CISMA).

Response #24: Agreed. This will be done.

Comment #25: *Lines 913-917 – should mention the year that the Treasure Coast CISMA was formed (e.g. see line 879 for the Everglades CISMA).*

Response #25: Agreed. This will be done.

Comment #26: *Figure 7-10 - does not name the CISMA in the far southwestern portion of Florida (Everglades), or designate the Osceola County Cooperative Weed Management Area. In addition, some of the colors in the key do not match the map colors. Also, the key includes the designation "Lake Okeechobee" – is this in reference to the Lake Okeechobee Aquatic Plant Management Interagency Task Force? And, shouldn't the Kissimmee River and Chain of Lakes Coordination also be shown in this figure? Please resolve these discrepancies.*

Response #26: Agreed. This will be done.

Comment #27: *Lines 972-986 – explain APHIS' new NAPPRA category, and mentions APHIS' other two categories, but does not provide information about how the bioinvasive plant species in South Florida have been categorized to date. This information should be added (e.g. as a small table).*

Response #27: This will be done if space allows. The authors agree that a summary table of all priority invasive species showing their current regulatory status would be informative. However, the table is likely to require most of a page if all 24 priority species are to be included.

Comment #28: *Lines 991-996 – list nine species of large constrictors that the USFWS has proposed to classify as injurious species under the Lacey Act. Two of these species, the Burmese (Indian) python and the northern African python, are designated by the District as priority species in this draft chapter. The writing should clarify which of the other seven species are also found in South Florida, and should describe their status.*

Response #28: This will be done.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 8

Joyce Zhang and Bruce Sharfstein

Level of Panel Review: Technical
Reviewers: P. Dillon (AA) and V. Novotny (A)

Posted: 10/07/11 at 03:15 PM by S. Ollis for J. Zhang

Comment #1: *As in previous years, this chapter provides an extensive amount of information on the current status of Lake Okeechobee and on measures that have been introduced to control its nutrient, particularly phosphorus, input. It is evident that the remediation programs have been and will continue to be extensive but as of now they are mostly in planning, research or early implementation stages. Consequently, the changes of the water quality and ecological status of the lake can be mostly attributed to hydrology and occasional extreme meteorological events. The lower TP concentrations and external loads during the WY 2011 are likely more a result of drought than of abatement.*

Response #1: We agree with the reviewer's comments. The lower external load during the WY2011 is a result of lower flow due to drought conditions. As stated in the 2011 Lake Okeechobee Protection Plan, extensive activities will need to be implemented to meet the Lake's TMDL.

Comment #2: *In general, the interpretation of the results appears to be technically sound. However, progress still has not been rapid and the challenges appear to be almost insurmountable. It will require implementation of almost all of the proposed reduction strategies and long-term success of almost every one if the planned load reductions are to be achieved. There appears to be little chance of reaching the goal by 2015, although because of the five year moving averaging of the loads, the 2015 deadline may, in fact, allow until 2020.*

Response #2: We agree. As noted in the 2011 Lake Okeechobee Protection Plan, the timeline for meeting the TMDL depends on funding availability, as well as technical issues. It is anticipated that there will be some lag time between implementation of the strategies/projects and the desired hydrological and ecological results, due to a number of technical factors that are difficult to predict, including legacy phosphorus in the watershed and internal lake loading.

Comment #3: *The programs of reducing external loads are in the initials stages and, in the agricultural area where participation is voluntary, full participation of agribusinesses (farmers) has not been yet achieved. This differentiates the Lake Okeechobee programs from the Southern Everglades programs which are adaptive and have a capability to develop and implement rules, i.e., the programs is a hybrid between voluntary and mandatory legislated programs.*

Response #3[3]: Full participation of agricultural BMPs is expected by 2017 if funding allows. It should be noted that there has been a 40% reduction in phosphate use in the Lake Okeechobee watershed between 2001 and 2010. We attribute a significant portion of that reduction to nutrient management BMPs.

Comment #4: *Chapter 8 is much better organized than in previous reports. There are some redundancies concerning the discussion of P loads remaining that could be ameliorated in next year's report, but in general, the chapter is well-written with the information clearly presented.*

Response #4: Thank you.

Comment #5: *The writers are not always consistent with units. Because the report is offered to a wide national and international audience use of international units is preferred, i.e., µg/L unit is better than ppb and 1,000 m3 is better than acre-ft, with the US equivalents in parentheses or vice versa. This will give consistency to the entire report.*

Response #5: We agree with the reviewer's comments and will adopt this approach in future editions of Chapter 8 of the SFER.

Comment #6: *The methodology used to derive the 140 ton target load should be utilized to estimate the P load target to achieve the same lake P under different long-term hydrologic scenarios. For example, if the long-term water supply to the lake decreases by 25%, the P load may drop as well, but the combined effect of both changes on the P concentration in the lake needs to be known.*

Response #6: The TMDL was set by FDEP based on our understanding of the lake hydrology and water quality at the time (2000) using a set of models that were calibrated and validated to lake conditions. The TMDL is load based and compared to a five year average in loads to account

[3] Refer to page App. 1-3-67 for revised agency response to panel comment #3, web-posted on 10/31/2011.

for variation in hydrology. While the proposal to evaluate the TMDL using lower flow rates is interesting, any changes to the TMDL must occur through a re-evaluation conducted by the FDEP with updated knowledge and understanding of the lake.

Comment #7: *The problems with the Nubbin Slough STA need to be addressed, and a mechanism needs to be put in place to deal with future problems (as per Taylor Creek) in a more timely manner.*

Response #7: Agree. The USACE is in charge of the construction and preliminary operations of the STA. The District is the local project sponsor and will work with the USACE to resolve the sediment issues and bank stability. It is anticipated that this STA will be operational in the 2012 wet season.

Comment #8: *The Northern Everglades Chemical Treatment Pilot Project should be initiated in the next year.*

Response #8: Staff resources will be allocated next year to study the feasibility of the chemical treatment at several large STA sites.

Comment #9: *Funding for the additional activities in Table 8-3 should be a priority because without them, there is no possibility of reaching the target load.*

Response #9: Agree. All activities identified will need to be implemented to reach the target load. The coordinate agencies will need to prioritize projects in light of the current financial situation.

Comment #10: *As noted last year, mt is not the proper terminology for metric ton.*

Response #10: We have discussed this with our technical editors during the past and it was decided to keep mt as metric ton and it has been used as such throughout the South Florida Environmental Report.

Comment #11: *The monitoring data are impressive in scope and it is good to see the District data and the USGS data combined. It is also very useful to have the loading data by contributing watersheds and sub-watersheds. Given the complex structure of the Lake's catchment with multiple branched tributaries, it would be worth exploring the nutrient model INCA (Integrated Catchment model). This model, which is widely used in Europe, deals with P and N fluxes from complex branched systems using a GIS and land use framework as a starting point.*

Although the number of sampling sites is high, the frequency of sampling is not. It is not clear whether there is any event-based sampling taking place, but large portions of the annual load from a tributary can occur in a few storm events.

Response #11: Future innovations in modeling will continue to be pursued and the product suggested sounds very appropriate for our watershed. The constraints with budgeting and staffing make it very difficult to employ an event driven monitoring network, however, the technology for receiving real time data from flow or stage recording stations has allowed us to start developing recorded flow type monitoring. This will allow us to better concentrate our resources on getting data during these critical flow events and will provide us better opportunities to catch the "first flush" type of events that are drivers of the water quality characteristics of the watershed and the lake.

Comment #12: *Although the P budgets are central to this report as they are to the well-being of the Lake, there is some redundancy in the information presented. In future, it would be helpful to put all of the P (and N) budget material in a single section of the chapter rather than cover the same material in several different sub-sections. This would avoid the overlap.*

Response #12: Thank you for your suggestion. While it might be possible to consolidate the reporting of P and N budget material somewhat, one of the primary purposes of the SFER is to satisfy specific reporting requirements for various permits and initiatives. As a consequence, information presented in Chapter 8 is typically organized to reflect these specific reporting requirements.

Comment #13: *Figure 8-11 illustrates that the nutrient loads reflect the annual water load very closely. A figure showing mean volume-weighted inflow P concentration would be useful (simply total P load divided by total water load).*

Response #13: We will certainly consider adding a figure similar to the one you describe to the next issue of the SFER. However, because we are now trying to maintain strict page limits for each section, many of the usual tables and figures that are included in the chapter were eliminated this year and we will probably maintain our selective standards in future editions as well.

Comment #14: *This section deals with monitoring of submerged vegetation, invasive vegetation species, phytoplankton composition, fish, wading birds and a very small subset of the benthos. Perhaps this section should be called Biological Monitoring and the previous section Chemical Monitoring.*

Response #14: Chemical monitoring (water quality monitoring) data appears in many sections, chapters, and appendices of the SFER. As a consequence, a more specific title was chose for the referenced section.

Comment #15: *The periphyton studies are comprehensive, perhaps too much so if efforts that would be better focused elsewhere are decreased.*

Studies of macroinvertebrates have focused on a key species, the Florida Apple Snail. The potential stocking of this species show promise. A more general benthic sampling program should be implemented as a regular part of the monitoring effort. This can be done very effectively with sampling only once or twice a year (unlike many other parts of the biotic assemblage).

Response #15: We agree with this comment and have suspended further periphyton work until hydrologic or nutrient conditions change sufficiently to indicate the need for additional studies under new conditions.

If funding is available, we also intend to re-initiate our regular macroinvertebrate sampling program at some level of effort.

Comment #16: *It is recommended that the health of the fish population is expressed by a lake Index of Biotic Integrity. Figures 8-28 and 8-29 lose too much detail because of the different scales needed for number and biomass. It would be helpful to use a double scale on each figure or to plot biomass X 10.*

Response #16: Thank you for your suggestions. Schulz et al. (1999) studied 60 Florida lakes and tried to develop the Index Biotic Integrity. It was concluded that the approach may be of limited usefulness for predicting anthropogenic impact. We can look at the possibility and will certainly consider adopting them in the next year's report.

Schulz E.J.. M.V. Hoyer, and D.E. Canfield, Jr. 1999. An Index of Biotic Integrity: A Test with Limnological and Fish Data from Sixty Florida Lakes. Transaction of the American Fisheries Society 128:564-577.

Comment #17 *It would also be most useful to identify some additional biological performance measures (analogous to Table 8-8) to keep track quantitatively of how fish, benthos and other organisms are doing.*

Response #17: We agree that additional biological performance measures would be useful. One of the goals of our ecological monitoring program is to acquire enough data on major system components to allow for the development of such additional performance measures which would stand up to our rigorous stakeholder review process. However, because the Lake is a very dynamic system that is often driven by extremes of climate and other stochastic events, accumulating sufficient data to define meaningful performance measures can take many years.

Posted: 10/31/11 at 08:46 AM by S. Ollis for S. Gornak

Additional Response to Comment #3 on the 2012 SFER - Volume I, Chapter 8 (Steffany Gornak)

Comment #3: *The programs of reducing external loads are in the initials stages and, in the agricultural area where participation is voluntary, full participation of agribusinesses (farmers) has not been yet achieved. This differentiates the Lake Okeechobee programs from the Southern Everglades programs which are adaptive and have a capability to develop and implement rules, i.e., the programs is a hybrid between voluntary and mandatory legislated programs.*

Response #3: Full participation of agricultural BMPs is expected by 2017 if funding allows. It should be noted that there has been a 40% reduction in phosphate use in the Lake Okeechobee watershed between 2001 and 2010. We attribute a significant portion of that reduction to nutrient management BMPs.

Additional Response: The 40% reduction in phosphate use was based on consumer fertilizer tonnage reports which are tracked by the Florida Department of Agriculture and Consumer Services. Reduction in fertilizer use could also be related to other factors including fertilizer prices, business needs, changes in land use, changes in crop production, etc.

Actual load reductions from BMP programs will be evaluated in the future when the Lake Okeechobee Works of the District Rule (Chapter 40E-61, Florida Administrative Code) has been amended to include performance metrics for source controls. The rule amendment will reflect the most recent Northern Everglades and Estuaries Protection Program (373.4595 Florida Statutes) requirements.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 9

Bradley L. Jones, David H. Anderson
and Stephen G. Bousquin

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

Posted: 10/07/11 at 08:24 AM by B. Jones

Comment #1: *Kissimmee River Restoration Project*

There are a few broad questions and comments that could be addressed to help those not familiar with this part of the overall south Florida restoration efforts.

