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

During September–November 2011, these comments from the peer-review panel and public were provided on the 2012 SFER WebBoard (www.sfwmd.gov/webboards). Information by the SFER panelists was prepared under Purchase Orders to the South Florida Water Management District. 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.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 1

Level of Panel Review: Background
Reviewer: J. Burkholder

Posted: 09/14/11 at 12:49 PM by J. Burkholder

The three-pages of Chapter 1

Panelists were asked to comment on the value of Chapter 1 in presenting strong BACKGROUND for the 2012 SFER, which provides status updates and highlighted data summaries for research and monitoring efforts in South Florida for WY2011, along with financial information. This chapter aptly describes this year's SFER as having been considerably streamlined relative to previous years, with strengthened focus on presenting information that [most] clearly relates to reporting requirements. The 2012 SFER has 10 chapters rather than the 12 chapters of previous years, together with supporting information contained in many (stated as 32) appendices.

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.

Appendices

Of six appendices to Chapter 1, four (understandably) will not be available to the Panel for review: Appendix 1-1, Overview of the Volume I Peer Review Process; Appendix 1-2, Peer Review Panel and Public Comments on Draft Volume I; Appendix 1-3, Authors' Responses to Peer Review Panel and Public Comments; and Appendix 5, Everglades Forever Act Annual Financial Report. These appendices clearly will strengthen Chapter 1. Appendices 1-4 and 1-6, which contain excellent supporting information for Chapter 1, are reviewed below:

Appendix1-4: Comprehensive Everglades Restoration Plan [CERP] Annual Report [CERP 470 Report – the District and Florida Dept. of Environmental Protection]

General comments –

This draft appendix, not yet complete, consists of an annual report that is described as "required to provide oversight and accountability for financial commitments…and to record progress in CERP implementation." Parts A and C involve the District, whereas Part B concerns only FDEP: Part A will provide an update on District and FDEP funding appropriations/expenditures related to CERP in FY2011, and a schedule of anticipated expenditures for FY2012; and Part C (Implementation Status), which has been completed, presents an overview of CERP implementation for all projects or components. In the interest of report streamlining, readers are referred to the SFER Consolidated Project Report Database for project-specific details.

Part C includes an excellent "background" description of CERP, which is a conceptual plan that proposes various projects (50 major projects, 68 project components) to reverse decades of ecosystem decline in South Florida. The original cost, $10.9 billion in 2004 dollars, has increased to $13.5 billion in 2009 dollars, due to certain necessary revisions based on finalized designs. Also, several new feasibility studies have been proposed because of an identified need to thoroughly investigate certain regional water resource challenges. The maps are also excellent, and very helpful in showing not only the general locations of CERP projects and components by region, but also various important points of information (whether there are acquired fees or acquired easements; identification of "Save Our Rivers" Projects unacquired lands, expedited project areas, and locations of critical restoration projects). The CERP implementation process is nicely captured in Figure 4.

The tiny print in Table 6 summarizes a remarkable wealth of information about the status of each of the 96 CERP projects and components. A total of 27 of these were actively worked on during this reporting period; 29 more were inactive in WY2011 but have been initiated; and 11 others are being coordinated by other local sponsors. A total of 25 planned projects have not yet been initiated. Among a list of strong achievements and milestones completed in WY2011, about 60% (~243,000 acres) of the estimated lands needed for CERP have been acquired. Although much remains to be done in this effort, the land acquisition to date is an impressive achievement.

Specific comments –

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.

Appendix1-6: The South Florida Water Depth Assessment Tool (SFWDAT)

General approach –

Foremost in District responsibilities is the management of water resources in South Florida for water supply and flood control, a "tall order" indeed for this very large, complex area. The authors of this Appendix stated that water resource management "benefits from the availability of real-time and historical water depth information" – but that seems an understatement: Frankly, it is difficult to fathom how effective water management can occur *unless* such information is in hand. Accordingly, the District's efforts are to be commended in developing the SFWDAT, a critically needed tool that provides water depth information for individual ecosystems and also integrates across ecosystems in near-real time. It supports adaptive management decisions year-round in balancing water supply needs with ecosystem needs. The SFWDAT is described as having resulted in both significant cost savings for data processing, and enhanced communication with stakeholders about water management issues in near-real time.

The SFWDAT was designed to have a relatively simple application framework and methodology that can be automated daily. The SWFDAT integrates the information from hundreds of real-time surface water and groundwater level gauges within District boundaries to produce spatially continuous estimates of mean daily surface water elevations for nine hydrologically distinct basins of the Everglades Protection Area (EPA), Lake Okeechobee, and Pool C of the Kissimmee River floodplain. Basically, water depth surfaces are calculated by subtracting known ground elevations (or gridded elevation models) from these water elevation surfaces. Near-real-time-based outputs include animated side-by-side outputs and static interactive spatial perspectives of water depth and depth-related indices over each of three present implementation regions.

The SFWDAT interpolates water elevation surfaces in relation to land surface (i.e., water depth) from water level gauges through the following process: It acquires data of breakpoint and daily average water levels (from District and USGS time series databases), integrates breakpoint data into daily mean water levels and data quality assessment; and fills temporal gaps based on pointto-point regressions for short-term gaps and a performance-ranked correlation matrix using adjacent gauges for longer-term gaps; and calculates mean daily water depth surfaces by displacing ground elevation from the water elevation surfaces, and calculates depth- and duration-related performance metrics. It produces spatiotemporal graphical, statistical tables and code-based output for Google Earth and web browser-accessible animations.

The relatively simplistic kriging suite of interpolators was selected as the primary interpolation approach that can be applied consistently across the various hydrologically distinct basins. The Everglades Depth Estimation Network (EDEN) tool (which uses a global or across-basins radial basis function), developed recently by the USGS to provide surfaces of water elevation and depths for a subset of basins in the EPA, was also considered, as well as various other integration approaches. However, neither the EDEN nor other integration approaches that were considered performed better, overall, than ordinary kriging in qualitative functional alignment, cross validation, and validation performance. Ordinary kriging also performed fairly well, unlike the other approaches, in capturing the relatively homogenous, flat water elevations of impounded area. Among various regression models that were considered, Gaussian-based variogram (semivariance) models consistently provided the best fit with empirical data and were used in the SFWDAT. These models could also be truncated for more accurate focus on the mean variation of local neighboring gauges instead of longer-range global gradients. Thus, the variogram range for each basin was fit to the mean local variance among gauges, rather than using the range for the overall variance of water elevations for the entire basin. Modeling the variogram for each basin also enables application of the models to spatially characterize the uncertainty (standard error) as a function of the distance from a given monitoring location.

SFWDAT performance was evaluated using a water elevation validation dataset from USGS for Apr-Sept 2007 in WCA-3a, 3b basins of the EPA. There was a very strong correlation between observed and SFWDAT-modeled water elevations with very little bias. Overall, the observed 95% confidence range for the two WCA-3 basins was less than 2 inches, an impressive level of precision. Thus, the interpolated water elevation surfaces produced by the SFWDAT for these two basins showed excellent agreement with observed levels from empirical field data; that is, the SFWDAT yielded relatively low uncertainty in estimates for water elevation. To extend across basins, the SFWDAT uses a gridded ground elevation surface based mostly on spot elevation heights collected at a uniform 400 meters spacing, and data gaps filled with information from LiDAR-based surveys.

Comments – I am not a specialist in this field; I read the Appendix for general understanding/clarity and make the following suggestions in case they are of help.

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.

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.

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.

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.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 2

Level of Panel Review: Background

Reviewer: J. Burkholder

Posted: 09/19/11 at 07:35 AM by J. Burkholder

The Panel was asked to comment on the value of Chapter 2 in presenting strong BACKGROUND for the 2012 SFER. Draft Chapter 2 and its Appendices provide excellent background about the hydrology of South Florida, and the District's water supply management within context. Hydrologic variation fundamentally shapes the ecology and physical features of South Florida, and is critically important in aspects of the District's efforts in water management. This chapter, including seven supporting Appendices, presents a helpful overview of the South Florida regional water management system, and then nicely summarizes hydrology in South Florida during WY2011 at the regional level. The important Summary section is well written, and includes a helpful figure and also an excellent table that compares flows in the Northern and Southern Everglades during WY2011 to historical average flows and to flows during WY2010, which was an unusually wet year because of an El Niño. The remainder of the chapter is also well-conceived.

South Florida is a very "thirsty and demanding" area: The District is confronted with the gargantuan task of making sure that millions of people have enough water, regardless of floods, droughts, hurricanes etc., while also attempting to ensure that sufficient water reaches the ecosystems with both appropriate volume and appropriate timing of delivery. The relatively low gradient of the topography makes pumping essential for moving water in the system. Just this task, alone, is daunting: the average annual pumping volume (Fiscal years 1996-2010) is 2.8 million acre-feet, 0.9 million of which pass through the Everglades Agricultural Area (EAA), and the number of pump stations (60) has tripled since 1996. P.2-9 has the clearest explanation of ENSO events that this reviewer has encountered - excellent writing.

WY2011 was mainly characterized by a La Niña event and a major drought (-12.39 inches of precipitation over the District area); rainfall for most months was less than average throughout the District, but some areas were affected more than others (e.g. Palm Beach, -21.28 inches; lower Kissimmee basin, -6.02 inches). The upper Kissimmee basin, vitally important to the entire District area, had a rainfall deficit of -9.46 inches, and produced volumes at the outflow of Lake Kissimmee that were only 66% of the historical average. Hurricanes contributed much less than the expected 15-20% of total rainfall. Potential evapotranspiration and actual evaporation for year-around-wet features was 15.28 inches higher than the total rainfall. Thus, the drought severity index of the National Integrated Drought Information System shows most of the District area as being in an extreme drought; in fact, in October 2010, drought return periods were as high as 1-in-100 years in some of the regions. Throughout much of WY2011, the District had to operate most water control structures under water supply mode (helpfully depicted in Figure 2-7) because of substantial surface and subsurface storage depletion in both Lake Okeechobee and the Water Conservation Areas (WCAs), which reached critically low levels. Importantly, the authors noted that the frequency of drought appears to be increasing, considering that most WYs in the past decade were drier than the long-term average.

General comment - 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.

Line 38 – by “normal,” do the authors mean “long-term average”? Please clarify.

Figure 2-1 – is an informative map, but the legend should explain the arrows.

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.

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).

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?

Line 484 – should be Alligator Lake

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?

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.

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

Appendices 2-1 through 2-7 [succinct list of contents]

Appendix 2-1 Water Year 2011 Groundwater Levels
Groundwater levels by region
References in chapter - lines 264-265, 432-433, 441-443

Appendix 2-2 Comparison of Water Years 2010 and 2011: Monthly Rainfall and Water Year Potential Evapotranspiration for District Rainfall Areas
WY2011 monthly rainfall, historic average rainfall, WY2010 monthly rainfall, and WY2011 evaporation/potential evapotranspiration and rainfall anomalies for each of the 14 rainfall areas
References in chapter - lines 229-231

Appendix 2-3 Historical Daily Average Water Year 2011 Water Levels
Period of record daily mean water levels (stage) graphs for lakes, impoundments (WCAs), and the ENP
References in chapter – lines 446-447, 479-481, 495-497, 505-507, 515-516, 555-556, 563-565, 573-574, 624-625, 646-647, 775-7756, 806-809, 841, 869-870

Appendix 2-4 Monthly Historical Average and Water Year 2010 and 2011 Water Levels
Comparison of monthly historical average, WY2010, and WY2011 water levels
References in chapter – lines 453-454, 469-470, 648-649, 776-777, 809-810, 841-843, 870-872

Appendix 2-5 Water Year 2011 Monthly Inflows and Outflows
WY2011 monthly inflows and outflows for Lakes Kissimmee, Istokpoga, Lake Okeechobee, and the WCAs; and inflows to the ENP
References in chapter – lines 577-579, 630-631, 653-645, 698-699, lines 708-709, 720-721, 790-791, 821-823, 857-858, 877-878

Appendix 2-6 Monthly Historical Average and Water Year 2010 and 2011 Flows
Monthly historical average, WY2010, and WY2011 inflows and outflows for Lakes Kissimmee, Istokpoga, and Okeechobee; and similar comparative information for inflows to the ENP
References in chapter – lines 579-580, 630-631, 654-655, 699-700, 721-722, 792-793, 823-825, 858-859, and 878-880

Appendix 2-7 Temporary Modifications in Regulation Schedules in Water Year 2011
As stated in the title. Note: This Appendix is very well-designed; it provides a clear, detailed synopsis of this information, including instructive figures.
References in chapter – lines 372-374, 484-485, 503-504, 514-515, 552-553, 562-563

Other information referenced in draft Chapter 2

2007 SFER, Volume I, Appendix 2-2
Stage-storage for major lakes and impoundments, and stage-area relationships where data are available (chapter, lines 158-160, 455-457)

2007 SFER, Volume I, Appendix 2-6
Regulation schedules for the lakes and WCAs (chapter, lines 368-369, 481-482)

Posted: 09/15/11 at 11:57 AM - Public Comment, Collier County

From: Nath, Ananta
Sent: Thursday, September 15, 2011 11:57 AM
To: Abtew, Wossenu; Pathak, Chandra; Ciuca, Violeta
Cc: Tears, Clarence; 'Gilpin-Hudson, David'
Subject: FW: 2012 South Florida Environmental Report

Lady and gentleman –

I'm forwarding below a comment on the draft SFER 2012 (your chapter on regional hydrology) from Mr. Robert Wiley of Collier County. Robert indicated that he is not tech savvy to post the comment on the web board, and requested me to communicate to you.

I do agree, the regional hydrology report has missed the LWC area south of the Caloosahatchee watershed. There are plenty of data in DBHYDRO (e.g. canal outflow data of the Golden Gate, Faka Union, Cocohatchee, Henderson Creek and Barron River etc Canals) for inclusion in Figure 2-1 and Table 2-1. Please let me know if you need any information from us. Thanks, and keep up the good work.

Regards,

Ananta

Ananta Nath, P.E, D.WRE
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Big Cypress Basin
South Florida Water Management District
2660 Horseshoe Drive N
Naples, FL 34104
Phone: 239-263-7615 Ext 7607
Fax: 239-263-8166

From: WileyRobert [mailto:RobertWiley@colliergov.net]
Sent: Thursday, September 15, 2011 10:46 AM
To: Nath, Ananta
Cc: LorenzWilliam; FrenchJames
Subject: 2012 South Florida Environmental Report

Ananta,

I just reviewed the 8-18-11 DRAFT of the 2012 South Florida Environmental Report (Chapter 2: Regional Hydrology) prepared by Wossenu Abtew, Chandra Pathak and Violeta Ciuca. Where is the readily available hydrology information from the Big Cypress Basin? All that I find in the report is the following statement: *The Lower West Coast includes large areas outside the drainage basin of the Caloosahatchee River.*

It appears that this report should be titled more along the line of the "Okeechobee Basin Environmental Report" since it seems to only deal with issues related to areas of inflow to and discharge from Lake Okeechobee. Even that information is pared down to just a few of the major system components.

Can the report be revised and expanded to provide a thoroughly detailed hydrology report for the entire South Florida Water Management District? This type of document could be a very useful reference if it was complete.

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PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3A

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

Posted: 09/12/11 at 08:25 AM by V. Novotny

The chapter is a pretty straight forward presentation of water quality data in the EPA and is, in general, written well (if dryly) and well organized. It is encouraging to note that the only parameters not meeting Class III criteria are those that probably have less direct influence on the ecological function of the EPA and may be out of compliance due to naturally induced deviations to generic numeric criteria (e.g. pH, alkalinity and EC).

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.

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.

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.

Specific Comments

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, 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).

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).

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?

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 exceedences 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.

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.

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.

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.

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.

Page 17 – Alkalinity. This reviewer agrees with the report that possible excursions of alkalinity are not of a major concern, they may be natural. Similarly occasional lower pH is most likely natural.

Page 18 – Specific conductivity. The situation with salinity excursions, most likely caused by salt ground water intrusions, has not changed from the last report. Because salt water underlying the fresh water aquifer in Florida is in relatively small depths, digging canals and discharging may have intercepted the salt water and caused the salt water discharge. Figure 3A-6 shows a decreasing salinity trend and the only problem with higher salinity is in WCA 2. In the other three areas, the interior salinity is normal.

Page 19 – Sulfate. This reviewer will address the sulfate issue in his review of Chapter 3B (“A” review). There is no sulfate standard and the issue is a potential connection between sulfates and formation of highly toxic methyl-mercury. Fish and other organisms residing in the Everglades system have very high mercury tissue content.

Page 22 – Pesticides. Only atrazine was identified as a pesticide of concern which follows the findings from previous years.

Pages 24 - 37 Phosphorus. Phosphorus has been recognized as a limiting nutrient for the Everglades (EPA). To protect the biota and limit potential eutrophication a criterion of 10 µg/L of Total Phosphorus was designated for the areas. The section on phosphorus presents the trends of long term geometric averages of TP in the various locations of the EPA water bodies. These long term averages were influenced in the water years 2005 - 2010 by extreme meteorological events from which the system may still be recovering. The plots generally show a downward trend from the highs in the1980s and in the Park interior the geometric means are already solidly meeting the 10 µg/L standard. Apparently in WT 2011 TP concentration in the interiors of all areas were meeting the standard, which is a significant improvement over the past years. However, maximal concentrations are still very high in some areas and the ranges indicate high fluctuations especially in the inflows. The impacted (i.e., phosphorus-enriched) portions exceeded the criteria. In all cases the annual network geometric mean TP concentrations for WY2011 in both the impacted and unimpacted portions of all three conservation areas were the lowest of the five-year assessment period.

Maximum concentrations of the TP in the inflows to the Refuge area and WC2 in hundreds of μg/L indicate a local impact that can be serious. Localized cyanobacteria infestation and algal bloom conditions have been observed in the past years throughout the EPA outside of the Park but not reported for WY 2011.

The long term TP loads to all areas except the Park are continuing the decreasing trend and because the concentrations of TP in the Park are below the criterion, the steady load to the Park is assimilated by the system and could be at the natural level. 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.

P. 37-39 Total nitrogen

Nitrogen is not a limiting nutrient but common sense dictates that both TP and TN loads should be reduced to protect the Everglades protected areas. Generally, most BMPs reduce both N and P. The reports suggested that one of the main sources of N, in addition to the agricultural inputs and Lake Okeechobee, is the organic matter produced naturally in the EPA wetlands. The TN concentrations within the entire system decreased from north to south and ranged between 3 mg/L in the north and around 1mg/L in the Park. These concentrations are high.

Specific questions and comments by line and figure number

224: What do the fatal qualifiers represent?

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.

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

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

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*?

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.

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).

Editorial page and line comments

187-192: This long sentence is very difficult to decipher.

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

Comments on Appendices

Six appendices are attached to the chapter. Appendix A3-1 reports detailed monitoring results for water quality; Appendix 3A-2 presents water quality monitoring results at individual stations; Appendix 3A-3 reports attainment of the dissolved oxygen site specific criteria at individual stations; Appendix 3A- 4 contains total phosphorus concentrations at individual stations; Appendix 3A-5 includes annual flows and total phosphorus loads; and Appendix 6 presents annual total phosphorus criteria compliance. All appendices were summarized in the text of the chapter.

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

Closing comments

This chapter is well written and is better, both in the content and style, than the last year chapter. Because of the international importance of the EPA it is appreciated that the writer used in general, the international system of units.