First and foremost of these questions is why the specific section of the C38 canal, namely the approximate middle third, was selected for restoration. Considering the relative ecological health of the upper basin (above Lake Kissimmee) and the connectivity to Lake Okeechobee on the lower

end it is odd that the upper section (or possibly the lower section was not targeted for restoration to maintain a contiguous (relatively) ecologically healthy section. The risk with the current approach is that the hydrologically restored section will not respond ecologically as predicted. This question is perhaps "water over the dam" considering the investment in the current approach, but a justification would be valuable.

Secondly, there is some concern that the approach to restore this particular section requires additional degradation of other locations (in the widening of other feeder canals) to maintain appropriate hydrologic regimes in the restored section. One can easily wonder whether the results will be to shift the ecological damage to a new location which when completed will require new ecological restoration measures. Considering the high profile of this flood protection turned ecological restoration project, the concern that the District and the Corps are simply repeating the same mistakes is a real concern. What measures have been taken to insure that the cycle will not repeat again?

Response #1: Early in the project it was determined that sections of C-38 were needed at the upstream and downstream end of the project to maintain flood protection. Some of the spoil originally excavated was used for other purposes, so the quantity of available spoil limited the amount of canal that could be backfilled. The cost of land acquisition also influenced the size of the final project footprint.

The canals that are being widened are smaller than the C-38, so that there is not an even trade of impacts based on area. Also, the project designs are reviewed from an ecological perspective and involve consultation with other agencies. Designs have been modified so as not to impact sensitive areas such as cypress strands along a canal bank.

Comment #2: *It would appear that much better integration between activities and especially knowledge of various divisions within the District could be achieved. Many supposedly unanswered questions or stated findings in this chapter could be anticipated (or alternatively new concerns raised) if knowledge reported in other chapters were applied. Examples abound, but a few are listed specifically below.*

One example is the lack an apparent lack of integration between this chapter and Chapter 7 on invasive species. Altering the hydrologic regime of the upland and flood plains and littoral zones will undoubtedly shift species of flora as desired and expected, but as demonstrated in Chapter 7 this shift is not necessarily to desired native species. The shifting hydrologic regime will like lead to manifestations of non-native species, a problem throughout the District, but apparently not as severe in the Kissimmee basin. This may change as the hydrology is altered. How will this be addressed and what mitigation techniques are being considered?

Response #2: Substantial integration takes within the District on an ongoing basis between the authors of Chapter 7 and Chapter 9, both with respect to implementation and refinement of species management plans and in the preparation of our respective SFER chapters. The District has a robust vegetation management program, providing operational leadership on sovereign lands and waters within District boundaries including those in the Kissimmee Basin, and coordinates with inter-agency teams such as the Kissimmee Chain of Lakes Aquatic Plant Management Group and the Florida Hydrilla Management Summits.

We are aware of the potential for invasive species to respond aggressively to hydrologic change. For discussions of 25 invasive plant and animal species of potential concern in the Lower Kissimmee Basin, assessments and management strategies for these species, and the roles of specific agencies in species management in the Kissimmee Basin, we refer the reviewer to Chapter 11 in the 2010 SFER, which included a special section, "*Invasive, Nonindigenous Species in the Kissimmee River and Floodplain.*"

Comment #3: *Kissimmee River Hydrologic Conditions WY2011*

Precipitation in the Kissimmee River Basin was about 20% below average in WY2011, which was much closer to the long-term mean than that in the southern-most areas of south Florida which experienced extended and severe drought. Flow into Lake Okeechobee was about 40% of the long-term average in WY2011. How will these more extreme climate and hydrologic conditions affect the KR Restoration when they occur?

Response #3: Since the first phase of construction was completed in 2001, the Kissimmee Basin has experienced a range of hydrologic conditions ranging from drought to intense rainfall associated with hurricanes and tropical storms. During the last ten years, four flow events have exceeded the pre-channelization maximum discharge for a 28 year period of record. During the high discharge events, there is a lot of sediment in transport and evidence of erosion and deposition. However, the distribution of point bars has remained fairly consistent. During high flows, fish will move from the river channel to the floodplain, but floodplain depths can become too deep for wading birds to forage.

The river has also experienced drought conditions when discharge from the upper basin stopped for 252 days in the winter of 2006-07. When flow stopped, the water surface profile of the river flattened, and the water surface elevation was determined by the downstream water control structure. This resulted in shallow water depths in the river channel and very dry floodplain in the upper reaches of the restoration project. Moving downstream, water depth increased in the river channel until the floodplain was also inundated. Because of cooler temperatures during most of this period, dissolved oxygen concentrations were increasing and remained well above thresholds of concern. The number of wading birds using the floodplain was much lower than was observed in the preceding or following years.

During the interim period, the effects of extremes in rainfall have been exacerbated by of the lack of storage in the upper basin. During wet periods, more water has to be moved through the system to maintain flood control. During drier periods, there is less water in storage to maintain flow. When project construction is completed and the headwaters revitalization schedule is implemented, the increased storage in the upper basin should allow for moderate and more natural flow regime.

Comment #4: *Kissimmee River Restoration Evaluation Project*

This is an extremely important section of the report. Twenty-five performance measures have been defined to allow quantitative evaluation of how well the project meets its ecological integrity goal. These expectations are based on estimated conditions in the pre-channelized system; the authors should be commended for establishing these expectations through external peer review. Table 9-3 which lists all of the monitoring studies and expectations also provides reference to results reported in all previous annual reports for each expectation. This is very useful and should be continued in subsequent annual reports. It would be helpful to number the expectations when discussing them throughout the document so that Table 9-3 can be referenced easily.

Response #4: Thanks for this suggestion. We will number the expectations.

Comment #5: *Of the 5 hydrology-based expectations, only 1 was fully met, while a second was met at some sites. This probably results from a combination of the low precipitation in WY2011 and some of the construction effects. However, annual variability in precipitation is such that 20% below the long-term mean (or 20% higher) is not likely to be a rare event, and needs to be planned for. Are procedures in place that will aid in achieving expectations in the future in cases where climate parameters are significantly outside the norm?*

Response #5: In addition to channelizing the Kissimmee River, the C&SF Project reduced the amount of storage in the upper basin, so that water has to be moved more quickly through the system to maintain flood control. At the end of construction for the Kissimmee River Restoration Project, the loss of storage will be addressed by the implementation of a new regulation schedule (the Headwaters Revitalization Schedule). This schedule will allow water levels in Kissimmee-Cypress-Hatchineha to go 1.5 feet higher and create an additional 100,000 ac-ft of storage. Because of the additional storage, the headwaters schedule is expected to allow more gradual changes in flows.

The lack of storage in the upper basin has been a major challenge for managing flow in the Kissimmee River during the interim period. Prior to channelization, the river typically had an annual flood pulse that was characteristic of the pre-channelization river with sufficient magnitude to inundate the floodplain followed by a slow recession of long duration. The characteristics of this flood pulse are captured in the expectations for hydrology. Interim operations results in multiple flood events of shorter duration and more rapid recession rate. Implementation of the headwaters schedule should greatly improve the frequency with which the expectations are met.

Comment #6: *The loading of P via the Kissimmee River Basin to Lake Okeechobee is high, with long term averages of 83 metric tons at S-65D and substantially more downstream nearer the Lake. It is surprising that there is no expectation around P loads. The authors note that determining long-term loading trends can be difficult because of the variability in hydrology; this is true but as they point out valuable information can be obtained by looking at the flow-weighted mean concentration (i.e. total load divided by total flow). An expectation based on concentrations should be feasible. The importance of hydrology is clear; although the P load of the last 5 years is 50% lower than that of the previous 5 years, this is almost certainly attributable to the 3 drought years in the last 5.*

Response #6: A draft expectation for post-restoration TP concentrations was prepared several years ago, but staff did not want to commit to a phosphorus reduction target for this project whose primary goal is to restore ecological integrity. This decision was appropriate because the restoration project was not designed to remove nutrients (i.e., it is not a constructed wetland like the STAs) and the amount of reduction that might occur could not be predicted with an acceptable amount of certainty. The modeling of phosphorus movement through the river and floodplain was very simplified due to a lack of data, and the results were accompanied by a large amount of uncertainty concerning the phosphorus removal efficiency as water flowed over the restored floodplain.

Comment #7: *The district has learned much about P retention under flooded and inundated wetland areas from operation of the STAs and monitoring of adjacent WCAs and wildlife refuges (Chapter 5). A take-home message of these results is that alternating dry and inundated conditions tends to increase P export from these locations as the drying cycle mineralizes soil sequestered P releasing it to the water column. Therefore a restoration of a more natural hydrologic regime, with alternating wet and dry periods, may actually increase P export and one (negative) outcome of restoration might be more P loading to Lake Okeechobee and downstream regions of the EPA. The collected data (higher TP export since restoration) supports this conclusion, and suggesting that the observed trends are a transitory effect related to immature wetlands is not supported by the District's research results in other locations.*

Response #7: The mineral soils of the floodplain may respond differently to alternating wet and dry periods than the organic soils of the STAs. Our current hypothesis is that net P release and export from floodplain soils may occur over a limited period, but will not continue long after restoration is finished. However, this question should be examined further and we are doing that

in our current study of Kissimmee River floodplain soils, which will also take the STA results into account.

Comment #8: *There are detailed data concerning the fish species reported, with some of the expectations being met. There is also some indication that changing the habitat through large-scale construction projects may influence fish communities negatively by, for example, creating habitat suitable for exotic species that are not currently present. The sentence beginning in line 687 is worded poorly.*

Response #8: It is true that modification of habitat can favor certain species adapted to the modified conditions. Indeed, construction of the C-38 canal in the 1960s had dramatic negative effects on native fish populations in the Kissimmee River (Koebel, 1995; Glenn, 2005) by favoring species tolerant of low dissolved oxygen and non-flowing conditions. The intent of construction of the Kissimmee River Restoration Project is to recreate the pre-channelization physical and hydrologic components of habitat in the river and floodplain (e.g., continuous flow, extended floodplain inundation, and improvements of dissolved oxygen concentration). These changes in turn are expected to lead to recovery of other components of habitat such as the prey base and suitable breeding and nursery areas, which are expected to favor recovery of native fish populations.

The referenced sentence has been rewritten as follows:

The relative abundance of redbreast sunfish remains far below the expected level for the post-restoration river. Factors potentially affecting the relative abundance of redbreast sunfish more than other centrarchids may include the species' requirements for sustained flow velocities (Kearns, 2001) and low turbidity (Aho et al., 1986). Redbreast sunfish populations may increase relative to other species upon completion of the KRRP and implementation of the Headwaters Revitalization schedule, which is expected to maintain mean flow velocities within a range (0.2–0.6 m/s) that is more suitable for this species.

References

Glenn, J.L., III. 2005. Status of fish assemblages of the Kissimmee River prior to restoration: baseline conditions and expectations for restoration. In: S.G. Bousquin, D.H. Anderson, G.W. Williams and D.J. Colangelo, eds. Kissimmee River Restoration Studies Volume I – Establishing a Baseline: Pre-restoration Studies of the Channelized Kissimmee River. Technical Publication ERA #432. South Florida Water Management District, West Palm Beach, FL.

Koebel, J.W. Jr. 1995. An historical perspective on the Kissimmee River Restoration Project. Restoration Ecology 3: 149-159.

Comment #9: *Fig 9-1 and 9-2: The insert location indicated (lower left of each figure) is the same for both figures. Clear it is right for only one; I am guessing it is right for Fig 9-1 and wrong for fig 9-2.*

Response #9: This insert will be corrected in Fig. 9-2.

Comment #10: *Table 9-3: As mentioned earlier, an updated version of this table should be included in each annual report. It could be applied to other chapters with significant long-term monitoring components such as Lake Okeechobee, the STAs and sulfur/mercury interactions. That said, it would be nice to list the entire page range, as would be done in a typical reference list. More importantly the table lists an expectation number and except for a few specifically written in the subsequent sections of the chapter, the read does not know what these are. A table that simply lists what the various expectations are should be included.*

Response #10: We will try to include the entire page range if it can be made to fit. We will include the expectation numbers in the text as they are discussed. Because this chapter is designed to be an update on recent work, we have not included a table listing all the expectations. The text cites earlier publications containing these expectations.

Comment #11: *Figs 9-10 through 9-13: It would be desirable to list the location of these sampling locations on a map, perhaps Figs 9-1 and 9-2.*

Response #11: We will consider an additional figure that shows the monitoring sites in Figures 9-10 and 9-11. We will refer to Figure 9-2 for the sites shown in Figures 9-12 and 9-13.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, CHAPTER 10

Richard Alleman and Chapter Coauthors

Level of Panel Review: Technical
Reviewers: J. Burkholder (AA), V. Novotny (A)

Posted: 09/14/11 at 12:52 PM by R. Alleman

Comment #1: *Explanation of the District's coastal priorities is lacking – The draft Coastal Estuaries chapter for the 2012 SFER is shortened to such an extreme that the chapter falls short of providing a clear description of the District's coastal priorities (with exception of Biscayne Bay). The major goal, to restore historic water balance or, at least, water balance that protects valued ecosystem components (VECs), is fairly clear. The District's coastal priorities are not. Thus, the subject of the chapter title is not addressed well in the chapter writing. Instead, readers are simply given a brief synopsis of projects that were addressed (focused upon) by the District in WY2011 (lines 29-31. That is a 'very different animal' than the clear explanation of the District's Coastal Priorities which was 'promised' by the title and which should have been included. Moreover, Florida Bay, which has been an obvious coastal ecosystem of focus, now is confusingly covered within the Everglades Research and Evaluation chapter (Chapter 6), without explanation in draft Chapter (e.g. line 13) or in draft Chapter 6. The Summary mentions five main subjects as the District's main focus in the Coastal Ecosystems during WY2011. One might infer from the writing that these five subject areas will provide the chapter structure but that is not the case. Rather, the chapter is then divided into sections named by estuary (or, in the case of the Northern Estuaries, the section is given that name).*

Response #1: We agree that the title of the chapter is imperfect, and some clarification is needed. The intent was to cover, as you state and described in the Introduction, District efforts in WY2011 that impacted coastal systems. These were the priorities for the water year. It is not intended to be a strategic plan. The reviewers are referred to the District's annual strategic plan for that information. We present important results from systems where restoration was active in WY2011, or where water rules were in place. The chapter is not intended to be a scientific discussion, but may be characterized more as an overview of the status of the systems where work is proceeding. Appendices 1 and 2 provide much more detail about the northern estuaries for purposes of a three-year update to the Northern Watershed and Estuaries Protection Plans. For next year, we may return to our previous format of including the more detailed technical information about the northern estuaries in the Chapter. This type of information is more conducive to a technical review.

We chose to keep the structure based on water bodies, since many stakeholders are interested in primarily in one system rather than District-wide issues, and District emphasis varies by water body.

The Florida Bay section was moved back to its original location under the Everglades chapter, because the restoration projects to benefit Florida Bay occur upstream within the Everglades. We will add an explanation.

Comment #2: *The important Summary and Introduction sections fall far short of providing the useful information needed, including a clear summary of the District's coastal priorities and how those overall priorities were specifically addressed in WY2011 - The District's emphasis on Coastal Ecosystems in WY2011 was described to include five subjects: (i) nutrient load reduction, storage projects, and water inflows from Lake Okeechobee to the Northern Estuaries; (ii) a pilot project to add freshwater to the Loxahatchee River Estuary during the dry season; (iii) a sediment trapping project in Lake Worth Lagoon; (iv) partial initiation of a flow redistribution project in lower Biscayne Bay; and (v) water control improvements on the southwest coast to benefit the Fakahatchee Estuary and Naples Bay. This basically is a list of projects, not overarching priorities with supporting rationale. Figure 10-1, a map of key coastal regions within District boundaries, includes no key for readers to use in deciphering differently colored areas, no labels for the coastal designations, and in fact shows, instead of key coastal regions, the names of projects that evidently were addressed in WY2011. The word "evidently" is used here in consideration of the fact that, while some of the project labels are mentioned in the Summary, others are not, or are differently named (this problem with the Summary needs to be rectified).*

Progress was then described for five monitoring/research/modeling efforts. They do not match the above five subjects and there is no explanation as to where they fit within them. The five monitoring/ research/modeling efforts were described as follows:

• Initiated a project to develop capability to "capture data" about the productive low-salinity zone within riverine estuaries under differing inflow regimes;

• Continued monitoring submersed aquatic vegetation (SAV) in the Caloosahatchee River Estuary (CRE) and Southern Indian River Lagoon (SIRL), emphasizing tape grass (freshwater eelgrass, Vallisneria americana) in the upper estuary;

• Completed a Science Plan for the Loxahatchee River as part of a collaborative effort;

• Initiated development of a hydrodynamic model for Naples Bay; and

• Successfully applied the Caloosahatchee River Hydrodynamic Model on a weekly basis to predict salinity in the estuary, which informed operational decisions about supplemental flows from Lake Okeechobee.

It seems logical, and would be helpful to readers, for the Summary section to briefly identify the major issues impacting the Coastal Ecosystems that will require restoration, but neither the Summary nor the highly inadequate, one-paragraph Introduction to the chapter do so. Previous SFERs identified three 3 major issues – disruption of the natural magnitude and timing of freshwater discharges, increased inputs of nutrients and other materials of concern, and continued loss of critical ecosystem habitats and biological communities.

Not until the third section, on the Northern Estuaries, are readers given information about major issues, and at that, not as clearly or generically as was done in previous versions of this chapter. In the Northern Estuaries section, the writing describes freshwater flow into the Northern Estuaries as a primary concern – both excessive freshwater flows in the wet season and minimal freshwater flows in the dry season, depending on Lake Okeechobee's regulation schedule.

Because of the minimal freshwater flow situation, salinities exceed levels needed to sustain key species such as Vallisneria americana. The authors then state that "nutrient enrichment in these estuaries is believed to cause phytoplankton blooms…" and that the decomposition of major blooms "may also depress dissolved oxygen (DO) concentrations (SFWMD et al. 2009a,b)." These impacts of anthropogenic nutrient pollution are not "believed to cause" algal blooms in these systems, and are not merely "possibilities" –rather, they have been documented in estuaries worldwide (Glibert et al. 2005, Glibert and Burkholder 2006), including South Florida estuaries (see the SFWMD et al. 2009a,b references cited by the authors of draft Chapter 10).

Response #2: We agree that the overall general problems with District estuaries should be stated. In terms of how the District prioritizes individual projects, please see the response above about the Strategic Plan.

We will add more information to the legend of Figure 1.

The bulleted list given in the summary are, as described, activities in addition to restoration projects, specifically monitoring, research and modeling activities. These are highlighted, since they are also discussed under the individual water body sections as appropriate.

While the Northern Estuaries are nutrient enriched, phytoplankton blooms are relatively rare in the estuaries. Blooms tend to occur in the freshwater tributaries, primarily. The specific causes of phytoplankton blooms in the estuary are not well sorted out.

Comment #3: *Even chronic problems, including some that would be very easy to address, that have been identified in reviews of previous versions of this chapter have resurfaced, such as English units, use of more than one unit for the same parameter, lack of definitions/explanations for key concepts such as MFLs, and insufficient information to enable technical evaluation – These problems, together with the lack of needed explanation and clarity in the chapter, have directly resulted comments such as:*

• A problem with this chapter is the use of old units (e.g. acre-foot, ppm, etc.) without conversion, and units are undefined and sometimes "switched" (e.g. psu vs. ppt)…. Because of the international and national significance of this annual environmental assessment report that is being released into the public domain, using proper units is paramount….It is recommended that before the final release of this chapter, proper definitions and conversion to international units are included. The authors should be aware that readers of their work may be unfamiliar with the system, the jargon, and undefined units. [For example,] the chapter refers to minimum flows and level (MFL) criteria which were not defined.

• There is also inadequate description of the coastal ecosystems, which impedes technical evaluation of the chapter. For example, with exception of Naples Bay, it is not known if the estuaries are stratified or completely mixed, which would strongly affect choice of sampling design and management/modeling considerations.

• P.5 – The nutrient loads to the estuaries were simply described as, "TN and TP loads were lower than last water year," which means nothing without further explanation. In addition, there is no interpretation that is needed to help readers' understanding – in this case, no mention of the drought which would have been expected to result in the lower TN and TP loads. As another of many such examples, the statement, "grass abundance was greatest in the fall, and declined throughout the rest of the water year as salinity increased" is, again, meaningless to readers: Is this a problem, or was it simply natural variation?

P.8, Figure 10-4 describes flows and management scenarios for the Loxahatchee Estuary. What is the "2 ppt line" and its significance? (That is, the legend of Figure 10-4 surely needs further explanation.] While previous SFERs can be consulted, this chapter should at least briefly include

such fundamental information. Otherwise, readers will be forced to consult numerous references, sentence by sentence, to try to decipher the meaning.

Pp.12-17 deal with Biscayne Bay south of the Great Miami urban agglomeration. In some portion of the bay, extensive macroalgal blooms were observed but the cause is cursorily dismissed as unknown. Is the main macroalgal species a known responder to nutrient pollution? The salinity of the bay is impacted by freshwater flows from a large number of drainage canals carrying urban runoff. The draft chapter reports that nutrients in the bay were within or below the (undefined) historical range and seagrass increased somehow (?).

• Overall, the chapter reports mostly routine flow manipulations to attain (mostly undefined) salinity targets. There is an almost casual, and clearly inadequate, description of the ecology of these vitally important systems (e.g. "TN and TP were the same as last year"). The present water quality status is also insufficiently described, mostly by reporting visual observations and monitoring salinity….So, how can the technical merit of District activities be assessed? Apparently, the major problem is excessive freshwater (probably polluted), sometimes intermittent (e.g., Naples Bay), drainage flows causing large salinity fluctuations, resulting potentially in osmotic shock to fish. The potential damage to the ecology of these coastal systems by freshwater inflows and low salinity should be at least briefly described, as well as the reasons for the salinity limits (a succinct table could be used to accomplish this). The status and health of important species in these systems should be given where available, and data gaps should be identified; instead, in draft Chapter 10, with very few exceptions, the key species almost entirely go without mention.

Response #3: We agree that metric units should be added.

When salinity was estimated using electrical conductivity, we indicate the salinity using practical salinity units (psu) even though the results technically are unit-less. See previous-year's responses to this comment. In some cases, particularly when describing criteria within a rule, we use parts per thousand (ppt) for salinity to be consistent with the rule language.

It is not our intent to provide comprehensive descriptions of the estuaries as that information can be found in other documents. We will ensure that references are given.

As indicated, we are only providing an overview of results. Where the results have not changed from the previous year, details are not given.

We will add more information about Figure 10-4.

At this point, and certainly within the 2011 water year, we are not aware of any change of water quality in the North Central region of Biscayne Bay that may be contributing to a macroalgal bloom. Few District-operated canals discharge into this area. Other organizations are monitoring it, but no cause has been identified.

Where the District has adopted salinity criteria, either by rule, or within approved documents, they are given for each water body along with the historical and current results. References are given that describe the rationale for each.

Comment #4: *Line 61 and throughout the chapter – change submerged to submersed; submerged is used incorrectly in describing these plants (see Wetzel 2001, Limnology, 3rd edition). The supporting figure (Figure 10-6) is compelling.*

Response #4: The words submerged and submersed are synonyms. Both are used in literature when referring to seagrasses. In the past, at least, SFER editors have preferred to use the adjective submerged.

Comment #5: *St. Lucie River Estuary and Southern Indian River Lagoon –*

During the dry season, supplemental freshwater flows from Lake Okeechobee were not necessary to maintain salinity within acceptable levels; the MFL criteria were met throughout the year. Lines 102- 106 require more explanation, including clear interpretation (e.g. drought influence on TN and TP loads – see above comments).

Response #5: Details of freshwater inflows and nutrient loads in the St. Lucie Estuary are provided in Appendix 10-1.

Comment #6: *Caloosahatchee River Estuary and Southern Charlotte Harbor (through S-79) –*

A map showing S-79 is needed. Despite numerous water releases from Lake Okeechobee, salinity exceeded the MFL criterion for nearly 60% of the year (215 days). Lines 115-118 need more explanation, including clear interpretation.

Response #6: Details of freshwater inflows to the Caloosahatchee River Estuary are provided in Appendix 10-2.

Comment #7: *Loxahatchee River and Estuary and Lake Wirth Lagoon [note suggested change to section title]*

The only major issue identified in both systems is reduced inflows that have promoted increased salinity in the upper river segments (e.g. lines 135-140, 150-152). This description is inadequate; previous SFERs have described District concerns about other very serious water quality problems in addition to salinity (and see lines 233-242 of the draft chapter). The introductory writing in this section should be altered accordingly.