The water quality of the WY 2011 is much better than that in the previous year and the trend is towards further improvement. The questions that should be discussed are:

- Are the results of WY 2011 a reflection of a continuing trend or a statistical outlier because of the lower flows into the system?
- The phosphorus loads from the Lake Okeechobee and other inflows are still high, the lake is (Chapter 8) overloaded with phosphorus and not healthy and in the future it may impact adversely the EPA system.
- The sulfate load and concentration that would not be a concern in most other water bodies are a concern because of the high mercury tissue content in fish, alligators and other prey animals. This issue will be discussed in the review of Chapter 3b.
- The SFWMD must be vigilant regarding the low DOs in the system. It is recognized that Florida and specifically EPA wetlands are generally dystrophic which is reflected in the site specific DO standard; hence, excessive excursions of the DO standard may lead to fish kills.
- The high maximal TP concentrations in the Refuge and WCA-2 zones are of concern and should be a focus of more intensive abatement of external and internal loads.

- The DO Site Specific Criterion (Standard) allows lethal concentrations to pass. Admitably, for most for parts of EPA these low concentrations and fluctuations may be natural but not all of the assessed area. The conflict between the federal DO criteria which provide full protection and the Florida's EPA site specific criteria which allow lethal concentrations must be resolved. This problem was pointed in the last year review.

Posted: 10/21/11 at 12:55 AM by V. Novotny

The chapter is a straight forward presentation of water quality data in the EPA and is, in general, well written and well organized. The reviewers recognize the limitations that the authors had to stay within the regulative boundaries of the SFWMD and Florida DEP criteria and guidelines and not to venture into debates that would go outside of these limits. On the other hand, the reviewers can and were required to have a broader outlook and point out issues that may not be in conformity with the local agencies' guidelines and established norms. In a majority of cases the authors satisfactorily answered reviewers' comments and suggestions and agreed to include more information where appropriate. Therefore, the final reviewers' comments will focus on outstanding issues and clarifications.

Comment #1:

The map will be much appreciated. The authors should either use one consistent name for the WCA 1 and/or Loxahatchee Wildlife Refuge throughout the chapter, or specifically note they are the same thing to avoid confusion.

Comment #2:

There are additional inconsistencies for reported TP concentrations (not just the typo on line 610). The authors need to be more diligent in finding these. For example, compare data in Figures 3A-9 and 3A-10 to values in Table 3A-4. Several lines representing the geometric mean do not match the values reported in the table. Either one or the other is wrong, or these are different data sets. If the latter is true, this is not apparent from the supporting text or from the captions on said tables and figures.

Comment #3:

The response raises more questions in the reviewer's mind than the original comment. Table 3A-5 and the supporting section are dedicated to measured OP concentrations. For example the table shows 253 data points are included for inflow to the Refuge with a geometric mean of 5.3 and minimum and maximum values or 1 and 140 micrograms/L, respectively. Why would this be the case if 70% (or more) of the OP samples were below the detection limit? The original comment addressed whether these data had been compared to the analogous TP data reported in

Table 3A-6 under the belief that this analysis might be useful. But now we do not understand the significance of the OP data at all.

Comment #4 –

The authors should include the text of the response on low DO and past exceedances of the criteria into the chapter. It should be pointed out that the DO variations in the lower ENP are either natural or a result of nutrient enrichment caused by phosphorus with ensuing increase DO daily fluctuations.

Comments 10 and 11 - see the reviewers comment on the issue of DO standards in the paragraph pertaining to Comment #22

Comment # 12 – The reviewers have no objections to the fact that SFWMD in its internal reports is using older US units. However, this report is being put into public domain and will receive national and international attention. Outside of the SFWMD the world is now metric, all US government documents and literature articles have to be prepared and published with SI units and, if desired, with US units in parentheses. Most of the other chapters of the report adhere to this principle and so is most of this chapter. The reviewers appreciate the authors' effort to use footnotes and conversions and hope that the use of the units will be more consistent in the future annual reports.

Comment #13:

The response makes sense, but this should be noted in the Chapter.

Comment #14:

The initial intent was to suggest alternative conclusions that *could be* true based on the reported data, not necessarily that it was a *better* explanation. However, the authors' response does not convince the reviewer that the proposed alternative is valid. Significant sulfate reduction does not occur in the water column where DO is measured but in the underlying sediments, which are likely anoxic no matter what the DO concentration of the overlying water is. In this likely scenario, sulfate reduction is controlled more by the availability of sulfate rather than by the inhibition due to the presence of oxygen. So more sulfate= more sulfate reduction. The main point of the comment however, was that this possible explanation for the sulfate gradient (even if only a partial explanation) may have important ramifications for the focus of chapter 3B, namely the mercury methylation problem.

Comment# 16:

A reference to previous SFER is not found anywhere near where these criteria are discussed. However a better approach would be to include the response directly in this year's chapter so that the significance of the section is more obvious.

Comment#17:

Again, it would be beneficial to explain how impacted and unimpacted sites where determined in the chapter. More importantly, the statement that an impacted site can be designated unimpacted if water quality parameters improve but that an unimpacted site could not become impacted if water quality decreased defies logic.

Comment #18:

Agreed, but this does not negate the alternative hypothesis associated with comment #14. The authors satisfactorily addressed comments: #15, #19, #20, #21, 23.

Comment # 22:

As stated in the preamble to the final comments, the reviewers recognize the fact that the authors must adhere to the current standards and guidelines and not to get involved in the debate on the validity or potential deficiencies of these standards that were imposed on SFWMD by the state and federal regulatory agencies, namely, Florida and US EPAs. The reviewers recognize also the differences of the dystrophic Everglades and similar wetlands from a typical lake or a stream. However, the reviewers have a responsibility to point out the problems with a standard that does not provide a protection to the indigenous biota nor provide conditions for maintenance of a healthy, well balanced population of fish (wildlife is fine with the low DO). Fish kills have been reported in the Everglades. As a way of example, literature on DO toxicity documents that the Everglades' trophy fish, largemouth bass (LMB), avoids areas where DO is less than 3 mg/l, and LMB juveniles are killed after several hours (3 hours to be specific) exposure to DO$\leq$ 2.5 mg/L. So, our comment is not particularly aimed at the authors, it is directed towards the agencies that developed and/or accepted the formula and its application to averaged DO measurements. The appendices are exhaustive and comprehensive. All appendices were summarized in the text of the chapter.

Summary Bullets

- Are the results of WY 2011 a reflection of a continuing trend or a statistical outlier because of the lower flows into the system?
- The phosphorus loads from the Lake Okeechobee and other inflows are still high, the lake is (Chapter 8) overloaded with phosphorus and not healthy and in the future it may impact adversely the EPA system.
- The sulfate load and concentration that would not be a concern in most other water bodies are a concern because of the high mercury tissue content in fish, alligators and other prey animals. This issue is discussed in the review of Chapter 3b.
- The SFWMD must be vigilant regarding the low DOs in the system. It is recognized that Florida and specifically EPA wetlands are generally dystrophic, which is reflected in the site specific DO standard; hence, excessive excursions of the DO standard may lead to fish kills.
- The high maximal TP concentrations in the Refuge and WCA-2 zones are of concern and should be a focus of more intensive abatement of external and internal loads. The link between TP and other important symptomatic parameters such as DO, DOC which may also affect methyl mercury should be pointed out.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 3B

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

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

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

Broad questions and comments that should be addressed

This chapter deals with the serious problem of high and unacceptable 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.

The report, as in the past years, attributes the methyl mercury formation and tissue contamination to (1) inputs of mercury from several sources into the EPA system, and (2) chemical sulfate (SO_4^{2-}) reduction without significant sulfide (S^{2-}) formation. 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.

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 anaerobic 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.

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.

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.

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

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

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.

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.

Specific questions and comments by line number

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

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.

110: “Length adjusted” implies fish length or time length?

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.

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.

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 methanogens and SRB.

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.

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.

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

688-696: A methodology for how “new” and “old” mercury is distinguished should be provided. Was it by isotope ratio analysis?

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.

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

Figure and Table comments

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

Table 3b-2: Please provide units for Mean THg

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? In figure 4 (and other locations within the document) is “the refuge” the same thing as WCA 1? If so please be consistent.

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.

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

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

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.

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

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.

Editorial page and line comments

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

115 ~~In~~ The

116: *The* rational*e*

201: Dunn*'s* test

202: ignore*d*

408: Capitalize *Wet*

769: It is *NOT* the acidity per se

787: micromoles not *micrometers*

801: Deltaprotobacteria g*roup*?

Summary and Recommendation

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.

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.

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.

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.

Posted: 10/21/11 at 08:41 PM by O. Stein

The chapter is well written by a team of experts from SWWMD, federal agencies and laboratories and universities. The methyl mercury problem although known and studied since 1960's is a complex biogeochemical problem and, in spite of the seriousness of the problem and many years of studying, new knowledge is being discovered. The team of experts preparing the chapter and response to the reviewers' comments are highly knowledgeable about the topic, have collected a superior data base and contribute significantly, year after year, to the state of the art of this difficult issue, in general and for the Everglades ecological system in particular. The authors prepared an exhaustive response document and the reviewers are pleased that the authors agreed to most comments provided to them and found a common ground.

Both the reviewer's comments and author's responses to the draft of Chapter 3B, presented a thorough and scientific discussion of issues and suggestions. When the reviewers asked for "explanation" the authors provided full and detailed responses. Due to the large length of the review and response documents, in the following closure the reviewers will focus on the issues where there were differences or additional recommendations. If a comment is not specifically numbered below, the reviewers believe the authors' response satisfactorily addressed the original comment.

Comment#2: The suggested replacement text is a vast improvement. The reviewer's comment did not imply that current sulfur additions to the EAA are the primary cause of elevated sulfate levels in the EPA, but that EAA region is a primary contributor without implying a specific mechanism.

Comment #3: The reviewers appreciate the authors' detailed response to the queries. The authors' command of not only the big-picture science but also the details of the driving mechanisms is impressive. Clearly Hg methylation does not require an independent source of methyl groups and the issue of DOC concentration and type may have an important influence on HgS complexation, e.g. Comment #11, especially at low concentrations of S^{-2}. However, the comment that the optimal concentration for methylation varies by an approximate factor of 4 between the ENP and WCA sites certainly indicates that the driving mechanisms are still elusive.

Perhaps the statement that 1-5 mg/L SO_4 is borderline to natural backgrounds is a stretch, but no more so than the hope that concentrations of approximately one order of magnitude less will ever be achieved in the foreseeable future. The comment's point was to suggest that achieving reductions in Hg methylation by reducing sulfate concentrations will be an extremely challenging proposition, as acknowledged by both reviewers and authors in other comment responses.

Comment #4: Excellent response.

Comment#5: The author's response on the reductions of local Hg sources and new modeling results corroborating these data is noted. The reviewers still question the use of the term "international" to represent atmospheric sources not originating in Florida. Is there any proof that the majority of the mercury comes from sources outside of the United States? If not, the use of the term is misleading.

Comment#6: The reviewers were certainly not implying that we should increase sulfate loading to push sulfide concentrations above an optimum for Hg methylation. On the contrary, we acknowledge (and stated, see comment#40) that reducing sulfate loading will provide environmental benefits exclusive of the Hg methylation concerns. Our point was that the District and its partners need to be aware that an aggressive program to reduce sulfate loading may have unintended results that might actually be in direct opposition to intended outcomes. The authors clearly acknowledge this, but seem to be downplaying its potential significance somewhat.

Comment#21: Best to add this information in a footnote, to avoid the appearance of missing data.

Comment#23: See our response to the usage of "international" (Comment#5). As for the comment on plant assemblage modification (and appurtenant microbial assemblages), this is happening on a large scale throughout the EPA, either passively via exotic species expansion, or actively via exotic eradication, and especially by the vegetation modifications within, and expansion, of the STAs. Plant modifications are also occurring in Kissimmee restoration region and potentially in the EAA. Considering all these activities, at least a mention of the possible impacts of plant and microbial modifications on DOC type and concentration in a generic process diagram seems warranted.

Comment#26: The comment should have said "there are *relatively* few data points…" to be more precise. The reviewers agree that there may be mechanistic and supporting data suggesting these optimal ranges, but it is hard to see where the data in Figure 3B-12 supports the suggested ranges. All high MeHg concentration values are within those ranges, but it still appears that relatively few points lie beyond the specified ranges (it is hard to see what values are less than 1 mg/L sulfate). More importantly, there appears to be as many (or more) low values than high values within the specified ranges. What the data in this figure suggests to us is that DOC is typically between 5 and 20 mg/L and sulfate between 0 and 10 mg/L, with (apparently) little correlation to methylation. Additionally, we did not imply using only a simple linear regression for a comparison between methylation versus sulfate concentration. However, it appears from the data presented in this figure that a simple scatter plot of these two variables would show little correlation of any type, but of course the only way to know for sure would be to plot it.

Comment #28: This comment was meant to reinforce the supposition of comment #26. The supporting data in this figure does *not* reinforce the supposition that the optimal range is 1-5 mg/L. Note that the reviewers are not implying that the optimal range could not be 1-5 mg/L sulfate in the ENP region, just that the data in Figure 3B-12 do not have the appropriate ranges to demonstrate any optimal range.

Comment#40: The reviewers and authors agree that the influence of DO or nitrate on Hg methylation is hypothetical (yet worthy of research, especially in region where the optimal range may be 1-5 mg/L SO_4). While adding nitrate is unrealistic and undesirable for other reasons, DO can be increased by reducing phosphate loads, which is a goal of the Everglades management plan. The proposed modeling research should focus on the effect of reduced P in the critical zones on DO and subsequent potential reduction of Hg methylation.

Comment#41: The modeling research should include a simultaneous effect of phosphorus and DOC on DO concentration and the symbiotic effect of DO and sulfate on HG methylation, if such a model is available or can be developed.

Recommendations (SOW Task 3)

- The team and subcontractors working on the MeHg problem should continue research searching for a better causative model describing quantitatively the nonlinear relationship between the input Hg, sulfate concentrations and simultaneous effects of phosphate on DOC and DO and their effect on methylation. The optimum DOC observed for methylation is around 10 – 20 mg/l, which is quite high and may imply eutrophication and nutrient enrichment and be a cause of anaerobic conditions favoring methylation in the sediment.
- For the development of the model, further studies are needed to better understand what forms of DOC are most affecting methylation.
- The effect of the oxygenated upper sediment layer (that can be one to several cm thick) on the transfer of MeHg from the lower anaerobic sediments into water column should be investigated. In other words, is an oxygenated layer a barrier to the MeHg flux from the sediment to water and from water to biota?
- A study on the optimum additions of sulfur to soils is needed which would also address whether or not an effective soil oxidizing substitute for sulfur can be found which would not stimulate mercury methylation in the Everglades National Park.
- There is need to quantitatively assess by modeling the effects of reduced high sulfate concentrations that could increase the area of the critical methylation zone, i.e. zone where sulfate concentrations are between 1 to about 10 mg/l.
- Research should continue on finding the sources of the Hg input from the atmosphere, soil, legacy contamination, etc.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 4

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

Posted: 09/12/11 at 03:43 PM by V. Novotny

Summary

This chapter presents an extensive review of the structural activities, Best Management Practices development, permitting rule development and other measures to achieve the NEEPP and EFA goals. These programs are adaptive, i.e., past and recent experience is incorporated into the new rule development and continuing and new research. Because the chapter has multiple authors, there are some redundancies and repetitions, acronyms are not defined when they first appear, and some minor inconsistencies with mixing US and international units exists. There are also significant differences between the efforts in Northern Everglades watersheds and the Southern EPA watersheds. In the northern watershed draining into Lake Okeechobee and the Caloosahatchee and St. Lucie Rivers the voluntary compliance with the BMPs programs and programs that are not regulated by NPDES permits is still insufficient and participation must be accelerated to meet the deadline of compliance with the EFA and NEEPP goals. Overall, the EPA water quality abatement and restoration is an excellent, remarkable effort, the most ambitious water quality effort anywhere in the world.

The chapter provides a defensible account of data and findings, with a logical synthesis and findings that are clearly linked to management goals and objectives. This chapter was generally well written with excellent, helpful maps. The authors nicely explained the regulatory requirements (and innate restrictions), and the complex interactions of various entities who share responsibilities with the District in efforts to develop effective nutrient source control programs. The summaries of activities in WY2011, and of activities planned for WY2012, were also very helpful.

The draft chapter and four excellent accompanying appendices also clearly convey that the District has shown appropriate accountability to the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA) in its actions to develop effective source control programs for the Northern Everglades and Estuaries and the Southern Estuaries, respectively. Many examples of sound accountability occur throughout the chapter. For example, in response to requirements set forth in legislation, the District (p.4-23) initiated a phased approach to develop source control programs in the Caloosahatchee and St. Lucie River watersheds, including (1) assessment of the existing monitoring network and historic data for measuring program performance; and (2) developing performance measures (PMs) to set benchmarks to evaluate program effectiveness. As another example, the District has ensured that data collection efforts in WY2011 were consistent with legislated requirements. The District has also shown attentiveness in avoiding duplication of effort and streamlining for increased program efficiency. There is a clear recognition that consistent, holistic approaches are needed for source control programs in the major watersheds, while also considering the unique source control issues that affect each sub-watershed. The identified strategies for moving forward seem logical and sound, and it is hoped that these strategies can move forward with adequate funding support.

Introduction

This chapter provides an overview of the source control programs in the major watersheds of South Florida and also describes source control issues of each sub-watershed which are included in the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA). For the Northern Everglades, the source control program is planned and implemented into the multiple programs for Lake Okeechobee (see also Chapter 8), Caloosahatchee River, and St. Lucie River Watershed Protection Plan. The plan for Lake Okeechobee was completed in 2008 and plans for the two rivers were finished in 2009. These plans are being updated every three years and WY 2011 is the year of the update. The goal of the plans is to minimize the undesirable flows to the estuaries and improve the quality of water delivered to Lake Okeechobee and the estuaries through source control programs, construction research projects, and water quality monitoring. For the Southern Everglades, the source control program planning is incorporated into the Long-Term Plan for Achieving Water Quality Goals in the Everglades Protection Area (see Chapter 5). The strategy includes implementation of BMPs for phosphorus reduction, (2) regulatory programs, (3) voluntary programs, (4) educational programs, and (5) integration with local and regional water quality projects.

The draft Chapter 4 describes District efforts (with a network of partners) in nutrient source control programs, which are required to include implementation of regulations and best management practices (BMP) development; implementation of improved BMPs) improvement and restoration of the hydrologic function; use of alternative technologies; and a water quality monitoring counterpart. The watershed source control programs should also have performance measures (PMs) that ensure consistent implementation of BMPs, measure actual reductions, and have a mechanism for triggering improvements if the water quality goals are not achieved.

Major Points

The source control programs, with the exception of the urban stormwater, are built on voluntary participation of dischargers, which has its limits as the results in some watersheds indicate. The results of the abatement also varies year to year, based on the hydrologic and meteorological (hurricanes) conditions. Apparently, WY 2011 year was a low flow period with no major catastrophic and disruptive meteorological events. Consequently, the watershed loads were on the lower side and not all of the load reduction can be attributed to the implementation of source controls. Correspondingly, the phosphorus concentrations throughout the Everglades were lower than in the most of the preceding years (see Chapter 3A). It appears that during the WY 2011 drought was more responsible for significant reductions of nutrient loads and their concentrations within the Everglades system. The deadline for meeting the goal of the plan is very near (2015) and the trend in some areas is not sufficient to guarantee meeting the goals.