Loxahatchee River and Estuary -

Lines 141-147 – "mix apples and oranges" – the writing mentions what happened in 2003 (adoption of a MFL rule for the Northwest Fork of the Loxahatchee River) and then describes an experiment that the District conducted in WY2011 and collaboration in WY2011 on completion of a Science Plan for the Loxahatchee River watershed and estuary (although the water year went unmentioned, adding to the confusion).

Lines 164-173 – describe a successful collaborative experiment, involving the District, that was conducted for 48 days (Mar 1-Apr 19) during the WY2011 drought. The objective was to add sufficient freshwater into the Northwest Fork of the Loxahatchee River to meet MFL criteria (note: a map of the water routing should be included to assist readers).

Lines 174-181 – more explanation is needed to enable technical evaluation.

Lines 183-194 – briefly describe continued bimonthly monitoring of seagrass (and, evidently, water quality although that was only mentioned) at four locations along a salinity gradient and one background location in the Loxahatchee River Estuary. Readers are referred to the 2011 Addendum to the Restoration Plan for the Loxahatchee River for both methods and results. Thus, consistently as throughout the draft chapter, the information provided does not enable technical review. For example, the authors state, "Comparison of water quality with seagrass data shows only shoal grass (Halodule wrightii) and Johnson's seagrass (Halophila johnsonii) are successful in the darker water areas…." No supporting data, methods, etc. whatsoever are given to enable readers to evaluate the technical merit of this statement. Comparison of draft Chapters 6 and 10 is instructive here. Both chapters were to be evaluated on technical merit. Many volumes of information have been written about some of the large-scale experiments such as CHIP. Rather than instructing readers to look elsewhere for any and all of the technical information, Chapter 6 is excellent in technical merit because it provided, succinctly and clearly, the information needed to evaluate technical merit. Such an approach is critically needed throughout Chapter 10.

Lines 195-204 – describe oyster monitoring by FDEP, but without including any of the technical information that would be needed to evaluate technical merit.

Lines 205-218 – mention surveys of hydrology and vegetation at ten Loxahatchee River transects from calendar years 2003-2010. Some interesting information on groundcover stem counts was described, but no technical information was mentioned. Thus, the technical merit of the study cannot be evaluated.

Lines 221-230 – as a milestone of progress, a Science Plan was completed in 2010 for the Loxahatchee River to prioritize monitoring efforts and fill in knowledge gaps about ecosystem restoration success.

Response #7: It is not the intent to provide a comprehensive list of problems with these estuaries. This information is contained in other documents. The problems mentioned relate to the specific issues that District projects addressed during WY2011.

The list of items for the Loxahatchee River is intended to provide an introduction about the results described later. It will be clarified.

Details of the project to move water to the Loxahatchee River should be available in the SFER project database. Figure 10-3 indicates the features used to move the water.

It is not the intent to provide technical details of the monitoring programs as they are available in other documents.

Comment #8: *Lake Worth Lagoon –*

The introductory information for this subsection nicely summarizes major concerns of the District about the quality as well as the quantity of stormwater runoff entering this system.

Lines 243-266 – describe a collaborative effort, involving the District, to conduct annual hydrographic surveys (2007-2010) of the C-51 canal system following a major sediment management project (muck removal) in 2006. The overall objectives were to evaluate the effectiveness of a sediment trap, and to examine erosion vs. accretion in Lake Worth Lagoon over the (unspecified) project area. The project had a mixed outcome: The sediment trap was a success during 2007-2009; there was a net accretion of ~11,394 cubic yards, mostly accumulated in the sediment trap. However, in the final project year (2009-2010), there was a major loss of sediment throughout all canal reaches, apparently because of an increase in volume flushing through the S-155 structure.

Lines 259-266 – somewhat confusingly describe a second project during 2009-2010. Evidently, ~41,000 cubic yards of sand from near Ibis Isle, a mangrove fringed island, were brought into the above project area to cap the muck and raise the wetland shelf to ~intertidal levels, and then mangroves and cordgrass were planted there and lime rock was added to create oyster habitat. Although the project was described as "completed" in 2010, there is no mention of whether the project was successful - whether the mangroves and cordgrass survived and grew, or oysters established, or the sand effectively capped the muck, etc. For both projects, no technical information is presented; thus, technical merit cannot be evaluated.

Response #8: We will add some information about the success of the Ibis Isles project.

Comment #9: *Biscayne Bay –*

Identified major issues were altered salinity patterns, water quality, and reduced fisheries relative to historical conditions. In this section the District's coastal priority for Biscayne Bay was identified, namely, restoration of the south-central area by redistributing freshwater flows through the Biscayne Bay Coastal Wetlands (BBCW) Project. This effort is also expected to result in "some incidental reduction" (line 279) of nutrient loads to the bay (readers should be directed

to p.10-16 for further information). The District monitors water flow, water quality, and vegetation as required by permits, but no technical information is provided about these efforts; thus, their technical merit cannot be evaluated.

The District also monitors salinity in the nearshore area of south-central Biscayne Bay to assess how the system responds to canal inflows. The data indicate hypersaline conditions during the dry season, which is considered unhealthy for many of the estuarine species. The first phase of the BBCW Project unfortunately is not expected to alleviate this serious problem; the second phase might, but planning has not been scheduled to date. Clarification would be helpful - how far into the future is it anticipated that the second phase will be developed? The authors also mention that the District is presently investigating the feasibility of operational changes that could alleviate the hypersaline conditions. Overall, the writing seems to indicate that this serious situation of hypersaline conditions is unresolved and likely to remain so for a considerable period of time, unfortunately impairing estuarine health.

Lines 272-297, 313-334 – A serious imbalance in water supply for estuarine health is also expected to affect Manatee Bay and Barnes Sound in (already prone to hypersalinity; see lines 314-316) southern Biscayne Bay, wherein the C-111 Spreader Canal Western Project, a CERP restoration project, will restore some freshwater flows to northeast Florida Bay while reducing freshwater inputs to Manatee Bay until "more" (unspecified) of that project is actually implemented. The authors state that the "expedited components" of this project became operational in August 2011. What does this mean for freshwater flows to Manatee Bay from August 2011 on? – clarification of this writing is needed.

Lines 304-309 – a major increase in the chlorophyte Anadyomene stellata is described in north-central 7 Biscayne Bay, just offshore of the western shoreline. This macroalga has maintained increased abundance at least since 2005, and the authors aptly note that its abundance in this area is out of natural proportion. Although the draft chapter states that there was no obvious cause, Collado-Vides et al. (2011, Spatio-temporal patterns and nutrient status of macroalgae in a heavily managed region of Biscayne Bay, Florida, USA - Botanica Marina 54: 377-390) analyzed tissue nutrient contents of macroalgae in Biscayne Bay and reported that tissues at all sites had very high nitrogen content and high N:P values. Moreover, some macroalgae, including close relatives of this species (and likely this species as well), are considered excellent indicators of nutrient pollution (see Collado-Vides et al. 2011, and references therein). Are there plans to assess the impacts of A. stellata on the ecosystem, and the role of nutrient pollution/sources from freshwater deliveries to the Bay in stimulating the major blooms of A. stellata? In this regard, excessive anthropogenic contributions of nutrients (N and P) are known to affect Biscayne Bay (see Collado-Vides et al. 2011, and references therein), and (lines 376-382) tissue analyses of seagrasses along the western nearshore zone of the central Bay indicate N-replete habitat.

Moreover, lines 365-369 mention excessive inorganic N during the wet season of WY2011 (Sept, Nov, Dec). The chapter writing seems inordinately cautious here: The cause of the excessive inorganic N is described as "not clear, but the peaks in N concentration appear to be associated with lower-salinity events that occurred…." Precipitation events are known to contribute excessive inorganic N from anthropogenic sources in estuaries throughout the world, including the U.S., the Southeast, and Florida (see the National Research Council 2000, Clean Coastal Waters – Understanding and Reducing the Effects of Nutrient Pollution, National Academy Press; and see Collado-Vides et al. 2011, and references therein). This writing does little to help readers' understanding, and should clarify (1-2 sentences would accomplish this) the enormous pollution pressures that affect Biscayne Bay from the extremely large adjacent human population (e.g. see Collado-Vides et al. 2011, and references therein).

Line 344 – percent, or parts per thousand (or psu…)? As a general comment, throughout the chapter, salinity should be unit-less. See: United Nations Educational, Scientific and Cultural Organization (UNESCO) (1981) Background papers and supporting data on the International Equation of State of Sea Water 1980. UNESCO Technical Papers in Marine Science No. 38. UNESCO, Paris. UNESCO (1985) The International System of Units in Oceanography. UNESCO Technical Papers in Marine Science No. 45. UNESCO, Paris. If the District elects to use units for salinity anyway (e.g. for consistency with other SFERs), then only one unit should be used consistently throughout the chapter.

Lines 372-376 – describe "visual surveys" of fishes since 1998, apparently undertaken by NOAA.

Lines 376-384 describe annual surveys for SAV and macroalgae; readers are referred to a "gray literature" report for further information. The writing asserts that the seagrass Thalassia testudinum "increased somewhat from previous years," and that "macroalgae was [should be: were] more abundant than seagrasses." Additional information is needed; as written, these sentences are scientifically meaningless. Moreover, because no technical information was provided, it is not possible to evaluate whether this information is science-based, and whether the technical approaches were sound.

Lines 385-390 – briefly describe an apparently successful effort (Apr 2011) to "clear" 25 acres of coastal wetlands of non-indigenous invasive plants. Was this a District priority for the coastal ecosystems, or simply project effort in WY2011 under "Valued Ecosystem Components Highlights"?

Response #9: No timeline is available for completing the second phase of the Biscayne Bay Coastal Wetlands Project.

For details of the effects of the C-111 Spreader Project, readers are directed to Chapter 6.

See previous response to comments about the macroalgal bloom in North Central Biscayne Bay. In regard to the paper mentioned, while a good start, it provides little insight about the habits of *Anadyomene stellata*, and focuses on other species indicative of freshwater inputs. This and other research in Biscayne Bay indicates that water temperature plays a large role in macroalgal growth. The District has no specific plans to investigate the *Anadyomene stellata* bloom at this time as other organizations have the lead. We will be monitoring results, however.

The pollution pressures on Biscayne Bay are well documented elsewhere.

See previous responses about the use of "psu".

A reference will be supplied regarding the visual fish census monitoring.

We will provide more information about the exotic vegetation removal.

Comment #10 *Fakahatchee Estuary –*

The CERP Picayune Strand Restoration Project is planned to rehydrate the former Southern Golden Gate Estates Subdivision by restoring pre-drainage hydrology. It is envisioned that restoration will create a combined natural area that functions as a single connected regional ecosystem of estuaries, freshwater wetlands, and uplands. The altered hydrology and impacts in the area, and District restoration efforts thus far, are nicely described, except that explanation is needed (p.10-18, 10-19) about the projected time to completion, the restoration phases etc.

Lines 457-478 – describe a ~decade-old study of oyster resources in the area; the results were evaluated as helpful but inconclusive in determining a baseline or pre-implementation condition, because of the short study period. It is unclear as to why this information was included, as it has little to do with WY2011 and, because it is so dated, provides information of limited utility about oysters in the area. Technical details were not provided.

Response #10: A timeline for completion of the Picayune Strand Project is not clear at this point.

We will consider removing the information about the oyster study.

Comment #11: *Naples Bay –*

Stratification problems during the wet season in northern Naples Bay apparently have increased due to higher freshwater flow and construction of deep, dead-end canals. The lower (southern? – please alter for consistency) bay is less affected by such problems because of more tidal mixing. The District is engaged in efforts to reduce the freshwater discharges that are degrading water quality in Naples Bay (inferred as its major coastal priority for Naples Bay). The District is also funding near-real-time data collection (15-minute intervals) of salinity, temperature, and tidal water level by the USGS at two stations (May 2011–), and data collection from a third station is planned. In addition, near-real-time flow rate data are being collected at Gordon Pass Inlet (15-minute intervals). Based on Figure 10-15, this station unfortunately was dry from Nov-Apr in WY2011. The data are being used to develop a three-dimensional hydrodynamic and salinity model for Naples Bay and the Rookery Bay Estuarine System, and will be valuable for selecting optimal management/ restoration strategies. The model will simulate mixing, circulation, and distribution of salinity in Naples and Rookery bays using the Curvilinear Hydrodynamic three-dimensional modeling platform. This sounds like an excellent effort that will yield a very valuable model to guide restoration in Naples Bay. However, information needed to assess technical merit is not given.

Lines 517-518 – please clarify "up to 120 days of collection every 15 minutes"

Figure 10-14 shows the extreme salinity fluctuations ranging between less than 1 (presumably nearly all freshwater) and 35 (presumably mostly Gulf water).

Response #11: We will clarify the statement about the period or record of 15-minute data.

Posted: 10/27/11 at 10:57 AM by S. Ollis for P. Doering

Response to Final Comments on the 2012 SFER - Volume I, Chapter 10, Coastal Priorities

Peter Doering, Section Administrator, Coastal Ecosystems

Circumstances Associated with the Coastal Priorities Chapter: The reviewer's final comments on Chapter 10, "Coastal Priorities", begin with a perceptive description of the perennial challenge for the design of the chapter since its inception in 2005. As the reviewers point out, there are at least 8 coastal systems depending on how you delineate them and there are numerous agencies and governmental entities, in addition to the District, that monitor, regulate, and conduct research on these systems and their resources. Annually, an impressive volume of information on the coastal ecosystems within the District's boundaries can be generated. The challenge is what level of effort is appropriate to synthesize and interpret data from these disparate sources and towards what ends relevant to the District's mission. The reviewers make it abundantly clear that they seek far more scientific evaluation of coastal ecosystems.

We agree that because coastal resources are at the receiving end of all upstream activities, the District's coastal ecosystems integrate alterations to the landscape within their watersheds. To a large extent, their ecological condition reflects the success or failure of land-based restoration efforts. While this logic argues convincingly for inclusion of a "coastal chapter" in the South Florida Environmental Report, it does not define the content of the chapter or the level of technical detail.

The content, organization and length of what is now Chapter 10 has varied considerably over the years since it was incorporated into the SFER. Early efforts (2005-2006) reported on 9 different coastal systems and resulted in a chapter that averaged about 125 pages in length. The 2007

effort, only 59 pages long, reported on 9 systems again but placed a large amount of material into 3 appendices, with one being about 600 pages in length. Following a recommendation from the Review Panel, the Chapter then shifted its emphasis to highlight one or two systems each year with short reviews of the remaining 6 or 7 systems. This approach has resulted in Chapters ranging in length from 110 – 189 pages. In response to the Northern Everglades and Estuary Protection Program legislation reporting requirements, the St. Lucie and Caloosahatchee Estuaries have been emphasized since 2009.

Over the past two years, the scope and design of the coastal chapter have responded to efforts to streamline reporting and internal re-prioritization of agency work. . The reviewer's point out, there has been a concerted effort to focus and streamline the coastal chapter in particular and the SFER in general. Second, budgets have decreased significantly and this has constrained and reduced the District's activities in coastal systems, particularly in the monitoring and research arenas.

These two trends continued this year and underlie some rather fundamental criticisms both in the Panel's original comments and also in their final submission. In general, only summaries of projects, results, and environmental conditions were given in the body of the draft 2012 Chapter 10 and these were limited to "coastal ecosystems where the South Florida Water Management District … focused its efforts during WY2011". The intent was to reference the documents where technical details could be found. In addition, a major, 3-year update of the Caloosahatchee and St. Lucie River Watershed Protection Plans (part of the NEEPP) was required. It was decided that updates of these plans, which contain technical details, would be appendices to the coastal chapter (Chapter 10). As a result, the body of Chapter 10 has very little technical detail. While the NEEPP appendices are now available, they were not available at the time Chapter 10 was reviewed, further reducing technical content and adding to the Panel's frustration with the lack of technical information. While Chapter 10 may arguably present a summary overview of District activities in coastal ecosystems, we agree with the reviewers that streamlining of the chapter has gone too far and precludes technical review.

Plans for Responding to Panel Concerns over the Next Three Years: Given the importance of coastal ecosystems as integrators of land-based restoration, the quality of information presented in future chapters will rise to a level commensurate with a technical review. The chapter title will be amended to reflect a greater emphasis on presentation of technical results.

For the next two years, the chapter will highlight the St. Lucie and Caloosahatchee Estuaries to fulfill the reporting requirements of the NEEPP legislation. Specifically, the annual report shall include a summary of the conditions of hydrology, water quality and aquatic habitat based on results of the Research and Water Quality Monitoring Plans. Thus, we will also include results of NEEPP research projects completed by District staff or by contractors. The breadth and scope of information presented will depend on the District's level of effort. When the next major three year update of the NEEPP occurs, relevant appendices will be available at the time of Chapter review.

In addition to the NEEPP, the Coastal Ecosystems Section's program will focus on two elements of the District's core mission: operation of District infrastructure and quantifying environmental water supply to support natural systems. Progress on these fronts will be reported in the SFER and include:

Implementation of the Lake Okeechobee Regulation Schedule and Operations:

- Results of field studies to determine effects of pulsed, low level releases of freshwater to the St. Lucie, Caloosahatchee and Loxahatchee Estuaries

- Progress on developing a statistical position analysis to forecast of salinity in the Caloosahatchee and St. Lucie

Water Protection Rule Development:

- Report any technical work conducted to support reservations of water for the C-43 Reservoir and Biscayne Bay Coastal Wetland CERP Projects
- Report technical progress on 2017 update of the Caloosahatchee Minimum Flow and Level
- Report technical progress on the Naples Bay Hydrodynamic Model

SPECIAL REVIEW: RESPONSES TO COMMENTS ON DRAFT VOLUME I, APPENDIX 1-6

Jason Godin

Level of Panel Review: Technical
Reviewers: G. Christakos, Y. Rubin and J. Stedinger

Posted: 10/09/11 at 09:32 PM by J. Godin

General Response to Reviewers

This SFER based review of the SFWDAT provides expert guidance on the future development of these tools. The development of an automated, processed-based system for evaluating water conditions as discussed in Appendix 1-6 relies on a decision-tree based process for employing relevant geostatistical methods, e.g. Ordinary Kriging vs Universal Kriging. In finalizing responses to the review questions, please outline and define a process based approach as it may pertain as a solution to the questions asked, e.g. standard statistical tests for evaluating geostatistical assumptions like stationarity. Thank you for your expert assistance.

Level of Panel Review: Technical

Reviewer: Rubin

Comment #1: *The Report discusses interpolation of water depth using semivariograms of same variable. It is not a commonly used approach. Rather, the variable of choice is the hydraulic head.*

Response #1: Hydraulic heads or what are referred to as water elevations in the SFER document are the basis of all interpolations. Water depth is not interpolated, but calculated by displacing interpolated hydraulic heads from interpolated ground elevation.

Comment #2: *As a general comment, many of the assumptions and requirements discussed above are not met, or are not shown to be met, or are not documented. It is very much possible that I missed the relevant sections, and I expect that these issues will be clarified in the response.*

The concerns are raised here should best be viewed, for now as questions.

Response #2: Each interpolation basin meets different levels of the basic Ordinary Kringing assumptions. Please outline standard statistical methods and significance levels for evaluating these assumptions, such as stationarity, that could be employed into future development activities.

How would these assumptions be affected by the use of Universal Kriging as opposed to Ordinary Kriging?

Comment #3: *In page 1 line 24-26, the Report indicates that the water depth was subtracted from ground surface without correcting for the relief. This introduces an error into the semivariogram model and accentuates the effects of non-stationarity. As mentioned, stationarity is required for geostatistical interpolation. That non-stationarity could possibly be removed, but this is not done here, to my understanding.*

Response #3: Following Response #2 above, can you please outline standard statistical methods and significance levels for evaluating stationarity. Our goal is to improve the SFWDAT process by introducing a decision tree that would correct for drift / relief features based on optimizing for Kriging assumptions.

Comment #4: *This question needs to be addressed through cross validation (see point #17 in Background). Cross-validation is addressed in the Report, but additional information is needed in order to assess its adequacy. To begin with, no reference is provided.*

Response #4: Cross-validation as presented in this chapter of the SFER provides an estimation of error associated with systematically evaluating the difference between each point's observation and an interpolated estimate at that point as removed from the remaining interpolation data set.

Comment #5: *The term "cross validation RMSE" was not defined in the Report. Please provide details and definitions.*

Response #5: "cross validation RMSE" represents the Root Mean Square Error of the cross-validation methodology defined above in Response #4. Please define other standard cross-validation methodologies that could be evaluated.

Comment #6: *With regard to the question about modeling the semivariogram at the origin (zero lag distance), the theoretical models (see Background), which neglect measurement error, are differentiable (meaning, no nugget). The presence of a measurement error, which is unavoidable, would create a nugget effect and that nugget would render the semivariogram at the origin non-differentiable.*

Response #6: The presence of measurement error may provide a good argument for employing a nugget effect; however the SFWDAT needs to provide consistent water elevations with those that are recorded by each monitoring location and related database archive systems. The archived values are the basis of regulatory activities that mandate the operations of the regional system. Inconsistencies in water elevations between SFWDAT and point based monitoring databases could create issues for contention.

Comment #7: *As discussed in the Background section, the head semivariogram is anisotropic by definition. Heads (or water table elevations) are correlated over larger distances along streamlines (or mean flow direction). However, an isotropic version could be defined by averaging over all directions, and the isotropic semivariogram could then be used for kriging. Such an approach could be justified only following successful testing (possibly) using cross validation techniques.*

Response #7: Anisotropy does not appear to be present in local water elevation estimates. How would longer range anisotropy be effected by the use of Universal Kriging and which cross-validation techniques should be used evaluate for anisotropy?

Comment #8: *Geostatistical models differ in their underlying assumptions. Many assumptions are adopted in the process of developing such models. Although one or more assumptions may appear to be more reasonable than others, and possibly suggesting that a certain model should be preferred over another one, I believe that the only way to accept or reject such assumptions*

(and the associated models) is through a detailed, well-documented cross-validation. Furthermore, no assumption should be accepted without testing.

Response #8: Please outline standard statistical methods and significance levels for evaluating these assumptions, such as stationarity, that could be employed into future development activities.

Comment #9: *Based on my published work on this topic (see Background section), the magnitude of the average gradient should not pose a limitation. As discussed earlier, my concern here is with regard to variations in the mean gradient and the implications with regard to stationarity. The head semivariograms should be applied over regions with a uniform or slowly-varying mean*

head gradient. Pumping and injection introduce strong local effects that affect stationarity. With regard to maximum distances for interpolation, this issue needs to be investigated and resolved using cross-validation. It is not a major concern in high measurement density environments, but critical in others. The question of what constitutes high-density etc is still open and needs to be addressed.

Response #9: Pumping / injection activities predominate in the canal networks that interconnect the regional system and typically buffer large fluctuations in water elevations of gravity fed basins of interest. Please provide an outline of stationarity tests that could be used to evaluate spatial point densities.

Comment #10: *Experience shows that the quality of the semivariogram could deteriorate significantly at distances of the order of 0.4 to 0.5 of the range. This suggests to me that kriging should not be carried out over distances larger than that. It is difficult to determine what is the range here because the Local Fit model, which suggests the existence of a range (see Figure 5), is inconsistent with the Global Fit model.*

I would also like to see the number of pairs used for each point on the raw semivariogram to get a better sense about where the break in the quality of the semivariogram occurs.

Response #10: Since water level gauges are not typically clustered and the lag bin size is consistent with the average nearest neighbor distance, the largest number of pairs is at the lowest lag distances. The total number of variogram pairs is disproportionate from basin to basin and based on basin areal extent. How would the number of pairs, weighted by area, affect the reliability of the method? Is there a threshold of information loss that could be the basis for truncating the range of the variogram?

Comment #11: *There is also a perception that hydrologic models are more expensive in applications. I believe this is no longer true for most applications. The calibration of such models could be a challenge, depending on the availability of data for calibration. Given the abundance of water depth data, there is a very good basis to start with.*

Response #11: Typically the performance of hydrologic models in these basins have been 2-3 times less precise than the Kriging approached discussed here. Likewise, most hydrologic models are initialized by interpolated water elevations. What are the best geostatistical approaches that align with physical characteristics of these types of basins?

Comment #12: *Several of the semivariogram plots (e.g., Figure 5) suggest unbounded growth (i.e., no range), which could be a consequence of not removing the trend in ground surface elevations. In this case, it is unclear what is the model predictive capability without testing.*

Response #12: The models have been validated for 2 basins (WCA-3A and 3B) of contrasting gradients and align well despite the underlying long range trends.

Comment #13: *The Report does not provide an indication about the quality of the prediction/estimation error. The question is, how does the kriging error compare with actual*

error? This question could be address in part through cross validation. It would be helpful in assessing the predictive capability of the model. At best, we would want to see that the kriging errors compare well with the estimation errors at the testing points.