The chapter describes the programs but not the technical details.

Northern Everglades –

River Watershed Pollutant Control Plans have been developed for the Caloosahatchee River and the St. Lucie River. These plans are required to be updated at three-year intervals, and both are being updated in this year's SFER. The main goals of these plans are to minimize undesirable flows to the estuaries while also improving the quality of water delivered to Lake Okeechobee and the estuaries. The draft Chapter 4 (lines 33-36) refers readers to Appendices of Chapter 10 (Coastal Estuaries) for the two plans, and to Chapters 8 and 10 for further information.

Page 3 – Nutrient control highlights - Lake Okeechobee (see also Chapter 8). This section describes the progress on development of the monitoring program. Of note is the effort to include precipitation in the development of the performance metrics that would ensure consistent implementation of BMPs, measure actual reduction achieved, and have a mechanism for triggering improvements should the water quality goals not be achieved. The first required three-year update of the Lake Okeechobee Protection Plan was completed in March 2011. The draft chapter highlights progress since March, including (i) development of a data sharing process for tracking progress of landowner participation/ implementation of agricultural BMPs; (ii) an analysis of historical flow and water quality data, as the initial step in developing a baseline period and performance measures (PMs) for each sub-watershed; and (iii) a PM of rainfall variability. Appendix 4-1 provides more information about evaluations for regulatory source control programs in this watershed.

Caloosahatchee River and St. Lucie River Watersheds

A synoptic monitoring program and development of performance metrics were completed for the Caloosahatchee River and initiated in the St. Lucie watershed. In both watersheds the district performed an evaluation of existing watershed monitoring networks resulting in recommendations of proposed modifications to the monitoring and completed an analysis of historic data. For the Caloosahatchee River watershed in WY2011, the synoptic monitoring program from the June-Oct 2009 wet season for freshwater river tributaries was analyzed; an inventory and compilation of TN and P data was developed; and existing monitoring programs in the watershed were evaluated. District efforts described in the draft chapter mainly focus on implementation of regulation, BMP elements of source controls, and work on an amendment to the Environmental Resource Permit (ERP) Program called the Statewide Stormwater Rule. Appendix 4-4 provides more information about source control programs in both the Caloosahatchee and St. Lucie River watersheds. For the St. Lucie River watershed, in WY2011 the District began a synoptic monitoring program for P and N in the WY2011 wet season for the C-23 and C-24 basins (which contribute 60% of the TP load); began to develop PMs at the watershed and sub-watershed levels; and evaluated existing monitoring networks in the watershed.

Everglades National Park

In WY 2011 significant reduction of phosphorus loads and concentrations within the park have been measured.

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.

Pages 6-13 is a subchapter providing overview of the Northern Everglades source controls. Watersheds of the Lake Okeechobee, Caloosahatchee River, and St. Lucie River were identified in the NEEPP as critical water resources that were in the past adversely modified by land use changes and drainage. The NEEPP includes a phased, comprehensive, and innovative protection program that included construction projects, source control programs, and research and water quality monitoring programs. The source control programs are carried out by the SFWMD, Florida Department of Environmental Protection (FDEP), and Florida Department of Agriculture and Consumer Services. The University of Florida is responsible for the development of effective yard fertilizer controls. The pollutants of concern are phosphorus for Lake Okeechobee and both phosphorus and nitrogen for the estuaries of the two rivers. The list of the source control measures and responsible agencies is included in Table 4.2.

Pages 13 -35 – Status of Source Controls in the Northern Everglades watershed. The approach consisting of (1) implementation of regulations and BMP development, (2) implementation of improved BMPs, (3) improvement and restoration of the hydrologic function of natural and managed systems, and (4) utilization of alternative technologies for nutrient reduction at the source, is sound and logical. The programs and the status for Lake Okeechobee are also described in Chapter 8.

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%.*

Page 15 - FDACS has developed and adopted BMP manuals for most agricultural commodities in the Northern Everglades. Because application of BMPs by farmers is voluntary about 77 % are enrolled which, if it was not for Everglades watersheds, would be considered as reasonably successful. However, because the nutrient water quality goals have not been met in Lake Okeechobee, further efforts to include more agricultural acreage and developing higher efficiency BMPs must continue and expand. In the Caloosahatchee River, and St. Lucie River watersheds (page 17) extensive multifaceted BMP development and implementation programs have been initiated. The permitting program regulates activities involving the alteration of surface water flows, including activities in uplands, wetlands, and other surface waters but it does not include BMs. 38 % of acreage in the Caloosahatchee River, and 68% in the St. Lucie River watershed, respectively, are included in the ERP permitting program (Table 4-6).

Pages 20-21 document that a fully functioning monitoring program needed to assess the needs and compliance of the source programs in the Caloosahatchee River and St. Lucie River are not fully nor satisfactorily developed. The work on enlargement and intensification of the monitoring programs is ongoing. An assessment of historic TP, TN, inorganic nitrogen, and flow tributary data for the Caloosahatchee and St Lucie watersheds has been completed. Development of performance measure methods is being coordinated with protection plan data and model assessments on the estimated performance of the preferred projects and programs of the river watershed protection plans. The source programs are in the implementation phases. The district is now working rapidly to implement the BMPs to achieve the objectives of the river and lake protection plans. Specific elements that need to be incorporated encompassing the Northern Everglades watersheds, include the following:

- For agricultural lands, areas where agricultural nonpoint source BMPs, or interim measures have been adopted, the owner or operator shall implement these measures or demonstrate compliance with the District's nutrient source control program including monitoring prescribed by the District.
- For non-agricultural lands, both (1) implementation of an ongoing program for improvement of existing and development of new BMPs, 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 nonpoint sources shall implement these measures. These measures will be implemented in a phase-in approach. Currently only 43% of agricultural land enrolled in the BMP plan in the Caloosahatchee and 52 % in the St. Lucia watersheds (Tables 4.7 and 4.8).

The nutrient reduction programs by the Florida Department of Agriculture and Consumer Services (FDACS) described on pages 24-27. These programs, with the exception of the animal manure application rule, are mostly voluntary. FDACS included training, education and demonstration programs which were not described in the programs by SFWMD. The agency collaborates with the University of Florida to develop the balanced crop nutrient use limits to minimize nutrient losses from farms.

The urban turf fertilizer application rule was enacted in 2007 (p.27) and is enforced at the market place. The expected reduction of phosphorus and nitrogen loads from urban lawns is very modest and most likely uncontrollable at the point of use by individual homeowners, who per land unit area, use far more fertilizer than farmers. Beginning January 1, 2014, certification will be required for any person applying commercial fertilizer to an urban landscape. The agency promotes three new programs: which educate homeowners, builders, and developers about how to design, install, and maintain Florida-friendly landscapes, and Green Industries BMP Program, which trains and certifies landscape professionals (page 32). The landscaping initiatives also include golf courses.

Discharges of runoff and wastewater flows from concentrated animal feeding operations are carried out under the National Pollution Discharge Elimination System (NPDES) permitting. Feedlots are statutory point sources, requiring permitting. NPDES permits are also regulating nonagricultural point discharges (Table 4-11 on p. 30) and, less rigidly, urban and highway runoff (p. 31). Urban point discharges are a significant source of phosphorus and nitrogen and NPDES permitting is a very efficient tool to reduce the P and N loads. However, these permits must be water quality limited Clean Water Act Section 303(d) derived limitation requiring TMDLs. TMDLs can also be used for controlling nonpoint sources but implementing such control is more difficult. Only Lake Okeechobee has a TMDL base derived limit and load restrictions (see Chapter 8). The agencies involved in the abatement should use more TMDL derived water quality based standards and plans.

ECP Basins –

The Everglades Agricultural Area (EAA) and C-139 basins contribute ~88% of the P discharged to the Everglades Protection Area (EPA). Other smaller contributors are the C-51 West and L-8 basins. Appendix 4-2 provides additional information about P source control programs in these basins.

Page 35 – briefly describes the source control programs in Southern Everglades based on the Everglades Forever Act. The goal of the Everglades is to achieve the water quality limit in the EPA of 10 μg/l of total P. The EFA mandates specific performance levels for controlling phosphorus in discharges from the Everglades Construction Project (ECP) basins, Everglades Agricultural Area (EAA) and C-139 basins in the Southern Everglades. To ensure compliance the District must comply with specific source control permit requirements which incorporate a comprehensive approach for controlling phosphorus, including implementation of source controls through the utilization of regulatory, cooperative and educational programs. For the EAA and C-139 basins, the EFA mandates a regulatory source control program to implement BMPs to control phosphorus at the source and a monitoring program to assess program effectiveness; hence, the programs are not voluntary. The BMP plans are comprehensive, generally consisting of nutrient management, water management, and sediment controls. Ongoing research is being conducted to improve BMP effectiveness and design of BMPs. Some BMPs listed in other chapters are not efficient.

EAA Basin – The District is required to monitor and report on the progress of the Everglades P source control strategy for discharges from these basins. An overall goal has been set to reduce TP by 25% compared to the baseline. **Notably, during WY2011, the TP loads discharged from the EAA Basin decreased by ~79%** compared to the predicted load from the pre-BMP baseline period adjusted for hydrologic variability associated with rainfall (Table 4-13, p. 39 and Figure 4.9, page 41). Moreover, the total cumulative, long-term reduction in TP loads because of BMP implementation was 55%, overall, since WY1996. However, the average P concentrations in the outflow from EAA are still high and cause localized problems in the EPA near the outflow structures (Chapter 3A). Nevertheless, this load reduction is a very good accomplishment and experience and regulatory tools should be extrapolated to farming areas of the Lake Okeechobee watershed where the outcomes of the BMPs and other nutrient source controls are doubtful.

Also in WY2011, post-permit compliance activities continued, emphasizing BMP inspections following a prioritized list; more research was conducted toward improving BMP effectiveness by canal discharge management and control of floating aquatic vegetation (FAV); and area specific data collection/analysis was conducted to refine source control approaches to improve (reduce) P discharge concentrations. In this section, the ongoing and anticipated activities for WY2012 are nicely described and their importance is clear.

C-139 Basin – The TP load discharged from the C-139 Basin was above the predicted load from the pre-BMP baseline period adjusted for rainfall. However, the load is much smaller than that from the EAA. TP concentration in the flow is about the same than that from EAA. Similarly to EAA the WY2011 load is one of the smallest since 2000 (Figure 4-11 and 4-12).

Non-ECP Basins – The seven 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. Five have permitted regulating structures operated by the SFWMD. As in the basins contributing flows to EPA, the total TP load of 11.4 metric ton (similar in magnitude to S-139 basin) discharged to the EPA from the non-ECP basin structures during WY2011 represents a significant decrease from the total TP load 1597 discharged in WY2010. TP discharges to the non-ECP basins are regulated by permits.

Among the projects, the United States Army Corps of Engineers is constructing four water resource areas designed to improve water quality, restore wetland hydrology, increase water storage capacity, and enhance flood protection within the Big Cypress Seminole Indian Reservation.

Comments and Suggestions

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).

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.

Lake Okeechobee – 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?

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

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%.

Line 346 – Briefly explain the incentives.

Caloosahatchee Watershed – The PM for this watershed is nutrient loading at the watershed level. Please explain why concentrations are not also considered as a PM.

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

Lines 401-402 – How is progress being tracked to implement the plan? - please clarify.

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%)

Lines 447-448 – It would be helpful to provide a page number here.

Line 496 – Briefly explain why these discharges are considered negligible.

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

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

St. Lucie R Watershed – 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.

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.

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.

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

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?

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].

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 – 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."

EAA basins – 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.

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

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

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.

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.

C-139 basin – 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. The information stated in lines 1290-1293 should also be given in the legend (or footnoted) of Figure 4-11.

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

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.

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.

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

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.

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.

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?

Other ECP basins – C-51 West and L-8 – Lines 1496-1501 – add brief description of the number of stations and frequency of monitoring.

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.

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.

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

Lines 1579-1581 – This helpful information should be moved toward the front of the chapter.

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

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

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.

Editorial Suggestions

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

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.

Line 100 – …area-specific…

Lines 125-126 – suggest rewriting

Lines 206-207 – =….The evaluation method within 40E-61 is currently being….

Line 422 – …Current ERP Program rules require activities to be…

Table 4-2, footnote 7 – please rewrite; does not accurately state what is meant.

Line 426 – …demonstrate that the proposed…

Tables 4-9, 4-10 – it would be helpful to include percentages of the total.

Line 921 – …Lucie watersheds, anticipated to….

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.

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

Line 1393 – …if the water is flowing….

Lines 1435-1436 – …(4) the contract was executed and Phase II was initiated.

Line 1454 – …14 kg…

Line 1793 – SFERs, …

Appendices Evaluation

Appendix 4-1 – Lake Okeechobee Watershed

This appendix provides an excellent synthesis of information about nutrient source control programs and TP loads, and the regulatory basis. The monitoring data are explained in detail, including numbers of samples, and the information is presented in such a way that readers can discern sampling frequency. The accompanying maps and summary graphics are very helpful.

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.

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.

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.)

Appendix 4-2 – The Two Main ECP Basins

This appendix is also excellent in design, synthesis, and content. For EAA basins and the C-139 basin, it provides performance compliance calculation details (including the regulatory basis for determining compliance), water quality summaries by basin and sub-basin, short- and long-term precipitation and runoff, (related) short- and long-term variations in basin loads, and (EAA) permit-level water quality monitoring data. The EAA permit-level data are used for secondary compliance determination in the event of basin-level non-compliance. Additional information includes the separate hydraulic drainage areas, TP concentrations, and TP loads from individual farms in the EAA; and individual structure flows, related TP loads, and flow-weighted mean concentrations. These data are valuable for use in focusing on BMP source control efforts; individual farm performance can be evaluated. Appendix 2 clearly explains the pertinent regulations and requirements. This remarkable synthesis even includes a summary of the Agricultural Privilege Tax Incentive Credits earned to date.

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.

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.

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.

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.

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.

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.

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.

Appendix 4-3 – The Seven Non-ECP Basins

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.

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

This appendix provides information, including excellent maps, of the source control programs in these two major watersheds of the Northern Everglades, including lands with District WOD and ERP/SW permits, and agricultural lands that have enrolled in a BMP program.

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.

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.

Figures 1 and 7 – some of the labels are very difficult to read and should be enlarged.

Posted: 09/12/11 at 03:43 PM by V. Novotny

Summary Evaluation

Chapter 4 presents an extensive review of the structural activities, best management practices (BMP) development, permitting rule development, and other measures undertaken to achieve the goals of the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA). The programs that are described reflect adaptive management, that is, past and recent experiences are incorporated into the new rule development and continuing and new research. Overall, the EPA water quality improvement and restoration represent an excellent, remarkable effort. It is noteworthy that this chapter has both national and international significance. In this regard, the authors should be commended for their consistent use of metric units, with English units also helpfully given in parentheses.

Remaining Points That Should Be Addressed

The panel greatly appreciates the authors' thoughtful responses to the comments. Among the any comments that were made, only five points merit further consideration. The first three include the need for clarification about the status of performance measures (PMs) for the Caloosahatchee watershed; the lack of availability of two important appendices to Chapter 10, referred to in Chapter 4; and the need for work to improve the performance of a regression model for estimating target nutrient loads to the Everglades Agricultural Area (EAA) basins.

The fourth point reflects a problem stemming from the District's concerted efforts to shorten this year's SFER: Toward the goal of streamlining, the authors of Chapter 4 indicate that even brief explanations requested by the panel to ensure readers' understanding will not be included. As stated below, the panel strongly feels that such an approach overlooks the fact that a central purpose of the SFER is to educate policy makers and the general citizenry. The panel believes that "the pendulum has swung too far" in the "shortening direction," and that the inclusion of this information, in the form of the succinct responses from the authors, will greatly strengthen the chapter.

The fifth point is the continuing low participation of agricultural sources in the voluntary programs to reduce the phosphorus loads, especially in the northern watersheds and tributaries. This is hampering the progress toward meeting the goals, specifically the 2015 TMDL goal for Lake Okeechobee which is the main source of nutrients to the Everglades National Park. The report should provide a time line of compliance of agricultural sources.

Clarification of nutrient performance measures (PMs) **-** The panel asked the authors to include explanation about nutrient PMs for the Caloosahatchee and St. Lucie watersheds. The authors' response was that PMs for source control are under development, and that "upon completion, a preliminary proposal will be made on whether concentration, load, or combination-based PMs are advisable." Yet, lines 82-85 state that the District has completed "a recommendation of the type (nutrient load or concentration) of PM[s] that would be appropriate…." This writing seems to contradict the authors' response. Clarification is needed and should be added to the chapter.

Important information contained in Appendices to Chapter 10, referred to in Chapter 4, but not available throughout the SFER review process – Clarification was requested about how progress is being tracked to implement the Caloosahatchee and St. Lucie Watershed Protection Plans (lines 401-402). The authors' response referred the panel to appendices to Chapter 10, which still are not available. This problem unfortunately has extended through the 2012 SFER review process, making it impossible for the panel to evaluate important aspects of the SFER that relate to these appendices.

Improved performance of a regression model to estimate target nutrient loads to the EAA basins *(pp.4-37, 4-45)* - The panel noted that the target loads as estimated by a regression model are imprecise, at best, and asked whether steps were being taken to improve the regression model so that tighter confidence limits for the target loads can be used. After stating that helpful clarification would be provided in the chapter, the authors indicated that no efforts are presently being made to improve the regression models. Such work clearly seems to be needed. The explanation to Comment 24 by authors is not clear. Is the 25% reduction a margin of safety? A 50% median value means that 50% of the loads could be greater than the target value. It is not clear what would be the probability of exceeding the reduced target load.

Further explanation, needed to help readers' understanding –

* Regarding the District's development of water quality performance criteria and regulatory program updates, the panel asked for brief explanation about the present discharge target concentrations and why they were described as outdated (lines 331-333). The authors responded that the information would not be included; in the interest of streamlining the SFER, readers would instead be directed to a list of documents. The panel feels that this strategy is insufficient, and will weaken the quality of the SFER.

* Regarding the District's regulatory program, the panel asked the authors to include brief explanation about the program incentives (line 346). The authors stated that in the interest of streamlining the SFER, readers would be referred to a website for this important information. The panel feels that this strategy is insufficient, and will weaken the quality of the SFER.

* As a source control objective for the Caloosahatchee and St. Lucie watersheds, Chapter 4 states that the District and partners are ensuring that biosolids within the river and estuary watersheds do not contribute to nutrient loadings in the watersheds. Explanation was requested by the panel about the steps being taken to accomplish this task, which seems formidable. The authors' succinct, helpful response should be added to the main body of the SFER (either Chapter 4 or Chapter 10).

* Regarding Tables 4-5 and 4-6, the authors were asked to indicate (i) the overall goal of the percentage of sub-watershed acreage; (ii) whether there is a timetable for achieving this goal; and (iii) why some percentages are so low. The authors' helpful response should be added to the chapter.

* Explanation was requested by the panel about how nutrient discharges will be evaluated to assess whether network improvements or hydrologic modifications would be needed in the Caloosahatchee watershed (lines 484-485, 497-502). The authors' response did not seem sufficiently clear. A succinct, clear explanation should be added to the chapter.