Response #13: A validation was presented in Figures 9 and 10 for WCA-3A and 3B basins. Additional validation datasets are presently being collected.

Comment #14: *Figure 5 in the Report shows a Local Fit model and a Global Fit model. I can see what could possibly be the motivation for adopting a local fit model, but in my opinion there is no justification for that, particularly since the consequences of this practice are not documented in the Report. This situation calls for modeling the raw semivariogram through a combination of authorized models (see Rubin, 2003, Section 2.3.6). It is a consistent modeling approach that would avoid the ambiguity and possibly estimation error due to the disparity between the Global and Local Fit Semivariograms.*

Response #14: What type of relationship should exist between the mean nearest neighbor distance, the number of variogram pairs, and the range? The semivariance models were best fit to the largest number of pairs and subsequently truncated longer ranges. What should be the cut off in terms of number of points to pairs that contribute to the range?

Comment #15: *A major problem with kriging is that there is no distributional model underlying the kriging error (for example, we cannot take it for granted that it is Normally-distributed, for example). Without a distribution for the errors, one cannot assign confidence intervals to the kriging estimates. Without a confidence interval one cannot assign any level of trust in the estimates. Note that kriging errors are not estimation errors. They can provide, in theory, some indication on whether the error is expected to be large or small, but in practice this rarely works. I would strongly caution against using a model unless the estimates could be associated with confidence intervals.*

Response #15: Are you suggesting the use of the validation data set to check for alignment with the standard error maps?

Level of Panel Review: Technical

Reviewer: Christakos

Comment #1: *5.1 At a methodological level this reasoning should be aware of the so-called self reference of Kriging (and of other spatial regression techniques), namely, one uses the variogram function to predict future water elevation values from the existing gauge dataset and, at the same time, one has used this gauge dataset to calculate the variogram function itself. Self-reference may lead to logical inconsistencies.*

Response #1: The SFWDAT methodology does not predict future water elevations. Each daily surface is based on an independent semivariance model fit for each basin for each day; there is no reliance on previous date models. The review document only exhibits models and figures for a couple of dates. Please provide additional information if self-reference applies to independent daily based models.

Comment #2: *Uncertainty quantification for spatial estimation purposes is a multi-sourced affair. Otherwise said, in situ water measurements maybe characterized by different kinds of uncertainties: technical, conceptual, physical, and aleatory. These different kinds need to be explicitly considered, since they have varying effects on the modeling of water levels and related site variables.*

Response #2: The presence of measurement error may provide a good argument for employing a nugget effect, however the SFWDAT needs to provide consistent water elevations with those that

are recorded by each monitoring location and related database archive systems. The archived values are the basis of regulatory activities that mandate the operations of the regional system.

Inconsistencies in water elevations between SFWDAT and point based monitoring databases could create issues for contention.

Comment #3: *Beyond the technical issues of gauge monitoring, physical uncertainty maybe related to weather conditions. For example, when one seeks to accurately represent normal water table, sampling times should be carefully chosen to reflect normal weather conditions. Data used to obtain groundwater surfaces may need to be based on water level readings taken on the same time period and from the same type of wells, etc.*

Response #3: For the present implementations these tools rely almost exclusively on surface water stage monitoring instruments, most of which have been recessed into the ground surface to capture below ground or water table levels.

Comment #4: *One observes in the report that there are regions in which the number of monitoring locations maybe not sufficient for spatial estimation purposes (see, e.g., lake Okeechobee and pool C of the Kissimmee floodplain; SFER Fig. 1). Recall that a sound variogram computation needs at least 30 pairs of points (some other investigators raise the number to at least 50). This is an uncertainty issue in need of clarification.*

Response #4: The density of gauges commensurate the spatial extent or area of the basins sampled. Since the monitoring locations are typically not clustered, the number of comparison pairs in the variogram is typically 2-3 times the number of locations. With the exception of WCA2-B with only 8 monitoring locations, the other basins all meet the >30 pairs for fitting the semivariance model. What would be a statistical approach for characterizing the number of gauges required per unit area, per unit of surface slope, per units of precision / uncertainty to estimate a surface?

Comment #5: *SFER's view of the situation is described in lines 71-72, among other places: "Interpolate surfaces of mean daily water elevations for the previous year for each basin independently." Temporal effects on water surface interpolation are implicitly ignored, although the dynamics of a water body with low relief, as is the case of South Florida sites, could exhibit strong temporal dependences.*

Response #5: Each daily set of data is intended to represent an independent perspective of the system. What benefits would be gained by exploiting the temporal effects?

Comment #6: *There is also a scale effect to be taken into consideration here (lines 64-76 of SFER). Generally, there is sufficient empirical evidence that different results are obtained if one*

(a) first integrate breakpoint data into daily mean water levels and then interpolate to generate surfaces of water level estimates vs.

(b) first interpolate breakpoint water level data and then integrate the breakpoint water level estimates into daily mean water levels.

8.1 It seems that SFWDAT is favoring Approach (a) rather than (b) to the integration interpolation problem. Is there any empirical support for this choice?

Response #6: Integration is meant in a literal sense, such that integrating the area under a curve, or more specifically temporally weighting sub-daily sampling intervals (e.g. 15 minute data) to approximate a daily average water elevation. There is no integration interpolation.

Comment #7: *The physical consistency of the OK assumptions and the specifics of their implementation in the South Florida ecosystems should be checked during various stages of the*

analysis. Otherwise said, one must be cautious whether or not the modeling assumptions of OK actually apply in situ.

9.1 OK generally assumes that the underlying probability distribution is Gaussian, the water elevation field is adequately described in terms of a constant spatial mean and the corresponding variogram. Measures of asymmetric dispersion, like skewness or kurtosis, are not considered.

Response #7: Please outline standard statistical methods and significance levels for evaluating these assumptions that could be employed into future development activities. How would the use of Univeral Kriging differ in evaluating these assumptions?

Comment #8: *The non-linearity of piezo-height estimates, for example, may be due to the varying penetration times required for rainwater to reach the subsurface water table. A cross validation study, linear vs non-linear water level estimators, may be helpful in assessing the in situ situation.*

Response #8: For the present implementations these tools rely almost exclusively on surface water stage monitoring instruments, most of which have been recessed into the ground surface to capture below ground or water table levels. Rainfall penetration lags are typically buffered-out through the process of daily averaging.

Comment #9: *In circumstances such as that described in §9.4, secondary (soft) information that provides some indirect information about water level distribution can potentially improve the accuracy of elevation estimates. In this study, some additional information exists, such as land use in Figure 11. Is in SFWDAT's plans to incorporate secondary information into water elevation mapping?*

Response #9: We are not presently considering the incorporation of land use into the SFWDAT methodology. Can you please provide examples either through publication or other efforts that have benefited from in the incorporation of land use information into water elevation surface modeling of natural systems?

Comment #10: *Having said that, some reflection is due. Consider global vs. local empirical variogram fitting (lines 140-146; and Figure 5). Due to the distinct behavior of raw variogram calculations within local neighborhoods, SFER suggests that modeling should only be applied to local empirical variograms rather than to those of the entire study area.*

Two possible concerns need to be addressed here:

(a) Only three points are used to represent the so-called local behavior of spatial dependence. One may argue that more points at varying distances within the locality are needed to validate SFER's assumptions.

(b) Should nested variogram models be included that can account for spatiotemporal dependence in multiple spatial and temporal scales?

Response #10: (a) The variogram plots with error bars depict the average condition at each lag interval. There are several pairs (often times hundreds) of comparison points utilized to fit the variogram models, not just 3 points displayed.

(b) How would one implement nested variogram models or is the use of Universal Kriging a viable solution to removing trends that account for the multi-scaled raw variograms?

Comment #11: *One would argue that the anisotropy of water level observations be always*

checked when using geostatistical methods.

Response #11: Anisotropy does not appear to be present in local water elevation estimates. How would longer range anisotropy be effected by the use of Universal Kriging and which cross-validation techniques should be used to evaluate for anisotropy?

Comment #12: *15. If Kriging with no nugget value is favored (line 105ff), it is implied that water level does not exhibit any kind of discontinuous spatial variation.*

Response #12: In close proximity, such as a lag distance approaching 0, water elevations in all of these areas are continuous. Likewise, the SFWDAT needs to provide consistent water elevations with those that are recorded by each monitoring location and related database archive systems. The archived values are the basis of regulatory activities that mandate the operations of the regional system. Inconsistencies in water elevations between SFWDAT and point based monitoring databases could create issues for contention..

Level of Panel Review: Technical

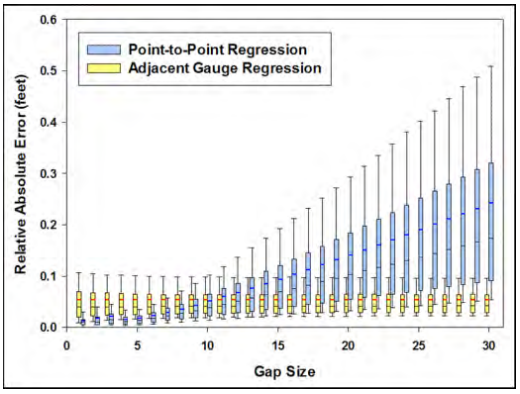
Reviewer: Stedinger

Comment #1: *Given that the SFWMD and USGS have such a good network, and the interpolation is done well, it should provide a very good basis for determining water surface elevations for the listed purposes. The exception may be c, because I do not know what is needed for permit support –does one need historical observed values for such functions, or forecasts of future values?*

Response #1: At the present time permit support is limited to current and historical water elevation surfaces and associated water depths. There is no forecasting functionality.

Comment #2: *Page 4 line 68:I assume here that point-to-point means that you use the time series at the point of interest because that is sufficient, whereas for longer gaps you rely on a regression employing nearby gauges. This seems reasonable, the question is determining when to switch over from the short to the long--term solution. If the time step is very short, then the response time between nearby gages and the gage of interest is much longer than the time between measurements at the gage, so one should interpolate between points at the same gauge that are close in time. However if the gap is very long, then nearby gage date becomes informative relative to data at the same gauge at some distance point in the past or the future.*

Response #2: The figure below exhibits a Monte Carlo simulation of performance thresholds between short (point-to-point) and long term gap filling (regression based); In this example the threshold is ~10-days.



Comment #3: *Page 8 I was wondering why a nugget was not employed to allow measurement error, or very local variations in depth, but I see in figure 5 there is not support for it in the data.*

Response #3: The SFWDAT needs to provide consistent water elevations with those that are recorded by each monitoring location and related database archive systems. The archived values are the basis of regulatory activities that mandate the operations of the regional system. Inconsistencies in water elevations between SFWDAT and point based monitoring databases could create issues for contention.

Comment #4: *The global variance model in the figure does not fit the local values well, and that relates to a poor choice of global functions. I think the choice of semivariance deserves to be improved. Excluding points more than some fixed distance away is reasonable, and was perhaps done.*

Response #4: The local variance model is predicated on the exclusion or truncation of longer range lags due to distances beyond the interpolation requirements (e.g. nearest neighbor distances were typically within the range) and the smaller number of pairs per lag contributing to the fit.

Comment #5: *Page 9---- If you estimate the water level using kriging, and the ground surface elevation using kriging, you can compute a standard error for the water depth, which is the difference. Does SFWDAT provide such standard errors for water depth?*

Response #5: Presently we are not providing a cumulative standard error for water depths, but this is a good suggestion to include in the future revision to help communicate relative uncertainty to a wider audience.

Comment #6: *The point was made here that in many cases the water surface elevation is known with more precision than the ground surface. That being the case, perhaps substantial improvements in efforts to map the water surface are not needed, except perhaps near boundaries.*

Response #6: We are in the process of collecting higher precision ground surface information for several areas where water depth precision is key to operational activities. The Kissimmee floodplain and areas of Everglades National Park are presently being updated

Level of Panel Review: Technical

Reviewer: Burkholder

Comment #1: *Lines 121-125 – The authors suggested that application of universal kriging rather than ordinary kriging might be advantageous in areas with fewer gauges, or if monitoring networks were reduced.*

It would be helpful to clarify whether this refinement of the SFWDAT is being developed.

Response #1: The specialized review of Appendix 1-6 is focused on obtaining expert opinions of standard geostatistical approaches to hydrology. Key features of Universal Kriging should be beneficial to the process of the SFWDAT and will be explored farther following additional feedback from reviewer.

Comment #2: *Lines 175-179 –The higher errors along the basin boundaries suggested to the authors that it may be useful to impose boundary conditions where unconstrained (open to surrounding marsh) conveyance features (e.g. monitored canals) could provide additional interpolation network support. It would be helpful to clarify whether this modification is in fact being pursued to further improve the SFWDAT.*

Response #2: We are presently adding this enhancement to certain key features of the Kissimmee floodplain and will evaluate the feasibility of enhancing the EPA implementation.

Comment #3: *Lines 181-195 – A validation effort indicated that the 400 m-spaced heights are within a + 0.5 foot confidence interval (used as a nugget value), which is a relatively high level of uncertainty in ground elevation estimates. Brief explanation should be added about efforts, if underway, to further refine and improve this level of uncertainty.*

Response #3: We are in the process of collecting higher precision ground surface information for several areas where water depth precision is key to operational activities. The Kissimmee floodplain and areas of Everglades National Park are presently being updated.

Comment #4: *Lines 196-200 – Describes present implementation of the SFWDAT for the EPA as including three water depth-based ecological performance metrics. Please identify them more clearly.*

Response #4: The SFWDAT for the EPA presently tracks three water depth based performance metrics.

1. Muck Fire Area Index: Provides risk based (low to high) perspectives of muck fires occurring. This index is based on the combination of percent organics in the soil / sediments (static) and water depth.

2. Minimum Depth Area Index: Provides categorical perspectives of depth and duration criteria as based on the Everglades Minimum Flows and Levels (MFLs). Depth and duration criteria vary spatially depending on soil type (peat or marl based soils).

3. Wading Bird Foraging Index: A water depth recession and depth base index that provides perspectives of poor to optimal foraging characteristics.

RESPONSES TO COMMENTS ON DRAFT VOLUME I, APPENDIX 10-2

Pinar Balci and Lesley Bertolotti

Public Comments	District Responses
Coordination on TMDL/BMAP – Development and Need for Greater SFWMD Involvement - The FDEPs revisions to the Caloosahatchee Estuary TMDL provides an opportunity for the SFWMD to get more fully engaged in the development of both the marine and freshwater TMDLs and corresponding BMAPs. Lake Okeechobee releases contribute nutrient loads to the Caloosahatchee River that historically would flow south of Lake Okeechobee. Such loads should not be considered part of the natural background and must have a more efficient accounting.	We agree that our involvement in the Caloosahatchee TMDL and BMAP processes is essential and our role in these processes is accentuated by their inclusion in the NEEPP Legislation. The District staff has been and will continue to be fully engaged in the state Total Maximum Daily Load (TMDL) processes in the Caloosahatchee Watershed. Some highlights of our investment of time and resources during the development of the original FDEP estuarine TMDL and during its current revision process include: • Working very closely in aligning the Caloosahatchee RWPP and BMAP processes and streamlining reporting • Participating in multiple meetings including stakeholder meetings, and focused technical meetings and recent FDEP TMDL revision working group meetings in which the District has provided critical technical information and input on modeling alternatives and technical requirements. • Hosting a USEPA/FDEP workshop (July of 2011) to promote dialogue on the models being considered by the federal agency in its NNC rulemaking that FDEP is also considering for portions of the Caloosahatchee TMDL revisions. Thanks to Lee County, and the Charlotte Harbor National Estuary Program (CHNEP) for their participation. • Contributing staff time and funding to help support local projects and efforts. In 2010 and 2011, the District provided staff time and over $50,000 in the CHNEP's development of NNC recommendations. The District has also provided support (both monetary and staff time) for multiple local water quality projects that will help lower nutrient loading to the estuary and are being considered in the Caloosahatchee BMAP. • Also, three District water quality monitoring sites were used as the basis for water quality targets in the original TMDL within the estuary and our published work was cited in the documents as one of the reasons for the final modeling targets. In addition, the District, Lee County and FDEP each recently (in November 2010) submitted significant comments on the USEPA's four proposed TMDLs within the Caloosahatchee watershed (upstream of the DEP estuary TMDL). Regarding your comment regarding the Lake Okeechobee releases should not be considered part of the natural background, for the CRWPP Update flows and loads from Lake Okeechobee were excluded from the water quality analysis. As for the TMDL process, the Tidal Estuary TMDL addressed the total load at the estuary; the loads were separated into four major categories: (1) Anthropogenic loads from upstream of the Franklin lock, (2) Anthropogenic loads from the tidal basin, (3) Natural background loads, (4) Loads from Lake Okeechobee. The current BMAP only considers the load both anthropogenic as well as background that comes from the Tidal basin.

Pollution Source Control Program - While there was significant progress made on construction projects during the initial three years of the 2009 Plan, the first phase of the Pollutant Control Program has fallen short.Significant efforts have been made by FDEP and local governments to address local urban BMPs. Mandatory local urban fertilizer rules have been adopted by Lee County and most of the municipalities in Lee County. These local rules emphasize education in BMPs and have enforcement consequences should applicators of fertilizer fail to educate, register and properly apply fertilizer. Unfortunately, the agricultural BMP rules adopted are voluntary and have no enforcement mechanism to ensure compliance.	We agree that there has been significant progress made on local water quality and storage projects. The District has been partners on many of these projects and has provided monetary support, staff time or both. Since 2004, the District has provided a total of $15.6 million dollars in funding, with matching local dollars, for 80 water quality improvement projects. As discussed in the Pollutant Source Control Program Section of the CRWPP Update, there has been progress made in this program such as the amendment to the biosolids rule, Chapter 62-640, F.A.C, and development of key technical data analysis to support the NEEPP requires expansion of the Caloosahatchee Nutrient Source Control Program. However there have been challenges in implementation especially in funding and in the regulatory arena. Although enrollment in the FDACS agricultural BMP program is voluntary, it is anticipated that eventually all operators will either be enrolled and compliant with this program, or required to demonstrate compliance with an expanded 40E-61 SFWMD Regulatory Source Control Program. In the Caloosahatchee watershed where BMPs have been adopted by rule of the FDACS the NEEPP requires agricultural operators to do one of two things: (1) implement the FDACS BMPs applicable to their operation; or (2) demonstrate compliance with the District's Works of the District program by conducting monitoring prescribed by the Department or the District. Under the FDACS BMP program the compliance component is called "BMP Implementation Assurance". The BMP implementation assurance check includes a site visit to determine if the applicable BMPs identified in their "Notice of Intent to Implement" document are being operated and maintained. It also includes records reviews for BMPs such as nutrient management. BMP compliance mechanisms are described further in the source control chapter of the SFER (Volume I, Chapter 4).

Pollution Source Control Program - Proposed revisions to the regulatory programs (40E-61 Regulatory Nutrient Source Control Program, ERP Basin Rule, Statewide Stormwater Rule) as identified in Phase I have had no significant developments since 2008. The revisions to 40E-61 were intended to expand the Program boundary to include the Caloosahatchee River Watershed and focus on Nitrogen as well as Phosphorus. The ERP Basin Rule was intended to ensure new development would result in no net increase in total runoff that ultimately discharges into the Caloosahatchee Estuary. NEEPA mandated that these regulatory provisions be "implemented on an expedited basis." Florida Statute, Section 373.4595(4)(a)(2)(a).	While F.S. Section 373.NEEPP does require regulatory provisions be "implemented on an expedited basis" it does not specifically refer to ERP Legislation or to the ERP Basin Rule. This response is separated into the three programs/rules as follows: Statewide ERP Legislation: As discussed in the Source Control Section, FDEP is proposing Statewide Environmental Resource Permit legislation in coordination with the Office of Fiscal Accountability and Regulatory Reform (OFARR). This legislation addresses the adoption of statewide environmental resource permitting rules to govern the construction, alteration, operation, maintenance, repair, abandonment, and removal of surface water management systems. These new rules will rely primarily on the rules of the FDEP and the water management districts that are currently in effect, reconciling any differences for a statewide approach. Differing physical and natural conditions will be accounted for. The applicant's handbook, adopted as a part of this rule, will include a discussion of stormwater quality and quantity criteria. Basin Rule: We assume the "ERP Basin Rule" referenced in the comment is referring to the "Lake Okeechobee and Estuary Watershed Basin Rule" (LOER). As presented at the 7/21/11 Northern Everglades Interagency Meeting, the District is not moving forward with this rule. The Lake Okeechobee Phase II Technical Plan (2008) and the 2009 RWPPs included development of a Northern Everglades Basin Rule to address improvement in hydrology in accordance with the intent section of the NEEPP. During rule development a proposed methodology to demonstrate no increase in the average annual discharge volume from existing conditions was developed using calculations similar to the draft Statewide Stormwater Rule. It was proposed that the methodology could be applied under existing ERP criteria. This proposal was included in the latest version of the LOPP Update (2011) but it will not be pursued due to concerns that implementation of the methodology using existing criteria would be considered non-rule policy. 40E-61 Regulatory Nutrient Source Control Program: Although the developments for the revision of the 40E-61 Regulatory Nutrient Source Control Program may not be apparent, there has been significant technical groundwork to support and expedite the stakeholder/peer consultation and rule revision process. Besides the expansion of program boundaries and inclusion of nitrogen, at the core of the proposed revisions is the development of performance measure methodologies to assess the overall progress of the collective source control programs (FDACS' voluntary and SFWMD's regulatory) to affect phosphorus and nitrogen levels in runoff. As discussed in Chapter 4 of the South Florida Environmental Report, the District has identified the monitoring network that will be used for regulatory purposes and analyzed historic water quality data which will serve as the baseline for evaluation. Development of the preliminary performance measure methodologies is underway and will be subject to peer review by the end of 2012.
Pollution Source Control Program - Lastly, there has been no noted progress with the Comprehensive Planning and Growth Management Project.	FDEP and SFWMD are continuing to contribute technical and regulatory guidance for local and regional planning activities consistent with legislative changes in the growth management area.

Research and Water Quality Monitoring Program - The 2009 Plan has improved upon the modeling tools used to predict the response of the Caloosahatchee Estuary to changes in freshwater inflow. However, more robust sampling and analysis is necessary in order to adequately represent the water quality of the Caloosahatchee River Estuary. It must be noted, the current version of the system monitoring program does not address basin-wide issues. The modeling focuses on the area west of S-79 (Tidal Caloosahatchee) and the relationship within the Estuary as influenced by rainfall. While water quality is significantly impacted by rainfall, water quality within the basin is important in defining the source and magnitude of pollutants reaching the Estuary. The collected information may then be used to target areas and activities most appropriate for remedial action or retrofit. Thus, the protection of the watershed may only be achieved via total watershed assessment.	We agree that identifying the location and magnitude of important nutrient sources within the basin is a key step in choosing appropriate, cost-effective approaches for nutrient load reduction. As indicated in Chapter 4 of the South Florida Environmental Report, the District conducted a synoptic water quality and flow monitoring project at 30 freshwater tributaries upstream of S-79, in 2009 . Findings have provided insight on streams with higher nutrient concentration, the need to install permanent flow monitoring stations for accurate representation of load, and the fact that tributary areas may discharge to multiple streams, thus, the need for multiple monitoring locations to ensure that data are representative of tributary areas. Regretfully, these initiatives are costly and continued monitoring is needed to ensure that findings are representative in the long term. These efforts will be continued upon fund availability.
Issues and Actions - Future Recommendations. The 2012-2014 Plan implementation schedule (Table 24) does not adequately address the shortcomings in the previously stated goals of revising the 40E-61 Regulatory Nutrient Source Control Program, developing an ERP Basin Rule and a Statewide Stormwater Rule.	Pertaining revisions to 40E-61 Regulatory Nutrient Source Control Program: The District has completed key technical activities for establishment of collective performance measures required for source control program development in the Caloosahatchee River Watershed. Table 24 indicates that implementation of the SFWMD Regulatory Source Control Program for the Caloosahatchee watershed is planned to be initiated between 2012 and 2014, pending coordination with the Office of Fiscal Accountability and Regulatory Reform. This schedule is aligned with initiation of the stakeholder consultation and peer review in 2012. Nevertheless, rule development and rulemaking schedules may vary upon stakeholder considerations, regulatory considerations and the need for additional technical evaluation. Please also see the response to comment 3 above regarding the Basin Rule and the Statewide ERP Legislation.