* Further explanation was requested by the panel about why Marshpoint and Courtney Canal are not monitored in the Caloosahatchee Sub-watershed – are they significant or small/ minor contributors based, for example, on their size and/or on best professional judgment (lines 503-505). Similarly, the chapter should explain why Yucca Pen Creek is not monitored in the Coastal Caloosahatchee Sub-watershed (lines 506-507). The authors responded that they did not know why, and that this would be an item for follow-up as part of the development of a representative water quality monitoring network. This response seems insufficient; to help readers, the chapter should include brief explanation based on best professional judgment / familiarity with these sub-watersheds.

* The panel noted that a substantial part of the St. Lucie watershed (130,000 acres) is not represented by monitoring, and asked why a broad range of 12-20 additional water quality or flow stations have been identified as needed to adequately assess this watershed (lines 519-520). The authors did not clarify why a substantial portion of the watershed is not monitored; such

clarification should be added to the chapter. The authors' helpful information, about the broad range in the anticipated number of monitoring stations needed, should be added to the chapter.

* Explanation was requested about why there are such small percentages of agricultural lands enrolled in BMP programs for the tidal and coastal Caloosahatchee sub-watersheds, and for Basins 4-6 and South Fork in the St. Lucie watershed (Tables 4-7 and 4-8). The panel noted that because these lands are in such close proximity to the estuaries, participation in BMP programs by agricultural entities would seem especially important. The authors' response contained important information that should be added to the chapter.

* Further explanation was requested about why the discharges from the C-139 basin are not exceeding the targeted limit and meeting the PM, despite the fact that discharges were above the predicted load from the pre-BMP baseline period adjusted for rainfall. The authors provided a helpful response (four sentences), and this response in its entirety should be added to the chapter.

* The panel noted that the District's goal of balancing annual climate patterns with floods, natural resources (wetlands) protection, and water availability seems very important, and requested clarification as to when it is anticipated that this goal will be achieved. The authors explained that the purpose of the C139 Basin Regional Feasibility Study is to identify a suite of alternatives that can be planned to meet this goal; but that, although evaluation of the alternatives is scheduled for completion in WY2012, the timeline of actual implementation will depend on available (funding) resources. This brief explanation should be added to the chapter.

* Given that the drought substantially limited data collection (p.4-52, lines 1447-1449), the panel asked whether it would be possible for the District to extend the Chemical Precipitation study so that the important information can be obtained. The authors responded that additional funds for demonstration projects have been approved. This helpful, encouraging information should be added to the chapter.

* Clarification was requested as to when it is anticipated that the other three water reserve areas will be constructed for the Seminole Tribe Water Conservation Plan Project (lines 1620-1625). The authors responded that completion dates unfortunately are uncertain due to economic conditions. This information should be added to the chapter.

* Appendix 4-2, submittal of permit-level data – The panel suggested adding brief explanation about what is being done to encourage BMP permit holders in the C-139 basin to request the optional farm-level compliance. The panel noted the importance of such efforts – without them, the data gap will continue, despite the District's many efforts to develop strong control programs. The authors' helpful response in its entirety should be added to the chapter.

Final Bullets

<u>Key Findings</u>

- Chapter 4 presents a generally excellent overview of the District's adaptive management programs and actions undertaken to achieve the Northern Everglades and Estuaries Protection Program (NEEPP) and the Everglades Forever Act (EFA) goals.
- The chapter and its four excellent accompanying Appendices clearly convey that the District has shown appropriate accountability to NEEP and EFA in its actions to develop effective source control programs for the Northern Everglades and Estuaries, and the Southern Estuaries, respectively.
- In this chapter, the District has also shown attentiveness in avoiding duplication of effort and streamlining for increased program efficiency. There is a clear recognition that consistent, holistic approaches are needed for source control programs in the major watersheds, while

also considering the unique source control issues that affect each sub-watershed. The District's identified strategies for moving forward seem logical and sound.

Recommendations

- Clarification of the status of nutrient performance measures should be added to Chapter 4, such as for the Caloosahatchee and St. Lucie watersheds.
- In future SFER reviews, important information referenced in the chapter, such as information contained in Appendices or internal SFWRD reports and plans, should be made available during the review process and/or briefly summarized in the report.
- Efforts should be undertaken to improve the performance of the regression model that is being used to estimate target nutrient loads to basins in the Everglades Agricultural Area.
- Within the context of the District's intent to streamline the SFER, care should be taken to ensure that explanations are sufficient to ensure readers' understanding. Accordingly, where indicated by the panel, succinct information should be added to assist readers.
- In consideration of the present relatively low enrollment of agricultural nutrient sources in best management practices (BMP) programs, especially in the northern watersheds, the District should consider steps to strengthen agricultural participation, insofar as possible.

Posted: 10/19/11 at 07:28 AM - Public Comment, FDEP

Chapter #	Line #	Comment	Name	Agency/Entity	Phone #
4	Table 4-2	under FDEP replace " Domestic Wastewater Residual" with "New Biosolids Rule Chapter 62-640"	Chad Kennedy	FDEP OEP	561 682-2661
4	Table 4-3	#8 replace " Domestic Wastewater Residual" with "Biosolids"	Chad Kennedy	FDEP OEP	562 682-2661
4	267	replace FDEP manure application… with FDACS manure application	Chad Kennedy	FDEP OEP	563 682-2661
4	356	this legal language may no longer be appropriate due to a recent ruling	Chad Kennedy	FDEP OEP	564 682-2661
4	376	replace "possible" with "practicable"	Chad Kennedy	FDEP OEP	565 682-2661
4	409	replace "were" with "are" (we still are granted authority)	Chad Kennedy	FDEP OEP	566 682-2661
4	581	replace "district" with "FDEP" FDEP is tasked with writing the monitoring plan.	Chad Kennedy	FDEP OEP	567 682-2661
4	804	replace "conditions of a TMDL" with "conditions of a Basin Management Action Plan (BMAP)"	Chad Kennedy	FDEP OEP	568 682-2661
4	807	replace "TMDL" with "BMAP"	Chad Kennedy	FDEP OEP	569 682-2661
4	812	...submit to the Florida Department of Health (FDOH)…	Chad Kennedy	FDEP OEP	570 682-2661
4	Table 4-12	0091 Mantee Packet…ERP… the letter P is missing	Chad Kennedy	FDEP OEP	571 682-2661
4	1067	the map depicts a huge EAA reservoir, it appears to include FL crystals land, is this accurate?	Chad Kennedy	FDEP OEP	572 682-2661

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 5

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

Posted: 09/20/11 at 09:44 AM by O. Stein

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

Broad questions and comments that should be addressed

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. 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?

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 phosphorous. 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.

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.

See the comments associated with Figs 5-7 to 5-10 and Table 5-2. Maybe it is time for the District to take a completely different approach to characterization of the STAs for the enhancement of our knowledge of operational controls on performance if the goal to minimize the use of educated guesswork and trial and error out of operational management decision making as outlined in Summary and Recommendations section.

Specific comments questions by line number

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.

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

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".

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.

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

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.

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.

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?)

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.

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.

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.

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

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

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

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

Editorial comments by line number

231; change *have to* to *must*

250: contributing to *slightly enhanced* Cell 1 performance

545: change *depth* to *stage*

696: interval*s*

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

Summary and Recommendations

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

The panel believes it is time to carve out a more manageable experimental system somewhere within the STA system where the scientific approach can be applied to replicated experimental units large enough to representative of the field scale, but small enough to not be influenced by currently uncontrollable variables such as hydrology (water depth, dry out) soil type etc. Data from these experimental units would provide a baseline of "best achievable" TP removal results and used to optimize controllable inputs such vegetation type, hydroperiod, water depth etc. Though the initial cost of such a system might be high compared to continued annual expenditures of monitoring of current full scale system, it seems highly unlikely the current

approach of monitoring such an expansive, variable system, even if conducted over another decade or more, will ever produce the desired output of knowing what controllable factors can be manipulated to optimize performance. Monitoring of a controllable experimental unit, with multiple cell sizes on the order of a few acres, would ultimately lead to better operational decision making.

Posted: 10/24/11 at 12:04 PM by O. Stein

Closing comments on authors' responses

A general comment applicable to all chapters is that the reviewers believe the new streamlined format has made the job of providing a technical review more difficult. The streamlined format is probably sufficient for chapters requiring only and accountability review and previous versions Chapter 5 in particular could be streamlined somewhat without affecting a technical review, but the streamlining effort, in general, went too far. Some specific comments addressing the appropriate and inappropriate removals are provided in the initial review, authors' responses and in final comments below. This is addressed again in the final recommendations.

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

Comment#1: A common complaint of reviewers across many chapters appears to be that the new streamlined format has removed components critical to an effective peer-review, especially those that are designated for Technical Review, such as Chapter 5. Moving ancillary information such as data required solely for permitting from the chapters to appendices has merit for organization of the chapters, but moving critical information to appendices to simply reduce the length of the chapter makes little sense. Summarized data on long term trends of P removal is critical for an evaluation of STA performance and the effect of management activities on performance and is not just a permitting requirement. The long term graphs included in previous reports were an easy-to-see way to summarize this type of information.

Comment# 2: The reviewers agree that one way to streamline the chapter is to summarize research projects only as they are completed. However, as suggested for other chapters, a table listing all current research projects with an expected completion date would help the reader comprehend the scope of activities that are ongoing and give a sense of when results for continuing projects can be anticipated. More importantly (and confusing) is that the response indicates that the authors are actively completing research projects as demonstrated by the listed peer-reviewed publications, yet these are not mentioned in the chapter despite the comment that completed projects would be included.

Comment#3: The reviewers hope that the District personnel will follow through on more complete data analysis, but point out that this has been a consistent recommendation for several years and, if anything, this year's chapter has moved in the opposite direction. There appears to be some new direction for next year's report which is positive, but this will require a new and more detailed approach to drawing causative relations to performance. The bulk scale analysis as shown in Figure 5-11, is proof that a simple one parameter model for performance prediction is insufficient. The reviewers point out that this type of analysis was start-of-the-art at the time the STAs were first coming on line and is still often used to develop design methodologies, but there are now far more sophisticated models to predict actual performance of treatment wetlands (Langergraber 2011, 2009). While these two references are typically specific to sub-surface flow systems, they are examples of the far greater sophistication of models that are available or could be developed. Additionally, more research emphasis should be placed on process-based experiments from which causative relations can be determined, because as the authors point out, broad-brush data collection from a huge operating system that must be continually managed to meet permit requirements as typically employed by the District incorporates all the noise as well as relevant relations, with the result being the large scatter seen in the relationships as shown in Figure 5-11.

Comment#4: It would seem more realistic to put off until next year's report an effort toward a comprehensive reorganization of the chapter around STA/flow way first then parameters of interest second. Instead focus modifications to this year's chapter on filling some of the missing gaps within the current organization.

Line #127: The reviewers do not understand the response and believe it might be inadequate. A flow weighted mean concentration split into components e.g. flow exiting a specific flow way or outlet structure, must still sum to the total flow weighted mean exiting from the entire unit e.g. the specific STA. The fact that the two reported values are so far off suggests much more is wrong than the listed potential factors. Whether that is an error in measurement or an error in describing exactly what the reported data represents is not clear.

Line#139: It sounds as if the District and the Corps have a difference of opinion as to the effectiveness of the PSTA option, since the District is continuing with its experiments in STA 3/4. Could the District take over operation of this PSTA project and double the opportunity for drawing cause and effect research on PSTA effectiveness by comparing results of the two systems, potentially at different input controls?

Line#363: The reviewers are encouraged by the authors' response, but suggest that authors review our response comment#3 and the recommendations below.

Line#492: Could the authors add the first sentence to the chapter to increase understanding of the issue?

Line# 521: The reviewer raising this issue is not an avian expert, but the chapter seems to indicate that the stilts prefer the levee over the open areas of the STA, which lead to the comment. The response seems to contract that supposition but doesn't really offer an alternative. Regarding the response about small islands causing preferential flow, elevated areas on the order of a few square meters randomly (or even geometrically placed) in an STA of 100's of hectares is extremely unlikely to cause preferential flow; they would not be continuous features and if placed at a density of even one every few 100 meters would be an insignificant influence on volume and retention times. It may not be worth the effort and expense, but the effect on hydraulics would be imperceptible.

Figs 5,7,8,10: The point of the comment is never responded to directly, namely that transect data should be normalized to standard models of reactor theory to better interpret results, a standard practice in most environmental engineering research. The level of model sophistication may vary, but even the most simplistic plug flow model would likely separate some of the temporal variation relative to spatial variation.

Table 5-2: Perhaps this comment was misplaced as it is generic to the entire STA research program not just the PSTA. See the general comments and recommendations.

Summary Response

Perhaps the authors have interpreted reviewer comments to suggest that we believe the District is not committed to the Long Term Plan and an Adaptive Management operational structure. It is clear that the District continues to modify operational controls and incorporate new findings to improve performance. And as previous reviews have indicted, the STA system and the District's efforts are world renown in the science and engineering of treatment wetland systems. Much of the scientific knowledge of treatment wetland performance in large-scale systems and P removal particularly can be directly attributed to what have been learned from the STAs. That said, it does appear to the review panel that the advances in the science of treatment wetland technology that have occurred over the last decade or so have not been incorporated into current thinking of District's management of these systems. This may be partly due to the fact that it is difficult to incorporate some advances (such as type of system) once a large scale system such as the STAs is brought into operation, but it is probably fair to say that the district's research program can longer be consider cutting edge in the field. This contrasts to the research effort on mercury methylation in the EPA. It is shame to see such a missed opportunity to not only improve the STA performance but to lead the scientific discoveries associated with treatment wetland technology.

References

Langergraber, G. (2011): Numerical modeling: a tool for better constructed wetland design? WATER SCI TECHNOL. 2011; 64(1): 14-21.

Langergraber, G., Rousseau, D., García, J., Mena, J. (2009): CWM1 - A general model to describe biokinetic processes in subsurface flow constructed wetlands WATER SCI TECHNOL, 59(9), 1687-1697.

Recommendations (SOW Task 3)

- The streamlined chapter has eliminated key pieces of information critical to an effective technical review. In particular figures showing long-term trends have been eliminated and the number and completeness of reported research has decreased dramatically compared to previous year's SFERs. As outlined above some material should be re-incorporated.
- Future research on STA performance and optimization should take advantage of the newer generation models for treatment wetland performance. If necessary available external expertise should be sought to adapt current models to the specifics of the STAs. Note that a recommendation last year stated, "*In general, the panel believes that the District tends to underutilize the volumes of data that are collected. Better and more complete analysis would make better use of the expense and effort put forth in the collection of the data*". A modeling framework would help make this possible.
- The current approach to the STA research program is not effectively leveraging the most recent advances in treatment wetland science. Newer process-based models require information that might not be available in the currently collected and/or available data sets. The District should reevaluate its research program to be sure it remains in sync with the current knowledge of treatment wetland performance, especially how it relates to phosphorous removal.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 6

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

Posted: 09/16/11 at 02:58 PM by J. Burkholder

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. 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). 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. 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.

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).

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.

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.

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.

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.

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?

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.

P.6-20 – Brief description should be added of the macrocosms and outlines the experimental design.

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.

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

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.

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.

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.

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.

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.

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

Figure 6-18 – should indicate significant differences.

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. 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).

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 roseate 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.

Plant Ecology (3 projects highlighted in the Summary, but 5 projects in the text)

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.

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?

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.

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.

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. 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). 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. 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.

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).

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.

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

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. 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.)

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.

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.

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

Lines 903-905 – please clarify the meaning of this sentence

4) <u>Manipulating submersed aquatic vegetation (widgeongrass) density in Joe Bay (northeastern Florida Bay)</u> – 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.

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]

5) <u>Submersed Aquatic Vegetation Physiology [suggest changing to Seagrass Physiology]</u> – 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.

Ecosystem Ecology (2 projects highlighted in the Summary, but 5 projects in the text)

1) <u>Update on the Cattail Habitat Improvement Project (CHIP)</u> – 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/SAVdominated. 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.

Line 983 – please define resilience.

2) <u>Florida Bay Water Quality</u> – 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.

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

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.

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.

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

3) <u>Central Lakes Region Sediment-Water Nutrient Fluxes</u> – 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.

4) <u>Lakes Phytoplankton Study</u> – 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.

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)?

5) <u>Soil Salinity Transects</u> – 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)

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.

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) <u>Paleoecological and Imagery Analysis of Tree Island Acreage Over the 20th Century</u> – 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) <u>Floccometer Transport Studies (toward preserving/restoring the ridge and slough landscape)</u> – 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."

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).

3) <u>Milestone – a new book</u>, "*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

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

Lines 7-8 – shouldn't the writing say five areas, including hydrology?

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.

Line 36 - …on large-bodied (predatory)

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.

Lines 91-92 – first mentions the period from 1960-1970, and then the period from 1960-1963. Please clarify.

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.

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

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

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

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?)

Lines 131-132: …below-average precipitation….would also have been below…

Line 139 - …Both of these…

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%.

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]

Lines 459-460 - …and their densities were analyzed using…

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

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]

Line 521 - …at which this organization collects..., performs bimonthly submersed aquatic…, and samples…

Line 557 - …suggest that numbers…

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

Lines 649, 912, 940 – change submerged to submer**s**ed

Line 710 – …were found too infrequently to…

Lines 805-806 – please add a supporting reference for “the White method.”

Line 826 – islands, which…

Line 926 – …rate of recruitment into these…

Lines 912-939 – this subsection needs an ending “Relevance to Water Management” for parallelism with other subsections.

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

Posted: 10/21/11 at 05:38 PM by J. Burkholder

Chapter 6 emphasizes key findings in Everglades research and evaluation within five subject areas – hydrology, wildlife ecology, plant ecology, ecosystem ecology, and landscape. 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.

Chapter 6 is excellent in technical caliber. The chapter is also nicely written and accompanied by generally excellent graphics and tables. The authors accomplished this despite the complexities of Everglades research and evaluation, and despite major space constraints imposed for this year's SFER. The panel greatly appreciates the authors' careful attention in having addressed each of the panel's comments and suggestions in this review process. Their responses additionally were thoughtfully conceived and informative. Because of the authors' thoroughness in responses to the panel's comments, only three points remain to be addressed:

(i) The helpful information contained in the authors' responses regarding percentage increases in submersed aquatic vegetation (lines 867-876) should be added to the chapter.

(ii) The Ecosystem Ecology section highlights only two projects in its Summary, but five projects are described in the section. This discrepancy should be resolved by adding the short paragraph given in the authors' responses to panel comments.

(iii) In Figure 6-30, the long-term patterns shown are promising, especially the suggested decrease in phosphorus. These data would seem to warrant at least preliminary statistical trend analysis. The authors' response to this comment was that the information was not added due to space limitations, but within the figure this information can and should be added. That approach will conserve space. The panel feels strongly that this information should be added, considering that a central purpose of the entire program is to lower the TP levels.

Final Bullets

<u>Key Findings</u>

- Chapter 6 provides a clear, scientifically sound synthesis of major findings and progress in Everglades research and evaluation within five subject areas – hydrology, wildlife ecology, plant ecology, ecosystem ecology, and landscape. This chapter exemplifies the *best* a SFER chapter can be in technical caliber.
- The studies described within each of the five subject areas have provided valuable data in guiding District restoration efforts.
- The new book, *Landscapes and Hydrology of the Predrainage Everglades*, is an important milestone by District scientists that provides the first well-documented "baseline" condition for Everglades science and restoration, including system-wide information on pre-drainage water depths and hydroperiods.