Issues and Actions - Future Recommendations. Additionally, the Addressing the Water Quantity section, fails to mention the development of the Water Reservation for the Caloosahatchee Estuary. Increasing water storage is accomplished not only by capital projects and the Dispersed Water Management Program, but also by regulatory action. The Plan Update should include a more detailed account of the Caloosahatchee Water Reservation rulemaking initiative and should be specifically listed as a "Regulatory Activity" in the 2012-2014 Plan implementation schedule (Table 24).	Agree. Text was included on the development of the Caloosahatchee Estuary water reservation rule and associated activities. Additionally, Caloosahatchee Estuary water reservation rule making is listed as a regulatory activity in the near-term activity table to be completed by 2015.
Line 73. Coordinating agency section, needs to include local government and special district roles as they are being tasked with an overwhelming portion of implementation. Further, this should be expanded to include DEP/DACS and SFWMD obligations beyond just "facilitating" or "coordinating" roles.	We agree that the local governments and special districts play a very important role in the process and in achieving the TMDL and water storage goals of the CRWPP. The following language was added to the suggested section to highlight this importance: "In addition to these coordinating agencies' roles and responsibilities, local stakeholders are also key in implementing the NEEPP. Local governments, special districts and other stakeholders play an essential role in helping achieve water quality and storage targets through development and implementation of local projects and by providing local information and knowledge that help guide coordinating agency focus and decisions." As defined in 403.4595 F.S. "Coordinating Agencies" are the FDEP, FDACS and the SFWMD. The term "coordinating agencies" is used to be consistent with the NEEPP legislation and does not imply these agencies only play a "coordination" or "facilitation" role. Please also note that this section is intended to provide the reader with an overview of the "Coordinating Agencies'' purview as defined in NEEPP, while more specific agency roles and responsibilities are discussed further in throughout the document (i.e. Pollutant Source Control Program and the Construction Project).
Line 102-103. Regarding the statement, "All of the load reduction was allocated to the categories of National Pollutant Discharge Elimination System (NPDES) municipal separate storm sewer (MS4) permit holders and nonpoint sources." Recommend striking "and nonpoint sources" because, in fact, the NPDES permit holder was allocated the "nonpoint source" load that is above background, including areas that are not under the MS4 permit holders stormwater management jurisdiction, such as ERP residential permitted areas that are presumed compliant under current State stormwater rules.	FDEP is currently working with stakeholders to clarify this comment. This issue needs further discussions through the NPDES program.

Line 103-104. It must be noted: The TMDL addresses permitted entities as stakeholders. It does not address pollutant sources or potential pollution generation entities. The Plan Update should not exclude potential pollutant generating activities in its assessment of the issue. Thus, the Agricultural component should be accounted for in the assessment and has not been addressed.	The TMDL for the estuary included loads from all anthropogenic sources in the tidal portion including those contributed by the agriculture. The Tidal BMAP has loads calculated by local entity based on jurisdictional boundaries and separates out the load that comes from any agricultural activities in the tidal portion. Also in the BMAP, agriculture receives an allocation.
Line 127. (FDEP and SFWMD): There appears to be a lack of continuity in defining agencies from section to section.	It was clarified in this section that FDEP is the lead agency in the BMAP development and that "agencies" refers to the FDEP and the SFWMD. In other sections of the document the term "agencies" is used to mean local agency stakeholders, but this was already clearly stated in the text. No other inconsistencies in the term "agencies" were identified in the document. As defined in 403.4595 F.S. "Coordinating Agencies" are the FDEP, FDACS and the SFWMD.
263/268: Planning Boundary: should also state that the watershed shrinks in the dry season to west of Ortona Lock	The Background Section lists reduced flows to the estuary during the dry season as one of the main ecological problems. The following language was added to this section to further address this comment: “The majority of downstream releases from the S-79 structure to the Tidal Caloosahatchee Sub-watershed occur during the wet season". Also to further illustrate this point, Table 8 showing total annual freshwater flow [106 acre-feet (ac-ft) per year] observed at S-79 by water year and source was broken down by wet vs dry seasons.
Line 385. The FDEP issued the third cycle of the Lee County NPDES MS4 permit September 13, 2011. (Replace previous sentence beginning with "The FDEP is in...")	Correct. This permit has been issued and the language was revised as suggested.
Line 1090. Salinity; Should add a graph showing daily salinity and 30 day average salinity at Fort Myers verses time, showing the 10 and 20 PSU limits, to show MFL compliance, or lack thereof.	Compliance with the MFL for the Caloosahatchee River and Estuary is not an explicit goal of the River Watershed Protection Plan. Please refer to the Lower East Coast and Lower West Coast Water Supply Plans for MFL and associated recovery strategies.

Line 1194. Three fixed sites, CES03, CES04, and CES06, are biased by locating in the dredged navigational channel and do not adequately represent the water quality of the Caloosahatchee Estuary. For example, typically the higher chlorophyll levels are found on the sides of the river not in the middle which typically has the lowest levels. Both the proposed EPA and FDEP numeric nutrient criteria (NNC) rely very heavily upon the CHNEP"s "Coastal Charlotte Harbor Random Stratified Monitoring Network" data. Compliance with NNC should be as statistically robust as the data used to formulate the NNC. It is strongly recommended that the SFWMD resume participation in the random stratified network by resuming sampling and analysis of their agreed upon stratum, the Caloosahatchee Estuary.	This comment raises three issues: (1) a recommendation that the District resume participation the Coastal Charlotte Harbor Random Stratified Monitoring Network , (2) bias in the fixed station sampling that the District uses in the Caloosahatchee Estuary; and (3) use of CHNEP's water quality data in EPA's/FDEP's numeric nutrient criteria formulation and associated compliance. (1) The District evaluated its water quality monitoring network through the Caloosahatchee Estuary re-engineering process in 2009. This evaluation resulted in series of recommendations included but not limited to increasing the number of estuarine stations monitored monthly from 4 to 7 consistent with the recommendation of the 2009 CRWPP Research and Water Quality Monitoring Plan, and dropping the participation in the CHNEP network due to redundancy. The monitoring network from S-79 through the estuary and into San Carlos Bay now consists of thirteen stations sampled monthly. This fixed station network meets several needs of the District: (1) to measure the effects of water management practices by capturing the longitudinal gradient in water quality that results from the mixing of freshwater discharges at S-79 with ocean water; (2) support the estuarine water quality model calibration/verification; and (3) capture long term water quality changes. Stratified random sampling may statistically be a more robust method of analyzing water quality data over long-term and it would be ideal to have both fixed and random stratified sampling networks for answering different questions as the needs arise. This is why the District had supported the CHNEP's stratified random monitoring network for 8 years (2002-2009). However, the District is currently facing significant fiscal challenges; hence we are examining every expenditure to ensure that we can fully carry out our mission to operate the Central & Southern Florida Flood Control Project and move forward on priority restoration projects. (2) This comment also raises the issue of a potential bias introduced by locating water quality monitoring stations in the channel. The District is in the process of evaluating its water quality monitoring programs and would be interested in seeing any analysis that demonstrates such a bias. (3) It should be noted that the proposed Numeric Nutrient Criteria for the Caloosahatchee Estuary submitted to EPA by the FDEP is the TMDL for the Caloosahatchee. The current TMDL is a nutrient load reduction intended to achieve a water clarity goal in the downstream San Carlos Bay. The District has fixed water quality monitoring sites in San Carlos Bay that are sampled monthly.
CRE 45. It should be noted that while Phase I and Phase II were completed, the project land stewardship management plan is still pending.	Thank you. This was noted in table A-1 in Attachment 1.
CRE 142. FY 2011 is over. The conceptual design and is either complete or pending in FY 2012.	This project status was updated in Table A-1 as follows: "Development of the management in corporation with Lee County is complete and the design of the physical improvements will begin in 2012."
CRE 146/148. Powell Creek spelled incorrectly – "Powel."	It was corrected in Table A-1.
p 45 (1252) - using shoot density to determine SAV condition does not equate to SAV productivity. I guess it boils down to the fact that just because the shoot density increases, there may not be a correlation to improved estuarine condition or an increase in SAV acreage.	We agree that increases or decreases in shoot density of SAV do not necessarily provide an accurate measure of SAV productivity or improved estuarine condition. Shoot density is one measure of standing crop. The text in question relates fluctuations in shoot density (hence standing crop of SAV) and its relationship to variation in freshwater discharges. The standing crop of marine SAV decreases at high rates of freshwater discharges.

p 34 (1090) - The salinity issues here are not an "anomaly" (line 1097). The greater periods in MFL violation were during the dry seasons and before the summer rains began. The rain fall was very high but on an annual cycle does not reflect the seasonality of flow in the river. There needs to be graphs added to show the daily and 30-day mean salinity levels with the MFL targets for each.	Agree. The sentence is revised accordingly.
p 150 (4035) -- Talking about the BMP compliance for ag...there is no way that you can get 100% compliance on a voluntary BMP, and I think the short-term compliance of 80% is also unrealistic. Either way, without compliance checks of any sort (which they do not base these estimates on actual compliance rates to begin with) trying to base a plan's success on unrealistic implementation is over-estimating the load reductions. They are fooling themselves at best or fooling the public at worst. Ditto on the urban BMP compliance.	Please note that both the near and long term Urban and Agricultural BMP implementation rates are planning goals and are our best estimate at this time. However, 80% near-term rate only applied to S-4, West and East Sub-watersheds in which the current implementation rates are between 55-76%. The expectation is that in these 3 sub-watersheds where the majority of land use is agriculture, the owner implemented BMPS will reach to 80% in the next 3 years (near term phase). Please refer to the response to comment 2 regarding agricultural BMP compliance under the FDACS program. In addition, revisions to the 40E-61 SFWMD Regulatory Source Control Program required under NEEPP for the Caloosahatchee watershed will advance Best Management Practices implementation (BMP) through: • For agricultural lands, the owner or operator shall implement the BMPs agreed upon in their FDACS Notice of Intent to Implement, or demonstrate compliance with the District regulatory program including monitoring prescribed by the District; • For non-agricultural lands, both implementation of an ongoing program for improvement of existing and development of new best management practices, and adoption of technology-based standards under the District's nutrient source control program are required. Where these standards have been adopted, the owner or operator of a non-agricultural non-point source shall implement these measures; • Performance measures to assess the progress of the collective source control programs in agricultural and non-agricultural lands to affect phosphorus and nitrogen levels in runoff based on monitored water quality data; As indicated in the plan, implementation of the 40E-61 SFWMD Regulatory Source Control Program for the Caloosahatchee watershed is planned to be initiated between 2012 and 2014. BMP compliance mechanisms are described further in the source control chapter of the SFER (Volume I, Chapter 4).
Page 97. Line 2869. Several projects are listed as plan implementation priorities. How were these priorities derived? What selection criteria were used to develop this list of priorities?	The Caloosahatchee River Protection Plan completed in 2009 identified a preferred plan with multiple projects needed to meet water quality and storage goals for Caloosahatchee through an extensive stakeholder process. This 2012 update builds upon that effort and lists the activities/projects planned to be completed or initiated within the next three years (2012-2015). The list captures the on-going efforts as well as the new initiatives. Also, every year, coordinating agencies develop Northern Everglades Annual Work Plan as required by the legislation which identifies the projects and activities to be funded during that Fiscal Year. When new funds are identified, as in the case of $19M for the Caloosahatchee Storage/Treatment initiative, a stakeholder process gets initiated to decide the priority projects to be funded. Each fiscal year, District's strategic planning and budget process also identifies the priority projects to be considered for the GB approval.

General comments: Overall, a great update to the Plan. Many valuable projects put forth. Note: PDF is poor quality with abnormal spacing that makes the document difficult to review. Recommend reformatting the document and allowing additional time for review.	Thank you for your support. As for the quality of the pdf, we believe that the formatting issue with the draft update was due to the line numbers inserted to facilitate the comment process for both the public and for those addressing comments. We apologize for this formatting abnormality and this should not be an issue with the final document.
This is a very important opportunity to suggest that SFWMD reinstitute support for the Coastal Charlotte Harbor Water Monitoring (CCHMN) in the Caloosahatchee R. The CCHMN is an interagency, monthly, probabilistic "random" water sampling program conducted throughout the CHNEP estuaries, including the Caloosahatchee R, since 2002. Originally, SFWMD supported CCHMN sampling in the Tidal Caloosahatchee & Estero Bay, but dropped their support in about 2007 & Lee Co. Environmental Lab has been picking it up since then. However, now we've lost another field crew (FDEP Charlotte Harbor Aquatic Preserves) which puts an even bigger burden on Lee Co Environmental Lab. The CCHMN data that was used to develop the CHNEP Numeric Nutrient Criteria & will be used to evaluate accomplishment of the criteria & is consistent with implementing the CRWPP.	Please see the responses to the comment #15 above.