<u>Recommendations</u>

- The District should continue to carefully track macroalgae in Florida Bay, and to assess the influence of nitrogen concentrations versus other factors, such as warming of the water as part of a climate change signal, on their apparent increased growth.
- The exciting floccometer instrument has the potential to be very valuable to Everglades science and also shows promise in important applications in various aquatic ecosystems

worldwide. The District should continue to test and develop this instrument and its capabilities.

- District scientists should be strongly encouraged to contribute additional syntheses, in the form of books, about the valuable science that they are conducting in the Everglades. Their often pioneering contributions are important to wetlands science, restoration, and management not only locally, but nationally and internationally.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 7

Level of Panel Review: Accountability

Reviewers: J. Burkholder (AA) and O. Stein (A)

Posted: 09/16/11 at 02:56 PM by J. Burkholder

This year's chapter on the Status of Nonindigenous Species (Chapter 9 in previous SFERs) presents a solid overview of issues involving invasive nonindigenous species in South Florida, including their relationship to restoration, management, planning, organization, and funding, and emphasizing progress in fiscal year (FY) 2011. The chapter also describes the status of nonindigenous invasive species, outlines programmatic needs, and summarizes any control or management that is underway for priority species that threaten the resources that the District is mandated to manage or restore.

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.

Accountability - Draft Chapter 7 demonstrates strong accountability. It presents a defensible synthesis of findings about invasive nonindigenous species (see "Synthesis of information about progress" section of this review, below), and the findings are clearly linked to management goals and objectives. A major improvement to this chapter in recent years, including this draft chapter, is coverage of bioinvasive fauna as well as flora. This year's version of Table 7-1, while not as complete and detailed as some previous versions, is excellent - very helpful in its presentation of updates at the overall system level and for each RECOVER module, including information on agency partners involved and the associated legislative mandates. The table facilitates cross-referencing of region (module)-specific coverage of invasive species issues.

The issue of invasive nonindigenous species crosses multiple jurisdictions, agencies, and groups; thus, appropriately, a separate section of the chapter addresses Interagency Coordination.

Cooperative Invasive Species Management Areas (CISMAs) are organizations, defined by a geographic boundary, that "provide a mechanism for improved communication about invasive nonindigenous species across jurisdictional boundaries to achieve the goal of regional species prevention and control" (lines 858-863). They are organized under an "umbrella organization known as the Florida Invasive Species Partnership (FISP), which is an interagency collaboration consisting of federal, state, and local agencies, non-governmental organizations, and universities. The two main CISMAs within the "footprint" of the Greater Everglades ecosystem are the Everglades and Treasure Coast CISMAs. Five others that are wholly or partially within the Greater Everglades "footprint" are the Florida Keys Invasive Species Task Force, the Southwest Florida CISMA, the Heartland CISMA, the Osceola County Cooperative Weed Management Area, and the Central Florida CISMA.

Among the excellent points made in the last section of draft Chapter 7, "Future Needs in Management and Control," the chapter authors identify the elements of a comprehensive management program for nonindigenous species – legislation, coordination, planning, research, education, training, and funding. This set of elements might, in fact, be considered as a logical framework for the chapter organization in future SFERs, especially if the chapter continues to be reviewed on the basis of accountability. It would be helpful, for accountability review, to mention the key legislation up front, then the coordination and planning, then the funding, followed by progress in research, education, and training. Table 7-1 lists (and does not describe) the state mandates; pertinent federal legislation is not mentioned. Yet, much later in the chapter (p.7-26), two legislative and policy initiatives are described, one concerning a change in the U.S. Department of Agriculture's Animal and Plant Health Inspection Service (APHIS – new category for invasive plants, "Not Authorized Pending Pest Risk Analysis," NAPPRA) and the other, a change proposed to the federal Lacey Act by the U.S. Fish and Wildlife Service. It would be helpful to include brief information about the federal legislation early in the chapter (perhaps as a new table, Table 7-1; then present Table 7-1 would become Table 7-2 etc.).

The "Future Needs in Management and Control" section describes a critical need to develop a comprehensive management program for nonindigenous animals, which historically have been underemphasized in management programs in comparison to plants. Of particular relevance to the District, the authors state that the threat of an "overwhelming" number of nonindigenous animals is becoming an important ecological and restoration issue. It seems very clear that nonindigenous invasive species, both plants and animals, have become a serious threat to various valued ecosystem components (VECs) and to the District's overall restoration efforts. In recognition of this critically important point, the authors advise that, given the documented impacts of nonindigenous species in South Florida, scientists are now "obliged to factor these species and their impacts into restoration models." In reviews of previous SFERs, the Panel has recommended increased consideration of nonindigenous invasive species in the District's restoration efforts, and also has suggested this issue as a cross-cutting theme. The Panel strongly supports the authors' recommendation for a comprehensive nonindigenous animal management program for Florida as a pressing need – a program that is supported by "meaningful legislation to significantly limit new invasions, funding for control programs, and coordination at all levels."

In other demonstration of accountability, draft Chapter 7 identifies research needs aimed at understanding the distribution, biology, and impacts of nonindigenous species using an "all-taxa approach." Because it is very important to catch the invaders early (incipient phase of invasion) in order to maximize the potential for containing or eradicating them, draft Chapter 7 also identifies a critical need for a strong, applied monitoring program (e.g. an EDRR program, Everglades Early Detection/Rapid Response) and tracking system for nonindigenous invasive species. Such a program would enable detection of new invasive species before they are able to become established, thus avoiding the need for "perpetual control" in long-term management. From a cost-effective standpoint, the authors point out that implementing an EDRR program is usually much less expensive than a long-term management program.

Synthesis of information about progress - The draft chapter presents a fairly clear synthesis of District activities related to management of invasive nonindigenous species in FY2011 (see section, "Broad Questions and Comments That Should Be Addressed," below). Some of the reported progress is very exciting, such as having nearly eliminated water hyacinth and water lettuce from urban canal systems in Broward and Miami-Dade counties. The digital aerial sketch mapping of invasive plants, in WCA-1 and on all District-managed lands along the eastern boundary of the Everglades, was a major milestone; other major progress involved the initiation in 2010 of a program to monitor priority invasive reptiles and amphibians and their impacts within the Greater Everglades system. The pilot program to use dogs to detect Burmese pythons sounds promising (hopefully the "Judas snake" approach will work as well), as does the use of imazamox to selectively control cattail without appreciable damage to beneficial vegetation (appropriately covered here as well as in Chapter 6). Of course, the District's remarkable, successful *Melaleuca* control program is world-renowned. It is most unfortunate that private lands continue to serve as sources of melaleuca re-infestation for public lands (p.7-9), considering the many great efforts by the District and partners to eradicate it.

Much of the information related in the chapter, on the other hand, underscores the immense challenge of efforts to control of nonindigenous species. It is hoped that mile-a-minute can still be eradicated, but the limited funding/staffing for this effort are a major concern (p.7-10). The information given for old world climbing fern also sounds worrisome: It was described (p.7-10) as posing a greater threat to South Florida's mesic upland and wetland ecosystems than any other nonindigenous invasive plant species. Moreover, "left uncontrolled, this invasive fern is likely to negatively affect restoration performance measures for freshwater vegetation mosaics and ridge-and-slough community sustainability," so it now threatens ecologically valuable tree islands as well. Torpedograss was also described as a formidable enemy, with perennating rhizomes that grow to depths of 20 inches and subterranean tissues that comprise most of the mature plant biomass. "Hair-raising" information about its reproduction was included (p.7-12): A single culm can produce ~23,000 rhizome buds in a year's time. The large-scale treatments to control torpedograss in Lake Okeechobee's marsh affected 3,600 acres in what must have been a very arduous effort. Yet, this amount of area seems small, considering that torpedograss is present in much of the lake's 100,000 acres of marshland. It is therefore somewhat surprising, and encouraging, that "broad expanses of the grass have been brought under control by annual treatments averaging several thousand acres per year."

New threats continually must be detected and combated, as early as possible, as illustrated by tropical American watergrass: In only two years, this plant went from first detection in North America (Lake Okeechobee) to being ranked in the "most invasive plant" category. Draft Chapter 7 includes an informative section on biological control of invasive plant species. Biocontrol holds great promise but also present formidable challenges because of all that is not known and consequently all that can go wrong, and because of the detailed and lengthy studies and reviews that are required before a biocontrol agent can be introduced. A nice example of a success story is the District's and partners' use of a combination of melaleuca weevils and melaleuca psyllids to control melaleuca. This biocontrol treatment that has achieved more than 80% stem mortality in some stands, substantial decreases in melaleuca canopy cover, increased plant species diversity, and a ~98% reduction in melaleuca re-invasion. A new mass rearing facility, scheduled for completion about a year from now (lines 54-58), will be valuable for the District's efforts in combating invasive nonindigenous species using biocontrol. The new facility is planned to support implementation of biological control rearing, field release/establishment, and monitoring for these organisms.

Notwithstanding the formidable challenges posed by priority nonindigenous invasive plants, the priority invasive animals present equal or greater challenges across South Florida. Among these, the Nile monitor issue has all of the "ingredients" of a very destructive situation: Its impacts on Florida fauna is unknown, but its potential to eliminate or significantly reduce native species is high – especially ground-nesting species such as gopher tortoises, sea turtles, burrowing owls, and Florida gopher frogs. *The Nile monitor has the potential to significantly impact restoration efforts*, especially performance measures for survival of juvenile American crocodiles and alligators. Presently available trapping methodologies will need considerable work to become cost-effective for monitoring and management. And, as a final major "ingredient" of concern, there continues to be little or no funding to support development of critically needed control tools or organized monitoring programs. The authors' logical prognosis, backed by supporting literature, is that if the resources for such initiatives continue to be unavailable, Nile monitor populations will continue to spread, and the likelihood of eradicating or containing them will decrease, so that the fundamental threat that this species represents for restoration efforts may well become reality. A similar threat exists regarding the Argentine black and white tegu, a large omnivorous lizard that preys upon ground nesting birds and reptiles such as the American crocodile and the Cape Sable seaside sparrow. The Kissimmee/Okeechobee Region is a concern, given that "some difficult-to-control species continue to threaten restoration goals." Also in that module, the authors report that new infestations of Old World climbing fern are increasing in number, "and it is proving difficult to stay ahead of its spread."

The fungus that causes laurel wilt, and the beetle that introduces it to trees, take the challenge of controlling nonindigenous invasive species "up a notch" because laurel wilt is spreading rapidly, affecting at least three beneficial tree species, threatening the structure and diversity of valuable Everglades tree islands and related performance measures – and there is as yet no feasible method for controlling this fungus or the disease it causes.

Broad Questions and Comments That Should Be Addressed

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.

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."

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.

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.

Line 168 – Is there a known reason why fire appears to aggravate infestations of downy rose myrtle?

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.

Lines 319, 320 – are disjunct; please correct.

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.

Line 347 – …is required for successful.

Line 358 – …of the invasive plant list.

Line 361 – …has found that it produces

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?

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.

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.

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.

Table 7-1 – What percentage if wetland acreage in the Biscayne Bay Coastal Wetlands do the 25 acres that were treated comprise?

P.7-4, last entry, middle column, 3rd line - …Encounter rates ranged from…

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

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

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 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").

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.

Line 872 – should mention the type(s) of training.

Lines 878-879 – should describe the specific location of the Everglades CISMA (e.g. see lines 915-916 for the Treasure Coast CISMA).

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

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.

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).

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.

Posted: 10/21/11 at 04:25 PM by J. Burkholder

This year's chapter on the Status of Nonindigenous Species (Chapter 9 in previous SFERs) presents a solid overview of issues involving invasive nonindigenous species in South Florida, including their relationship to restoration, management, planning, organization, and funding, and emphasizing progress in fiscal year (FY) 2011. The chapter also describes the status of nonindigenous invasive species, outlines programmatic needs, and summarizes any control or management that is underway for priority species that threaten the resources that the District is mandated to manage or restore. The panel greatly appreciates the authors' careful attention in having addressed all of the panel's comments and suggestions in this review process.

Chapter 7 demonstrates strong accountability. It presents a defensible synthesis of findings about invasive nonindigenous species, and the findings are clearly linked to management goals and objectives. A major improvement to this chapter in recent years, including this draft chapter, is coverage of bioinvasive fauna as well as flora. This year's version of Table 7-1, while not as complete and detailed as some previous versions, is excellent - very helpful in its presentation of updates at the overall system level and for each RECOVER module, including information on agency partners involved and the associated legislative mandates. The table facilitates cross-referencing of region (module)-specific coverage of invasive species issues.

Because the issue of invasive nonindigenous species crosses multiple jurisdictions, agencies, and groups, appropriately a separate section of Chapter 7 addresses Interagency Coordination. In other demonstration of accountability, the chapter identifies research needs aimed at understanding the distribution, biology, and impacts of nonindigenous species using an "all-taxa approach," including a strong applied monitoring program and tracking system. The authors also identify a critical need to develop a comprehensive management program for nonindigenous animal species, which have been under- emphasized in comparison to plants.

In the interest of streamlining/saving space, the chapter understandably is somewhat inconsistent with previous versions. Because of space constraints, compelling historic information about invasive species in this region unfortunately was omitted from this year's version. The outstanding indicator-based stoplight tables that were/are so "user-friendly" for the general public, and photos of the priority species, also were removed from this year's chapter, to conserve space and also - considering the stoplight information - because the status of many of the species has not changed over the past year or updated information is not available.

Final Bullets

Key Findings

- Chapter 7 demonstrates strong accountability: It presents a defensible synthesis of findings about invasive nonindigenous species, and the findings are clearly linked to management goals and objectives. It identifies research needs aimed at an "all-taxa approach" for understanding the distribution, biology, and impacts of nonindigenous species, and also a critical need to develop a comprehensive management program for nonindigenous animal species.
- There are very serious funding constraints impeding the District's excellent and critically needed efforts to control top-priority nonindigenous species in South Florida, including adequate funding for staff; sufficient dedicated funding for early detection and rapid response; and funding to support the development of essential control tools and integrated monitoring programs.
- Nonindigenous invasive species are now a serious threat to various valued ecosystem components, and to the District's overall restoration efforts.

Recommendations

- To strengthen the chapter content, in future SFERs Table 7-2 should be expanded to include priority animal and fungal species; a summary table of all priority invasive species should be added, including their current regulatory status; and, at five-year intervals, the stoplight tables should be updated in Chapter 7, and photos of all priority species should be included.
- An alternate framework should be explored for possibly improving the chapter organization in future SFERs, such as key legislation (state and federal); coordination and planning; funding; progress in research, education, and training; and identified research and management needs.
- The authors should consider a secondary classification for nonindigenous species, in addition to priority versus non-priority categories, to more clearly indicate the District's goals. Such a secondary system, for example, might include new invaders, species targeted for eradication, and established species.
- The panel strongly supports the authors' recommendations to develop an "all taxa" research approach for understanding nonindigenous species and their impacts; and to develop a comprehensive management program for nonindigenous animal species.
- Considering the documented impacts of nonindigenous terrestrial and aquatic species in South Florida, these species and their impacts should be factored into restoration models.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 8

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

Posted: 09/19/11 at 08:25 AM by P. Dillon

SUMMARY

Lake Okeechobee is the largest lake in the southern eastern USA. It provides southern Florida with water for water supply, recreation, irrigation for agriculture, flood control and wildlife habitat. The lake has been subject to three significant long-term stresses: (1) excessive total phosphorus (TP) loads, (2) extreme water-level fluctuations, and (3) rapid spread of exotic and nuisance plants in the littoral zone. In the past the water quality of the lake has been poor and the lake has suffered from the symptoms of severe eutrophication with blooms of noxious blue-green algae (cyanobacteria), loss of benthic invertebrate diversity, and spread of cattail (*Typha* spp.) in shoreline areas. In 2000, the Florida legislature passed the Lake Okeechobee Protection Act (LOPA), which requires state water quality standards to be achieved no later than January 1, 2015. The watershed abatement program is now a part of the Northern Everglades Environmental Protection Program (NEEPP) which also includes the Caloosahatchee and St. Lucia Rivers. Both rivers receive flow from the lake and so does the Everglades National Park. The TMDL water quality-based limits have been established for the lake for phosphorus which is the limiting nutrient. The annual maximum permitted load of phosphorus is 140 metric tons. Based on the NEEPP the abatement plan for the lake was updated in 2011.

Because WY2011 was a year of extreme drought (inflow of water to Lake Okeechobee was 1,151 million m3 or only 38 % of the baseline average for calendar years 2001–2009 of 3,000 million m3), the phosphorus load to the lake (177 metric tons) was much lower than the long-term mean load, the previous year's load (471 tons) or the 5-year moving average (352 tons). The current 5-year moving average includes the 3 driest years of the past 30, resulting in the lowest 5-year average P load in the 30-year period that these calculations have been done. However, the load in WY2011 is still higher than the 140 ton limit set for the lake.

While numerous efforts begun in WY2011 under the Lake Okeechobee Watershed Construction Project and ongoing abatement and implementation of best management practices throughout the watershed may have contributed to the reduced P load, the fact that most abatement measures are still in planning and research phases indicates that drought in WY 2011 was the principal cause of the reduced load. This has major implications for the LOPP. The detrimental symptoms of eutrophication are a result of elevated phosphorus concentrations, which are not solely a result of elevated loading. Other factors, particularly hydrology (expressed as water replenishment time, flushing rate, etc.) also play an important role, and all lake models that are used to predict P concentrations recognize this. In short, a reduced P load in conjunction with a reduced hydraulic load may not lead to any decrease in P concentration in the lake, and thus to no improvements in water quality.

The 140 ton target for the P load was presumably based on long-term average hydrologic conditions. Even though the target is a legislated figure, it may have to be reconsidered at some time if reduced water flow becomes the new norm. Using a 5-year moving average does reduce the effects of year-to-year variability; however, if there is a long-term trend to, for example, drier conditions, then the moving average method is not adequate, as the combination of reduced flow and reduced load will result in little change to lake P concentrations.

It should also be clarified how compliance with the long term goal will be evaluated. The report suggests that the load will be calculated as a 5-year moving average to reduce the effects of year to year meteorological and hydrological variations and provide time for the system to recover from the impact of extreme meteorological events (hurricanes). If WY2015 is the deadline for achieving the target, then the loads of WY2011 to WY2015 would be used to determine the success of the Plan. However, many of the abatement measures will be implemented during this time frame. An alternate approach would be to use loadings to WY2020 to calculate the 5-year average; this should be clarified in the report.

Because of the drought conditions, the lake stages were low which has had some ecological consequences such as 42 % decline of area covered by submerged aquatic vegetation and decline of endangered species (apple snail). Fish and fishing apparently was not affected.

p. 2 – As noted last year, mt is not the proper terminology for metric ton.

OVERVIEW OF LAKE OKEECHOBEE WATERSHED PROTECTION PROGRAM

This section provides an overview of the Lake Okeechobee watershed protection program under the NEEPP legislative act. These programs address the reduction of pollutant loadings, restoration of natural hydrology, and compliance with applicable state water quality standards. The Lake Okeechobee watershed plan was completed and submitted for approval in 2004. The plan gave a deadline of 2015 for achieving the TMDL phosphorus loads of 140 metric tons which would provide good water quality and protection of the lake ecology, assuming long-term average water inflows were maintained. The plan is updated every 3 years; 2011 was the year when the most recent update was made. Elements of the Lake Okeechobee Watershed Protection Program include (1) Lake Okeechobee Watershed Protection Plan, (2) Lake Okeechobee Watershed Construction Project Phase I and Phase II Technical Plans, (3) Lake Okeechobee Watershed Phosphorus Control Program, (4) Lake Okeechobee Watershed, (4) Research and Water Quality Monitoring Program, (5) Lake Okeechobee Exotic Species Control Program, (6) Lake Okeechobee Internal Phosphorus Management Program, and (7) annual progress reports. The work on most of these components appears to be on schedule;

Watershed construction project

There have been significant problems with the watershed construction program. The Taylor Creek Stormwater Treatment Area (STA) is now operating and the annual removal of 2.4 tons of P (in a 7-month period) exceeded the design specifications. However mechanical problems resulted in a shut down period of 18 months. Operation of the Nubbin Slough STA was initiated in 2006 but the facility has never functioned and full operation is not expected until 2012. The projected TP removal is 5 tons.

Since 2008 other projects have been initiated which include (1) implementation of the Northern Everglades Chemical Treatment Pilot Project, (2) construction and operation of several Hybrid Wetland Treatment Technology (HWTT) projects, (3) commencement of construction of the Lakeside Ranch STA Phase I, (4) implementation of the Fisheating Creek Feasibility Study, and (5) the refinement of the Watershed Assessment Model (WAM).

The Northern Everglades Chemical Treatment Pilot Project involves field-scale chemical treatment technologies to reduce TP loads in stormwater runoff. Phase II of the project is ongoing. The Lakeside Ranch STA in the Taylor Creek/Nubbin Slough Sub-watershed is expected to reduce TP loads to the lake by up to 19 tons annually; however, this project depends on access to future funding which is not, apparently, secured at this time. The Fisheating Creek Feasibility Study identifies the best mix of storage and water quality features to improve hydrology and water quality in the creek that brings, on an average, 86 tons of TP to Lake

Okeechobee annually. This is a very significant portion of the total P input to the lake; this project must move from the feasibility phase to the operational phase for there to be any chance of meeting the 140 t target load.

Watershed phosphorus control program

The watershed phosphorus control program, described also in Chapter 4, includes (1) continued implementation of regulatory and voluntary agricultural and non-agricultural BMPs, (2) development and implementation of improved BMPs, (3) improvement and restoration of hydrologic functions of natural and managed systems, and (4) use of alternative technologies for nutrient reduction. These programs are voluntary and participation ranges from 40 to 60 %. As specified in the Chapter 4 evaluation, an education and training component, along with strong incentives to obtain better participation were not addressed in the report.

Exotic vegetation control

There have been pronounced successes in controlling the exotic species, torpedograss, using chemical methods. In an earlier chapter, use of the herbicide imazamox resulted in control of another invading species (cattail) without damage to native species. Has there been any work done to determine whether imazamox will control torpedograss as well?

Internal phosphorus management program

There are about 200 million metric tons of P-rich sediments in Lake Okeechobee containing an estimated 30,000 metric tons of P in the upper 10 cm of the lake sediments. This phosphorus is typically bound to calcium, iron, or other organic matter, and in deeper lakes may not be released as long as the top sediment layer remains oxic. If the top sediment layer becomes anaerobic, massive P release from the sediment may trigger algal blooms of cyanobacteria. In addition, the lake is so shallow that wind-induced mixing must frequently result in the organic and P-rich surface layer of the sediments being re-suspended. The potential P release from the sediments into the lake is much greater than the P load from the watershed.

There are numerous cases in the scientific literature where sediment release has followed, and to a considerable extent, offset the benefits of reduced external P inputs. This is an almost intractable problem. One modeling exercise apparently estimated significant sediment P release for at least 100 years. The lake is so shallow that aeration of the bottom sediments would probably accomplish little or nothing. Dredging is not cost effective in a lake of this size. Experiments with ferric chloride and alum, the same chemicals used for tertiary treatment (P removal) at waste treatment plants, have shown some promise, but it remains to be seen whether this is practical at the scale of Lake Okeechobee. Alum, in particular, may have adverse side effects, in that accumulation of Al via the benthic food web may disrupt the biology of the lake.

WATERSHED STATUS AND MANAGEMENT

Watershed status

The most recent 5-year average P loading of 352 tons is the lowest average value since estimates were started, most likely because it includes the 3 driest years (WY2007, 2008, and 2011). Loading was particularly low in 2011 because of drought, but still above 140 t target. The highest five-year average load was 715 tons during the WY2002–WY2006 period of record. Because the abatement plan is still in the planning or initial phases, higher loads can occur if future years are wet. Because of this, the lake is highly vulnerable to becoming a hypertrophic water body.

The scale of the problem is made very clear here. Even with extensive and expensive efforts to reduce P loading, the input are still about 3 times the sustainable load. And the internal sources may very well contribute for decades. It is very hard to see how the goal of 140 tons can be met in 4 years. There are a number of other case studies in the scientific literature (e.g. Lake Taihu, China) that have comparable P loads and concentrations, and which experience hypereutrophic conditions including severe cyanophyton blooms.

Watershed management

The P load reduction activities that are currently underway will lead to a 26% reduction in P load to the lake if they are successful in accomplishing the full reduction that they are designed for. Because this will result in nowhere near the necessary P load reduction, additional activities that will reduce the P loads by a further 56% have been identified (Table 8-3 is an excellent summary). However, these additional activities are unfunded at present. If all of these proposals are implemented and all are fully successful, then a load reduction of up to 85% is feasible, a figure greater than the 65-70% needed to achieve the 140 ton target. These activities should be prioritized on the basis of P load reduction and cost in the case of partial funding. Furthermore, because each of these estimations of load reduction include uncertainties and risk such that success is very unlikely in all cases, more activities than theoretically needed to meet the target should be implemented.

WATERSHED RESEARCH ASSESSMENT AND MONITORING

Research and assessment

Nine research, demonstration, and assessment projects were underway or completed in WY2011 (Table 8-4). Of note are the results on using wetlands for nutrient removal. Average pre- and post-BMP nutrient loads indicate that of the two experimental wetlands one was a nutrient source and the other resulted in about 60 % reduction in both TN and TP loads.

A GIS-based Watershed Assessment Model (WAM) was developed and is now in β- calibration and verification phase. Results so far are very promising and the use of this model as a tool for predicting the effects of P reduction activities on P loads is encouraged.

While the wetland treatments showed marginally success at best, the Hybrid Wetland Treatment Technology (HWTT), a combination of wetland and chemical treatment approaches, is very promising and innovative. The flocs remain active after formation for a long time and can adsorb and immobilize a significant mass of TP. Flow weighted reductions of phosphorus ranged from 64 to 90 percent, which exceeds P removals achieved by constructed wetlands by a great margin (typically 50%).

Water quality monitoring

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.

LAKE STATUS

Performance measures

Despite the reduced P loading in WY2011, the lake status is poor and may be getting worse. Of 11 performance measures indicative of lake status, only 1 has been met. This is not surprising given that the P load is much greater than the required level of 140 tons. The TP concentration for the pelagic zone (most of the lake) in WY 2011 was 108 μg/L and N was 1.4 mg/L. Water clarity was low with a Secchi disc visible on 28% of the bottom area. Algal bloom frequency, in spite of lower TP concentrations due to drought reduced P load, was 16% and increasing. These data indicate that the lake is on the borderline between eutrophic and hypertrophic status, or at least the lake is highly vulnerable to hypertrophy. Apparently16% of Florida lakes is already hypertrophic. In summary, the report presents a fairly bleak picture of the current status of the lake.

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. 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). The lake P concentrations (Fig 8-12) increased continuously from the 1970's until reaching the maximum concentration of 213 μg/L in 2005 and suddenly dropped to the present just above 100 μg/L. The reduction is likely largely due to the recent multiple droughts, and as a result, the decrease may be short-term with high P concentrations returning until significant P load reduction is in place.

A minor point - the archaic units ppb and ppm should not be used.

LAKE MONITORING AND RESEARCH

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.

The lake plant population is dominated by macrophytes. Of concerns are invasive and exotic species including cattail, torpedograss, water hyacinths, water lettuce and torpedograss that can overwhelm the native biota. The case of Lake Victoria in Africa shows that water hyacinth mats can be so overwhelming that they impede with navigation by large boats.

Prolific blue-green algal blooms (most likely Microcystis) occurred in summer 2005 but have not repeated on this scale since. The conditions in the lake are favorable to the occurrence of Cyano-Harmful Algal Blooms (Cyano-HAB) and the lake is highly vulnerable. Cyan-HAB produces dangerous toxins that interfere with the use of lake water for water supply and contact recreation. They are toxic to fish and small animals and have been found toxic to humans.

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).

Fish population and composition seems to be reasonably healthy. The largemouth bass and black crappie populations have evidently recovered following the damage done by hurricanes. 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. 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.

Wading bird populations nesting and feeding relate to water level and water level fluctuations more than to any other parameter. The improvements in water level management should help to stabilize populations.

General Comments

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.

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.

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.

Recommendations

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.

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. Some chapters are already written in this way.

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.

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.

The Northern Everglades Chemical Treatment Pilot Project should be initiated in the next year.

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.

Posted: 10/24/11 at 07:48 AM by P. Dillon

Final Review Comments

Chapter 8 was reviewed primarily on the basis of the technical quality of the work carried out as part of the Lake Okeechobee Protection Plan and the interpretation of the results of these studies. However, some of the conclusions and recommendations below are based on a review of this work from an accountability perspective.

The central questions addressed in this review were:

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

Chapter 8 is generally well-written and is technically sound. The issues raised by the reviewers were, for the most part, addressed in a satisfactory manner by the chapter's authors.

Key findings:

1. The Watershed Protection Program is an important part of the overall strategy that was designed to reduce the Total Phosphorus (TP) input to Lake Okeechobee to 140 metric tons by 2015 and by doing so, restore the ecological integrity of the entire system. A key component of the Plan is the use of storm water treatment areas including constructed wetlands. Many of these projects, including those that potentially will lead to the greatest TP reductions, are either not working or represent feasibility studies that have not been implemented at this time. Without full implementation, the possibility of reaching the target TP load is greatly reduced. Another component of the Plan is the use of agricultural and non-agricultural BMPs, which have been quite effective in leading to TP reductions.

2. The TP stored in even the upper few cm of Lake Okeechobee's sediments represents a massive pool of potentially available nutrient that might be released by wind-induced physical mixing or by changes in the chemical redox state of the sediments. No viable plan is in place to deal with these possibilities and their potential consequences in terms of contributing to the input of P to the lake.

3. The low TP load to the lake in the previous year (177 metric tons) was an artifact produced by the extreme drought conditions that led to runoff of only 38% of the long-term average. Because of the extreme variability in precipitation and runoff between years, the use of the 5-year average load as the appropriate measure is justified. However, when evaluating shorter time periods, it is more useful to use a volume or total discharge-weighted load (equivalent to a mean volume-weighted input concentration) to assess the contribution of a specific year.

4. The current status of the TP load to the lake is that the 5-year moving mean TP load (352 metric tons) is still two and a half times the legislated 2015 maximum (140 metric tons) despite the fact that 3 of the past 5 years have been years of extensive drought and low TP loads.

5. The P load reduction activities that are currently underway will lead to a 26% reduction in P load to the lake if they are successful in accomplishing the full reduction that they are designed for. Because this will result in nowhere near the necessary P load reduction, additional activities that will reduce the P loads by a further 56% have been identified. However, these additional activities are unfunded at present. If all of these proposals are implemented and all are fully

successful, then a load reduction of up to 85% is feasible, a figure greater than the 65-70% needed to achieve the 140 ton target.

6. There have been pronounced successes in controlling two exotic species, torpedograss and cattail using chemical methods.

Recommendations

1. A significant portion of the TP reduction measures including implementation of some of the proposed best management practices will not be in place by the 2015 deadline. The schedule for achieving the 140 metric ton target should be adjusted to reflect this.

2. The large legacy storage of phosphorus in sediments is a potential threat. The current release of P from sediments may be significant and could be magnified by an order of magnitude if the redox potential of the upper sediment layer suddenly (e.g., by hurricanes) or gradually (by progression of eutrophication and insufficient reduction of phosphorus loads) changes. Most studies of internal loading are more than 10 years old and the question of internal loading should be revisited.

3. The TMDL load was estimated by the FDEP using old modeling tools. These models may not be adequate for water bodies that are on the border between eutrophic and hypertrophic conditions. FDEP and SFWMD should consider updated models that include, among other factors, the likelihood of formation and persistence of cyanobacteria blooms.

4. Phosphorus loads should be normalized to flow before comparison with the TMDL load. Internal TP load should be included in the estimated loadings when the loadings are compared with the TMDL load.

5. Funding for the Plan should be a high priority. Indications in the report that funding for securing full cooperation of current voluntary efforts to reduce P loads by agricultural sources may be insufficient should be addressed.

6. An Index of Biotic Integrity for the lake should be developed that takes into account multiple components of the biological assemblage.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 9

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

Posted: 09/19/11 at 08:24 AM by P. Dillon

General comments

As was the case last year, this chapter is well-written. The writing is clear and concise and easy to follow. For the most part, the chapter is organized in a logical fashion. Technically, the interpretations seem to be sound and there are no places where the interpretation of the existing data is questionable. This year, the Restoration Evaluation Project is given prominence as it should be. Many of the expectations with respect to environmental response to the construction projects were met, which is very promising.

Summary

Kissimmee River Restoration Project

This section of the chapter provides an over view of the activities related to the restoration of the Kissimmee River to a pre-channelization condition. The premise is that ecological health has been severely compromised by channelization prior to approximately 1970 and that obliteration of the middle section of C38 canal and restoration of the original river channel will reconnect the restored channel to its associated floodplain. This will, in turn, allow the associated assemblages of flora and fauna to revert to something similar to those of the pre-channelized basin. In addition to activities associated with this major restoration project, other topics such as the restoration and overall health of some of the upper lakes are provided.

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?

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?

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?

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.

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 future in cases where climate parameters are significantly outside the norm?

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.

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.

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.

In the wading bird section, the expectations have been met in some cases, which is good to see.

Comments on Figures and Tables

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.

Table 9-2: This a handy table to get a feel for the chronology of the various stages of restoration projects and the associated costs.

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.

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.

Posted: 10/24/11 at 07:49 AM by P. Dillon

Final Review Comments

Chapter 9 was reviewed primarily on the basis of the technical quality of the work carried out as part of the Kissimmee River Restoration and Basin Initiatives and the interpretation of the results of these studies.

The central questions addressed in this review were:

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

Chapter 9 is generally well-written and is technically sound. The issues raised by the reviewers were, for the most part, addressed in a satisfactory manner by the chapter's authors.

Key findings

1. As part of the Kissimmee River Restoration Evaluation project twenty-five performance measures have been defined to allow quantitative evaluation of how well the project meets its ecological integrity goal. Relatively few of these objectives have been met successfully. Of the 5 hydrology-based expectations, only 1 was fully met, while a second was met at some sites. This probably resulted from a combination of the low precipitation in WY2011 and some of the effects of construction activities. Upon completion of the restoration project, increased water storage in the upper basin is anticipated to resolve some of the hydrologic issues.

2. 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. This is a very significant portion of the total TP load to Lake Okeechobee. Although the P load of the last 5 years was 50% lower than that of the previous 5 years, this is almost certainly attributable to the 3 drought years in the last 5.

3. Alternating dry and inundated conditions increases P export 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.

4. Some of the expectations concerning recovery of fish and wading birds have been 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.

Recommendations

1. The annual variability in precipitation is substantial and needs to be planned for in terms of the expectation of meeting the hydrologic objectives of the project. Procedures should be put in place that will aid in achieving expectations in future in cases where climate parameters are significantly outside the norm.

2. Although achieving reduced TP loadings to Lake Okeechobee is not the primary goal of the restoration initiatives, the effects of the restoration on loadings should be considered at all stages of this project because it is not clear whether the net effect will be positive or negative. A new modeling initiative to address this and to lead to an estimate of expected changes in TP loading is warranted.

3. Because all of the expectations concerning performance measures related to the Kissimmee River Restoration project cannot be addressed each year, it would be useful to outline at the beginning of the chapter which ones are addressed.

PANEL COMMENTS ON DRAFT VOLUME I, CHAPTER 10

Level of Panel Review: Technical

Reviewers: J. Burkholder (AA), V. Novotny (A)

Posted: 09/14/11 at 012:52 PM by J. Burkholder

The Coastal Ecosystems are valuable resources and extremely important "end points" in evaluating all of the District's restoration efforts. The chapter design historically has been a major challenge because of the numerous estuaries included in South Florida, and because the estuaries are at the "receiving end" of all of the upstream, land-based actions that have altered watershed hydrology. In addition, there is a great deal of shared agency purview over estuaries, so that the District has relatively little control over various issues. Thus, the chapter authors have an exceedingly difficult task, made worse by the fact that the chapter contents and focus also have been "rapidly moving targets." Unfortunately, the present structure of draft Chapter 10 prevents evaluation of technical merit, as the Panel has been requested to do, because almost no supporting technical information is included. This "disconnect" is one of several fundamental problems with this year's version of the chapter covering the Coastal Ecosystems, renamed as "Coastal Priorities."

In carefully considering this year's chapter design and contents, the thought that repeatedly came to mind is that there is a difference between streamlining (a District goal identified in draft Chapter 1, regarding the various chapters of the SFER) and slashing to the point of all-but-eliminating. The latter is what has happened to coverage of the Coastal Ecosystems in the draft Chapter 10. This draft chapter does not do justice to the District's many excellent efforts in the Coastal Ecosystems, or the District personnel's strong knowledge about these resources, or the District's vision and plans regarding these systems. The following comments are offered in the spirit of helping to strengthen this important chapter about these coastal resources, which largely represent the final endpoint of success in the District's restoration efforts.

I. *Confusion Stemming from Fundamental Content and Organizational Problems*

There are some fundamental content and organizational problems with draft Chapter 10 that we felt it would be helpful to the authors to outline up front.

Chapter 10's stated purpose is to compile information and research efforts related to agency priorities about South Florida's coastal resources. The District has wrestled with this chapter for a number of years in considering how to best summarize and highlight its activities in the Coastal Ecosystems. Consequently, Chapter 10 (Chapter 12 in previous SFERs) has undergone several major "metamorphoses," from consideration of nine separate South Florida estuarine environments (the Southern Indian River Lagoon and St. Lucie River and Estuary, the Loxahatchee River and Estuary, Lake Worth Lagoon, Biscayne Bay, Florida Bay, the Caloosahatchee River, Estuary and Southern Charlotte Harbor Region, Naples Bay, and Estero Bay), to consideration of eight with plans to rotate emphasis on two to three estuaries per year, to division into Northern and Southern Estuaries with most emphasis on only a few of the coastal ecosystems (2010 SFER), and now to this somewhat self contradictory format.

a) *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).

b) *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 WY2011was 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 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).

The two key appendices are not available to the Review Panel - Readers are repeatedly referred to two Appendices to draft Chapter 10 (see lines 8, 40, 80, 82, 288, and 440), which are described to provide an update on Watershed Protection Plans for the Caloosahatchee River and St. Lucie River, respectively, including projects for improvement and restoration of hydrologic function, alternate nutrient reduction technologies, and stormwater and wastewater infrastructure updates and planning (see Chapter 4, lines 402-405, and p.4-32). Yet, these appendices are not available. They will not be available until after this review process is completed, impeding thorough review and evaluation of the chapter. The extremely short "overview" of these plans (p.10-5) conveys very little about them.

c) *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.

II. Comments by Section

<u>Northern Estuaries</u>

Information in this section is aptly presented within context of the Northern Everglades and Estuaries Protection Program (NEEPP) and coordinating agencies (the District, the Florida Department of Environmental Protection, and the Florida Department of Agriculture and Consumer Services). The legislation requires Watershed Protection Plans for the Caloosahatchee and St. Lucie Rivers. The overall goal of these Plans (lines 94-95) is to consolidate previous restoration efforts into a broader approach focused on restoring the entire Northern Everglades system." The required three-year updates to the Watershed Protection Plans for the Caloosahatchee and St. Lucie Rivers are to be contained in two appendices to the chapter (not yet available, as mentioned).

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

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).

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.

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.

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.

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 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"?

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.

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).

Posted: 10/21/11 at 06:05 PM by J. Burkholder

Summary Evaluation

The first two paragraphs of the panel's original evaluation of Chapter 10 remain appropriate to include here: The Coastal Ecosystems are valuable resources and extremely important "end points" in evaluating all of the District's restoration efforts. The chapter design historically has been a major challenge because of the numerous estuaries included in South Florida, and because the estuaries are at the "receiving end" of all of the upstream, land-based actions that have altered watershed hydrology. In addition, there is a great deal of shared agency purview over estuaries, so that the District has relatively little control over various issues. Thus, the chapter authors had an exceedingly difficult task, made worse by the fact that the chapter contents and focus also have been "rapidly moving targets." Unfortunately, the present structure of draft Chapter 10 prevents evaluation of technical merit, as the Panel has been requested to do, because almost no supporting technical information was included. This "disconnect" is one of several fundamental problems with this year's version of the chapter covering the Coastal Ecosystems, renamed as "Coastal Priorities."

In carefully considering this year's chapter design and contents, the thought that repeatedly came to mind is that there is a difference between streamlining (a District goal identified in draft Chapter 1, regarding the various chapters of the SFER) and slashing to the point of all-but-eliminating. The latter is what has happened to coverage of the Coastal Ecosystems in the draft Chapter 10. This draft chapter does not do justice to the District's many excellent efforts in the Coastal Ecosystems, or the District personnel's strong knowledge about these resources, or the District's vision and plans regarding these systems.

Unfortunately, the chapter authors (as represented by one author) were unresponsive to most of the panel's comments. Thus, the panel's comments are repeated here for comparison with the authors' further writing, which mostly cannot be accurately referred to as responses. What is shown below demonstrates that the authors combined many panel comments into several "umbrella" comments, did not mention some major comments from the panel at all, and then cursorily dismissed or ignored nearly all of the panel's comments that were excerpted. Thus, aside from a few references that the authors stated their intent to add, most of the comments that the panel originally submitted remain to be considered.

Chapter 10 remains of marginal quality, incomplete or, for some topics, lacking fundamental descriptions and documentation. The authors cursorily dismissed even the fact that this panel was asked by the District staff overseeing this review process to evaluate the technical merits of the chapter. In addition, the authors (as represented by the one author who responded) apparently were unwilling to make nearly all of the needed improvements, and unwilling to provide nearly all of the explanatory information needed. The panel's overall evaluation is that, despite the great importance of the Coastal Ecosystems to the District's many efforts, the chapter is not of sufficient quality for inclusion in the 2012 SFER because it cannot be evaluated for technical merit.

Panel Comments in Comparison to the Authors Further Writing

"Comment #1" versus the authors' further writing – As examples, the panel commented that a clear description of the District's coastal priorities is lacking, except for Biscayne Bay. The authors did not address that comment, and instead stated that the chapter "is not intended to be a strategic plan. The reviewers are referred to the District's annual strategic plan for that information." They also stated that the chapter title (Coastal *Priorities* - emphasis added) "is not perfect." Yet, the panel did not request that the authors make the chapter 'a strategic plan,' and, in fact, mentioned nothing about a strategic plan. Moreover, a chapter title that is as misleading as

this chapter title is beyond “imperfect” and needs to be changed to reflect the actual chapter contents. As another telling example, the reviewers stated that the chapter fell far short of providing the technical information needed to enable a technical review. The chapter authors did not address that comment by providing the information in the chapter and evidently had/have no plans to alter the chapter as needed. Instead, they stated, “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….” This writing indicates to the panel that there is a clear disconnect between what the panel was instructed to do in evaluating this chapter, and what the authors elected to do – which was ***not*** to provide a chapter whose contents could be technically evaluated. Second, the writing also indicates to the panel that the chapter authors plan/planned to do very little to seriously consider the panel’s comments. Third, the authors are aware that the two appendices they mentioned remained unavailable to the panel for the entirety of this review process. Yet, the chapter authors again referred to them, as though the panel should have consulted them.

“Comment #2” versus the authors’ further writing – The panel wrote as a series of comments,

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 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).

The authors' 10 lines of further writing gave only cursory consideration to a few of the above comments by the panel. The authors agreed that the "overall general problems with District estuaries should be stated," although they did not indicate their intent to do so. Despite the fact that the authors entitled this chapter "Coastal Priorities," they again referred the panel to the same Strategic Plan they had previously mentioned. Regarding other major points made above, the authors stated that the bulleted list was of additional activities, and that phytoplankton blooms are relatively rare in the estuaries, occurring mostly in freshwater tributaries and not attributed to known cause. The latter writing contradicts various publications about phytoplankton blooms in the Northern Estuaries.

Comment #3 versus the authors' further writing - the panel wrote,

"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. : 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:

- 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.

The authors' 17 lines of further writing gave only cursory consideration to a few of these comments by the panel. The authors agreed that metric units should be added, but did not state that they would do so, and indicated no inclination to alter the writing so that salinity was treated consistently throughout the chapter. They acknowledged that salinity is "technically unit-less" and indicated that their writing would not be altered. They wrote, "It is not our intent to describe comprehensive descriptions of the estuaries as that information can be found in other documents," and indicated that references would be added. However, the panel had not requested "comprehensive descriptions of the estuaries," as shown above. The authors stated that they would only be "providing an overview of results," again electing to ignore the fact that the District had instructed the panel to provide a technical review of Chapter 10. They stated that they were unaware of water quality changes that may have contributed to macroalgal blooms, and provided no substantive comments to address other major points the panel had made.

"Comment #4" versus the authors' further writing – the panel requested that an adjective for vegetation, submerged, be changed to submersed throughout the chapter, in accord with a respected scientific publication. The chapter authors declined to do so.

"Comment #5" versus the authors' further writing – Regarding the section on the St. Lucie Estuary and the Southern Indian River Lagoon, the panel requested more explanation about the chapter description that during the dry season, supplemental freshwater flows from Lake Okeechobee were not necessary to maintain salinity within acceptable levels, as the MFL criteria had been met throughout the year. The authors ignored this request and instead referred the panel to the unavailable Appendix 10-1.

"Comment #6" versus the authors' further writing – Regarding the Caloosahatchee River Estuary and Southern Charlotte Harbor (through S-79), the panel stated that a map showing S-79 is needed in the chapter. The panel also noted that, despite numerous water releases from Lake Okeechobee, salinity exceeded the MFL criterion for nearly 60% of the year, and panel requested more explanation and clear interpretation to be added to lines 115-118. The authors ignored this request and instead referred the panel to the unavailable Appendix 10-2.

"Comment #7" versus the authors' further writing – Regarding the Loxahatchee River and Estuary and Lake Worth Lagoon, the panel wrote:

> 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 and Lake Worth Lagoon [note suggested change to section title] 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.

The authors' 11 lines of further writing gave only cursory consideration to a few of these panel comments. They stated that it is not "the intent to provide a comprehensive list of problems with these estuaries" although the panel had not requested a comprehensive list (see above). Following this cursory dismissal of the panel's careful comments, they referred the panel to "other documents," unspecified. They stated that details of the project to move water to the Loxahatchee River "should be available in the SFER project database," which provided nothing by way of useful information. They also stated that "It is not the intent to provide technical details of the monitoring programs" and again referred the panel to "other documents," unspecified. The above-mentioned disconnect between the chapter authors and their District superiors is evident here, given that the chapter authors consistently refused to provide any additional information that would have enabled the panel to address the technical merit of the chapter.

"Comment #8" versus the authors' additional writing – Regarding Lake Worth Lagoon, the panel wrote:

> 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. The authors' 1 line of further writing indicated that they would add some information about the success of the Ibis Isles project. No further explanation was offered.

"Comment #9" versus the authors' further writing – Regarding Biscayne Bay, the panel wrote:

> 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.

[Note: The authors omitted a large paragraph of additional comments from the panel about the fact that excessive anthropogenic contributions of nutrients are known to affect Biscayne Bay. The panel stated that the authors' writing does little to help readers' understanding, and should briefly mention the enormous pollution pressures that affect Biscayne Bay from the extremely large adjacent human population.]

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 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”?

The authors’ 13 lines of further writing stated that a timeline for completing the second phase of the Biscayne Bay Coastal Wetlands Project is not available; that the panel should refer to Chapter 6 for information about the effects of the C-111 Spreader Project, and to the authors’ previous additional writing about salinity units; and briefly commented about the macroalgae issue. They provided nothing to address the panel’s comments/questions about pollution in Biscayne Bay, and stated only that “The pollution pressures on Biscayne Bay are well documented elsewhere” – while providing no references or other information to direct the panel, or readers of the chapter. The authors stated only that they would provide “more information about the exotic vegetation removal.”

“Comment #10” versus the authors’ further writing – Regarding the Fakahatchee Estuary, the panel wrote,

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. The authors' 2 lines of additional writing stated that a timeline for completion of the Picayune Strand Project is not clear, and that they would consider removing the information about the oyster study.

"Comment #10" versus the authors' further writing – Regarding Naples Bay, the panel wrote,

> 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"

The authors' 1 line of further writing stated that they would clarify the statement about the period of record of 15-minute data. Consistent with their treatment of the panel's comments above, the authors ignored the panel's important, fundamental comment that information needed to assess technical merit had not been given.

Final Bullets – Key Findings and Recommendations

<u>Key Findings</u>

- The Coastal Ecosystems are valuable resources and extremely important "end points" in evaluating all of the District's restoration efforts.
- Chapter 10's present structure prevents evaluation of technical merit, as the panel was requested by the District to do, because very little technical information is included. In addition, the two key appendices that were described as containing technical information were not available throughout the review process.
- The chapter authors (as represented by one author) were unresponsive to most of the panel's many comments, which were offered in an effort to strengthen the chapter.
- Chapter 10 remains of marginal quality, incomplete or, for some topics, lacking fundamental descriptions and documentation. The chapter does not do justice to the District's many excellent efforts in the Coastal Ecosystems, or to the District personnel's strong knowledge about these resources, or to the District's vision and plans regarding these systems.

<u>Recommendations</u>

- For future SFERs, District should take steps to ensure that the chapter authors seriously consider and address the panel's comments in order to restructure and dramatically improve Chapter 10.
- Despite the great importance of the Coastal Ecosystems to the District's many efforts, Chapter 10 in its present form is not of sufficient quality for inclusion in the 2012 SFER because it cannot be evaluated for technical merit.

SPECIAL REVIEW: PANEL COMMENTS ON DRAFT VOLUME I, APPENDIX 1-6

Level of Panel Review: Technical
Reviewers: G. Christakos, Y. Rubin and J. Stedinger

Posted: 09/13/11 at 12:10 PM by Y. Rubin

Background

This review provides a commentary on the document: 2012 South Florida Environmental Report, Appendix 1-6: South Florida Water Depth Assessment Tool, by Jason Godin ("Report"), and it summarizes my opinions and what I believe to be true.

Before answering the specific questions listed in the SOW, I thought I should provide some background, to lay the foundations for my subsequent comments and to provide a framework for the subsequent discussion.

There are several references that are relevant in the context of the Report is document. They are related to the two theoretical elements that form the foundation of the Report and my assessment. These elements are (1) the calculation and applicability of the head semivariogram[2], and (2) the use of kriging for point estimation. I will discuss these two elements in the Background section.

A comment on terminology. Throughout my review I will refer to heads (meaning hydraulic heads, equal to the water table in unconfined aquifers). In the context of the Report, the variability of heads is equivalent to the variability of the water table (assuming that ground surface elevation is uniform). Hydraulic head is a variable that was analyzed extensively in hydrology, and looking at these analyses allowed me to address the questions raised in the SOW using a sound theoretical base.

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. The semivariograms of the hydraulic head and the water depth would be equivalent if all the water table were measured with a reference to the same datum (reference elevation). Instead, the analysis used the ground surface elevation as datum. This would not be an issue of concern if one

[2] Godin used the term semivariance for semivariogram. I will use semivariogram here to be consistent with previous publications and common terminology. These terms are equivalent, and I assume that this is what's meant by this report. The head semivariogram would be equal to the water depth semivariogram if depths were measured with respect to a constant datum, as they should. In the semivariance plots, the units used are feet. The appropriate unit to be used is feet^2.

could assume a strong analogy between the two types of semivariogram (which would happen if the ground surface elevation would be uniform). This point will be discussed later, under Question 1. To my understanding, this should raise some concern.

The head semivariogram is discussed extensively in Dagan (1989) and in Rubin (2003). Additional discussion is provided in papers co-authored by Rubin and Dagan in the late 1980s. An extensive reference list is provided in Rubin (2003) and additional discussion is provided in Rubin (2003, Chapter 4). Of particular relevance in this context are the two papers by Rubin and Dagan (1988, 1989), and additional papers by Rubin and Seong (1994) and Seong and Rubin (1999). These publications provide a theoretical perspective on the nature of the head semivariogram.

The findings from these publications that are relevant to the questions listed in the SOW are as follows:

1. The head[2] semivariogram is anisotropic and it depends on the angle between the lag distance vector and the mean flow direction.

2. The head semivariogram is affected by the presence of physical boundaries;

3. Head boundaries and flux boundaries affect the semivariogram in different ways, but in any case they make it non-stationary.

4. In theory, the semivariogram could be considered to be stationary in infinite domain or at semi-infinite domains only at distances larger than ~ 2 integral scales of the logconductivity.

5. The behavior of the head semivariogram depends on the pattern of spatial variability of the hydraulic conductivity. Differences in the geology (e.g., in the average conductivity) could lead to non-stationarity. A semivariogram cannot be expected, in theory, to be applicable over different geological domains.

6. The head semivariogram depends on the mean head gradient. The head semivariogram is linked with the underlying head gradient model. When the head gradient changes, the semivariogram model would also change.

7. The head semivariogram is differentiable at the origin. However, measurement error could lead to a nugget effect, which make the origin non-differentiable.

8. Semivariograms can change with time. For example, the magnitude of recharge affects the shape of the semivariogram. If the mean flow direction would change, or of the magnitude of the gradient would change, the semivariogram would also change. They cannot be assumed to be stationary in time except after testing.

9. Pumping could introduce strong non-stationary effects, and these effects should be filtered (unless the water level is allowed to recover prior to measurement).

The principles of geostatistics are discussed in several publications (c.f., Rubin, 2003, Chapters 2 and 3; Isaaks and Srivastava, 1989; Christakos, 2000). A few of these principles that are of particular relevance in the context of this Report are as follows:

10. Semivariograms are statistical tools that are relevant for stationary or second-order stationary variables.

11. Semivariograms are inferred (determined) by assuming stationarity.

12. The properties of kriging estimators predicate on the applicability of their underlying assumptions.

13. There are several kriging estimators that are mentioned in the literature, and they are different in terms of their underlying assumptions. A kriging estimator should be employed only after testing how it conforms with the underlying assumptions. In our discussion here head semivariogram refers to head-residuals semivariogram. A residual is the difference between the head and its expected value.

14. When the underlying assumptions are met, the kriging estimators could be applied with some confidence about their predictive capabilities.

15. Stationarity is a key assumption in geostatistics and the consequences of violating it must be evaluated.

16. Violating stationarity could possibly be less consequential in high measurement density areas. But in such areas there is no estimation problem: we could get away with a lot. The challenges are, (1) estimation in the low- and medium-density areas, and (2) how to define high-density, medium-density and low-density areas.

17. Cross-validation is a powerful tool for testing, comparing and for gaining confidence in kriging estimators. There are a few well-documented procedures for conducting cross validation (cf., Section 13.3.7 in Rubin, 2003; Isaaks and Srivastava, 1989, Ch.15).

18. The depth to the water table could change with time. And that means that the corresponding semivariograms would also change with time.

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.

Responses to the specific issues stated in the SOW are provided below.

Response to Specific Questions

1. Is the use of geostatistical methods appropriate for the prediction of water elevation surfaces in each of the major hydrographic/physiographic features presented?

a. Lakes: Lake Okeechobee (very low relief: ~0.01 inches per mile)
b. Wetlands: Everglades Landscape (low relief: ~0.1 inch per mile)
c. Floodplain: Kissimmee River (low relief: ~0.5 inch per mile)
d. Aquifer: Biscayne/Surficial (variable relief)

Response: 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.

On a theoretical basis, there is no justification for not removing this effect. Theory also suggests what could be effect of the relief on the semivariogram. For a lag L, this effect would add variability of the order of ~$0.5(aL)^2$ to the semivarioram, where a is the relief (which is assumed here to be constant over the relevant domain). And so from a practical point of view, this effect is probably negligible in cases (a) and (b). It could also be negligible in environments with high-density of measurements (see discussion on that in Background). This needs to be tested by simulating various scenarios and the project managers could then decide if the error introduced by the relief is acceptable.

Additionally, there are all the other issues discussed in the Background section that must be addressed.

2. Does the exact kriging (no nugget value) method provide benefits over other interpretation methods (e.g., Multiquadric Spline, Inverse Distance Weighting) with regard to:

a. Precision (e.g., validation)?
b. Uncertainty (e.g., standard error surfaces)?

Response: A general statement that could be said on kriging is that in theory kriging is a very good (or best, in the sense of the mean squared error) linear unbiased estimator. There is no theory, to my knowledge, that would indicate its superiority compared to other methods, especially non-linear models. Furthermore, when the assumptions that underlie kriging are violated (see Background), then all bets are off. Recall now that the assumption of stationarity (in space and in time) is violated here (see discussion in the Background section), and so even the superiority of kriging among linear estimators is questionable. One cannot assume a-priori the superiority of kriging compared to the other models without testing.

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. Based on Google search, I think that some information is given here:

http://msdn.microsoft.com/en-us/library/bb895174.aspx

But I am not sure. That reference suggests several parameters that could be used to define the cross-validation procedure, but they are not documented in the Report. In any case, a complete documentation all the steps is needed, including:

- The relative size of the testing set and training set;
- Whether or not several combinations of training and testing sets were used;
- The geometry of the search neighborhood;
- The position of the points included in the testing set, including close to and far away from measurements and from physical boundaries;

I would like to see all the graphics discussed in Point 17 from Background before being able to answer this question. Furthermore, this should be done repeatedly, for multiple testing and training sets. Additionally, I would like to see the results from evaluating the effects of transients (does the method work well under transient conditions, although it is implicitly assumed to be under steady state?) and the effect of the relief and physical boundaries.

The term “cross validation RMSE” was not defined in the Report. Please provide details and definitions.

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 nondifferentiable.

3. Should anisotropy (property of being directionally dependent) be incorporated into limited range, water elevation surface interpolations? If so, please explain how this would be best accomplished.

Response: 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.

4. Should surface water elevation models be interpolated across hydrologically distinct basins or interpolated independently (e.g., Everglades implementation basins)? Please explain why.

Response: Boundaries between basins introduce non-stationarity, and geostatistical models are not built to handle such situations (see discussion provided in Background). Different basins could be expected to produce different semivariogram models and these models are not built for transitions.

Ad-hoc strategies could possibly be developed to handle transition between basins. For example, Isaaks and Srivastava (1989, p.348) suggest that modifying the search neighborhood in the vicinity of boundaries could be helpful.

In another example, Rubin and Dagan (1988, 1989) in their investigation of the effects of boundaries on variability developed and tested successfully strategies for modeling the effects of boundaries on variability. These strategies are built around the idea of modeling the boundaries as a series of fictitious measurements that are placed along the boundaries. What could work here is analyzing each basin with its semivariogram models, working with a limited search neighborhood near the boundaries, avoiding kriging on heads (or depths) across the boundaries and conditioning on the measurements representing the boundaries.

5. Are there any other geostatistical approaches that could more appropriately capture the physical or gravitational based properties of water surfaces (e.g., kriging with external drift (KED) or block kriging)? Please explain why these approaches would be more appropriate for capturing these properties.

Response: 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.

For example, I discussed early on the issue of stationarity. It is a major issue that should be handled with care. At the same time, it is possible that one could conclude (from cross validation) that a stationary model could work well when coupled with a limited-size search neighborhood.

Specific notes: Kriging with an external drift implies the presence of a secondary variable that is used for determining the trend (i.e., the drift). Your question does not provide information on this variable. This question opens the door to explore several options:

- Surface and airborne geophysics: for a general textbook on the subject, see Rubin and Hubbard (2005).
- Along this line, an application of Ground Penetrating Radar (GPR) technology for mapping the water table is described in Hubbard et al. (2001). However, I suspect this technology is of limited applicability here because it will be difficult to employ over very large areas. But it could be used for providing information in areas with low density sampling and where the water table is shallow. Its applicability depends on the soil properties.
- A promising technique is the work done by NASA see links:

  http://www.jpl.nasa.gov/news/news.cfm?release=2009-194

  http://news.nationalgeographic.com/news/2010/02/100217-groundwater-crisisnasa-satellites-india-environment/

It could be used as the secondary variable on KED, but its accuracy needs to be determined. I urge you to consider it.

- Perhaps the most promising venue in the short term could be using a groundwater flow model as the secondary information needed in a KED formalism. The flow model could be used to model the drift (average, or smoothed, depth to water table surface), whereas the semivariogram could be used to condition the drift locally on point information (such as locally-measured water depth). This strategy could open the door for bringing the underlying dynamics (physics) of the aquifers into the estimation process.

6. Are there (a) maximum water elevation gradients or (b) maximum distances between interpolation points that would preclude the use of geostatistical approaches to predicting water elevation surfaces? Please explain what these maximum gradients and distances should be between interpolation points relative to the different hydrographic / physiographic implementations.

Response: 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.

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.

7. What are the advantages and disadvantages of applying geostatistical approaches to predict water elevations as opposed to hydrologic or hydraulic simulation models?

Response: When applicable, geostatistical models offer the advantage of computational convenience. Also, when applicable, they could be considered as the good linear unbiased estimators (because of the concepts employed in their derivation). There is no guarantee that this would always be the case in application.

There are several disadvantages:

- They do not provide any measure of uncertainty. And so no confidence interval could be set around the kriging estimates.
- A map of kriged depth estimates should not be viewed as representing reality. It is much smoother. To see that this is indeed the case, one can try computing the semivariogram of the kriged depths. It will not conform to the semivariogram computed form the data.
- The kriging approach adopted here cannot be applied under changed conditions, e.g., to transients or flow direction.

Hydrologic models could include a groundwater flow model, one that could possibly integrate between the saturated zone, the vadose zone and land surface processes. A model that covers the saturated zone could provide a very good starting point.

Groundwater flow models models have the advantage of being based on physical principles and they can be used to model the depth to the water table under changing conditions. They can be applied under a broad range of conditions and are not as restricted as the geostatistical approach by assumptions such as stationarity, steady-state, distance from boundaries, the need to filter local effects such as pumping etc.

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.

8. What other issues or types of limitations should be placed on the use of these geostatistically based water elevation tools with respect to their application in (to be provided in the Application section of the draft document):

a. Water resources planning and evaluation?
b. Operational decision support?
c. Permit support?
d. Emergency management?
e. Communications with intergovernmental and extra-governmental (e.g., print and video-based media) organizations?

Response: I am aware of the issues underlying this question based on my work in the area of environmental information management systems. This work is documented in great detail in http://info.webh2o.net (for full disclosure: I am the president of WebH2O, LLC). We, at WebH2O, thought quite a bit about these issues and what we offer to our users is a solution that allows the administrators to decide who can see what and when, and what they can do with the data. Several types of users could be defined with each type being assigned different privileges in accessing data. There are several user types with different privileges assigned to each. Privileges can be changed over time. They can also vary between regions.

Additional Comments:

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.

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.

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.

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.

References

Christakos, G., 2000, Modern Spatiotemporal Geostatistics, Oxford University Press.

Dagan, G., 1989, Flow and Transport in Porous Formations, Springer.

Hubbard, S., Chen, J., Peterson, J., and Y. Rubin, 2001, Hydrogeological characterization of the South Oyster Bacterial transport site using geophysical data, Water Resour. Res., 37(10), 2431-2456.

Isaaks, E.H., and R.M. Srivastava, 1989, An Introduction to Applied Geostatsitics, Oxford University Press.

Rubin Y., and G. Dagan, 1988, Water Resour. Res., 24(10), pp. 1689-1697, 1988.

Rubin, Y., and G. Dagan, 1989, Water Resour. Res., 25(4), pp. 707-712.

Rubin, Y. and K. Seong 1994, Water Resour. Res., 30(11), 2901-2912.

Seong, K., and Y. Rubin, 1999, 35(4), 1011-1018.

Rubin, Y., 2003, Applied Stochastic Hydrogeology, Oxford University Press.

Rubin, Y., and S. Hubbard, 2005, Hydrogeophysics, Springer.

Posted: 09/13/11 at 02:06 PM by G. Christakos

Discussion of SFER

1. As described in the *South Florida Environmental Report* (SFER), the *South Florida Water Depth Assessment Tool* (SFWDAT) produces useful spatially continuous estimates (surfaces or maps) of mean daily surface water elevations for nine hydrologically distinct basins of the Everglades Protection Area, Lake Okeechobee, and a partially restored portion of the Kissimmee River floodplain (Fig. 1 of the SFER report) based on a large number of real-time surface water and groundwater level gauges. These surfaces provide valuable information to the real-time management of the Central and South Florida (C&SF) regional flood control system.

2. One cannot simply throw a set of water level data into the first available spatial estimation technique and hope to a get a quick and satisfactory solution. In the real world, one eventually gets a water surface that is most suitable to the dataset through understanding of the data and selecting the most adequate spatial interpolation technique based on comparative evaluation. The latter involves numerical cross-validation criteria, like the minimum root mean square error (RMSE) one. This is the SFER *methodology*, which is in the right direction. Yet, a few of its aspects need clarification and certain others could be improved.

3. To achieve its goals based on the available information (as described in Volume 1 of SFER and elsewhere), SFWDAT relies on the *linear geostatistics* processing of gauge data. This is done in three steps. The first step is data evaluation that offers insight on how data are distributed in space, and obtaining some initial hints on which interpolation method may be suitable for the specific site. The second step is to consider several candidate techniques and chose the interpolation technique that is most suitable to both the dataset and the study objectives. The third step is the comparison of the results using cross-validation, and the subsequent determination of the most suitable technique. As SFWDAT users gains experience and improve their knowledge of spatial interpolation techniques, the time required to derive the most suitable water surface should be reduced considerably.

4. It is argued in SFER that (lines 77-79), “The interpolation of water elevation surfaces from water level gauges is the key process that allows the SFWDAT to paint a spatial perspective of regional water elevations in relation to land surface (water depth).” For this purpose, SFWDAT’s spatial estimation “tool of choice,” so to speak, is *ordinary Kriging* (OK), which is favored over U*niversal Kriging* (UK) and other techniques.

4.1 This choice makes sense, in view of (*a*) OK’s substantive advantages over other interpolation techniques (inverse weighted distance, triangulation, polynomial functions, splines etc.), and (*b*) the observation that (lines 112-113) “the universal kriging method was significantly more complex to implement as an automated and uniform approach.” As a matter of fact, since the study areas are generally wetlands with relatively low relief and slow water speed, many hydrologists would find it reasonable to use linear geostatistics in the mapping of water elevation mapping.

4.2 In making its choice SFWDAT relies on testing the OK predictions in terms of RMSE, minimum error variance fit, observed-predicted scatterplots etc.

5. At this point, one could make a few initial remarks about SFER’s overall reasoning.

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.

5.2 Two crucial study elements that influence the quality and realism of the generated spatial estimates (surfaces) of water elevations are: *uncertainty quantification* and *composite space-time* (CST) effects. Otherwise said, this is space-time reasoning under conditions of uncertainty.

6. 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.

6.1 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.

6.2 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.

6.3 Also, water level uncertainty may be due to methodological reasons: a purely spatial analysis is considered that systematically ignores *cross* space-time effects.

6.4 Lastly, conceptual uncertainty can be the result of modeling (e.g., linearity) assumptions associated with the OK technique.

7. About the CST effects: cross space-time effects play a key role in the quality and informativeness of the water elevation estimates generated on the basis of gauges and, potentially, other kinds of *core knowledge* (hydrology laws and empirical models) and *soft data* (interval or probabilistic records, secondary information).

7.1 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.

7.2 In fact, CST variation of in situ water depths (and other performance indicators) needs to be better understood. Accordingly, the hydrogeologic study of the South Florida environment could go beyond the separable space vs. time considerations into the domain of physical *synthesis* of space and time. In particular:

7.3 Essential CST features are neglected by an ecosystem study that separates space and time without a sound justification. One of them is the way the physical *space-time distance* enters hydrologic modeling. Due to connectivity within and across basins, what happens in one site at a certain time can affect what happens at another site at a future time. These effects may be a function of the composite space-time distance and direction rather than a function of separated space lags and time intervals.

7.4 As a matter of fact, SFER's suggestion that one must (lines 8-9) "understand present (real-time) and historical hydrologic conditions in the South Florida environment from a regional *spatial perspective*. Additionally, there is an ongoing need for this type of information on a *year-round* basis" indicates an appreciation of the synthetic structure of the temporal and geographical components of the ecosystem.

7.5 In lines 75-76 of the report it is written that, "Produce spatiotemporal graphical, statistical tables and code-based output for Google Earth and web browser-accessible animations." This is further evidence that the consideration of a systematic space-time framework should provide insight that can lead to more realistic ecosystem indices.

7.6 Modeling uncertain hydrologic conditions in South Florida using an integrated regional-temporal perspective allows a better physical understanding of the dynamic pattern and distribution of water depths across entire ecosystems (wetlands, lakes, aquifers etc.). In turn, a better understanding of the CST aspects of the ecosystems can improve the *quality* of information needed for operational decision support during flood events as well as during normal conditions to improve the routing of water for water supply needs and environmental benefits.

7.7 As a matter of fact, there are several real world case studies demonstrating the superiority of the CST-based predictions of *space-time geostatistics* over those obtained from purely spatial statistics or geostatistics. Also, there is publicly available space-time geostatistics software that could be considered in the South Florida study and see if it fits the specific in situ needs. For example, CST *variogram models* of water levels and related variables can be calculated using this software (that includes a list of CST variogram models and space-time Kriging options), see also §17.

8. 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?

8.2 Another scale-related issue worth SFER's attention is the spatiotemporal dependencies between short-term observations and cross-dependencies between short and long-term water level data.

8.3 For the scale problem of the SFER data, the *scale uncertainty* may be a significant factor. For example, in the case of basin data with a distinct trend and a large number of missing values (because, e.g., most monitors only record during certain time periods), the estimation of long-term values can be seriously biased. This possibility is apparent in the report's comment (line 68), "temporal gaps based on point-to-point regressions for short term gaps and a correlation approach based on adjacent gauges for longer-term gaps."

8.4 How does the standard deviation of the differences between water level measurements and estimates change as the estimation time scale changes? A possibility is that the standard deviation decreases as the time scale increases. Then, one may infer that this is because the aggregated hard gauge data as well as the soft information (emerging as the time scale increases) can lead to a reduction of the water level estimation uncertainty and produce more informative estimates for real-time C&SF system management purposes.

9. 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. Yet, these measures are known to characterize the distribution of groundwater flow variables like hydraulic conductivity, for example.

9.2 OK restricts the search for an optimal spatial water level estimator only among *linear* ones. "The selection of ordinary kriging methodology as the principal interpolation approach for SFWDAT" (lines 115-116) avoids using *non-linear* estimators like *disjunctive Kriging* (DK) and *Bayesian Maximum Entropy* (BME) space-time estimation. This is a decision probably made in view of the relative complexity of DK and BME compared to linear Kriging techniques.

9.3 Linearity in spatial estimation is surely a very convenient property (especially regarding automating processing, which is a SFWDAT priority). Yet, this can be a restrictive property that has its consequences, like, neglecting non-linear spatial links and leading to increased estimation error. 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.

9.4 The effects of the linearity assumption can be seen in certain parts of the report. SFER's validation results show that less accurate water level estimates are observed in the *boundaries* of the study domain (lines 173-177; and Figure 10). This is probably due not only to the physical reasons discussed in SFER (e.g., open conveyance), but also to OK's linearity assumption: spatial estimates are obtained using a linear combination of the data.

9.5 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?

10. An issue worth examining in connection with OK is the computation of the *raw* or *empirical* variogram. In fact, the raw variogram and its *physical interpretation* are key components of the OK process, since they affect decisively the way OK weights are assigned to data points during interpolation, and, consequently, the variogram also has a significant effect on the quality of the OK outcomes (water surfaces etc.).

10.1 Fitting a theoretical model to the raw variogram values requires hydrologic judgment, and often requires a certain number of "trial-and-error" computations. According to SFER, "several different variogram models were evaluated to be utilized as a consistent approach to modeling the basin specific spatial dependency." (lines 137-138ff). This is a common practice in geostatistics.

10.2 Yet, one knows from experience that it is not always easy to ascertain whether water level variogram values actually provide an adequate representation of the system's spatial variation. No doubt, the reasons for choosing a particular variogram model to fit the experimental values are often difficult to explain in terms of basin processes. They can only be rationalized *technically* in terms of least-squares or maximum likelihood fits to the raw values. This is the case with SFER's variogram modeling and inference.

10.3 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?

11. We noted earlier that SFER uses cross-validation criteria to test the OK estimates of water level. In Figure 3, OK seems to perform better than UK, at least in terms of RMSE values (0.1342 for OK vs. 0.3092 for UK). From a methodological standpoint, since OK is a special case of UK, a more general model should be at least as good as its special cases. Loosely speaking, if OK has a range of applicability, say *RoA*(OK), that is smaller than that of UK, i.e. *RoA*(UK)>*RoA*(OK), this means that UK works as well as OK within the *RoA*(OK), but it also works outside it (within *RoA*(UK)) where OK is useless.

11.1 Is it possible that the improved OK performance is linked to *computational* factors? For example, the simpler OK is more accurately and easier programmed in software terms than the more complex UK whose programming may introduce considerable computational difficulties.

11.2 Could the report's cross-validation testing include numerical comparisons between separable space-time analysis (see SFER) versus composite space-time geostatistics? One can test *regional-temporal* water level estimation accuracy (separable vs. composite) in terms, for example, of mean/median errors and success rates of passing the chi-square tests of uncertain information. Such cross-validation tests could be enlightening.

12. Surely, model choice based on *simplicity* is a rather controversial issue, especially when this choice is based solely on statistical tests. In light of the evidence so far it would make sense to test the performance of OK predictions beyond statistical testing, in terms of their *consistency* with physical laws that govern the site's dominant processes.

12.1 For example, there are cases in practice where Kriging cannot predict water elevations accurately, as it does not take into account the aquifer dynamics and changes in boundary conditions. In some aquifers it is almost impossible to predict properly piezometric heads. Surely one can draw water elevation contours with the help of interpolators, but afterwards one may need to check them "by hand."

12.2 The above considerations encourage future investigations to consider water level estimation techniques that can readily incorporate groundwater flow models and empirical relationships. In addition to the gauge measurements, there may exist valuable information about the site in terms of empirical charts, histograms, intuition and familiarity with the specific aquifer, wetland etc.

13. The variogram is a tool that measures degrees of spatial correlation among data points in a site as a function of (*a*) the distance and *(b*) the direction between data points. Aspect (*b*) is known as spatial *anisotropy*.

13.1 Interestingly, an isotropic spatial pattern is assumed in SFER (lines 104ff). Although the reasons for this assumption are rather apparent, yet it deserves some testing. Anisotropic behavior can be strong even at short distances, as is depicted by differences in the short-range behavior of the spatial variograms (used in Kriging) along different directions. One would argue that the anisotropy of water level observations be always checked when using geostatistical methods. From experience, anisotropy can be more clear in cases of high reliefs in which the longer range is along the direction of the largest hydraulic gradient (e.g., floodplain).

13.2 If SFER's calculations assume spatial *homogeneity* of water level distribution, how can one explain the linear variogram behavior at larger distances (see relevant plots in SFER)?

14. OK assumes that the local water level trend is constant but unknown. Instead, in sites where the trend is known, could the *simple Kriging* (SK) technique of geostatistics be a better choice for these sites? Linear SK is as easy and automatic to apply as linear OK.

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. This should be clearly reflected in the corresponding variograms of SFER (see, for example, the case of the Gaussian variograms). Indeed, it is stated that, "the Gaussian model was selected as the standard variogram model to be utilized by the SFWDAT" (pages 142-143). This means that water elevations have a continuous and smooth spatial variation that is homogeneous as well.

16. Kriging has two other key properties: *de-clustering* and *de-screening*. The former property assures that data points close to each other are assigned collectively the weight of a single datum located near cluster's centroid. The latter property implies that the unrealistic influence of a data point will be reduced by adding one or more data points at the intermediate locations between the monitoring and estimation points. Given that the screen effect makes the contribution of distant gauges negligible, the use of a monitoring subset in Kriging is a safe practice compared to other spatial weighting techniques. It is not clear in SFER whether clustering and screening were real spatial mapping issues for the available gauge data (?)

17. In sum, SFER data analysis is surely interesting and useful, still, it could be possibly improved in certain ways, including:

(a) Quantifying multi-sourced uncertainty of water level distribution and relevant variables.

(b) Considering composite space-time variation in site's description.

(c) Incorporating core knowledge (groundwater laws, hydrologic models).

(d) Testing non-linear estimators.

(e) Accounting for spatial anisotropy and possible heterogeneities.

17.1 In the context of these potential improvements, one could consult the modern spatiotemporal geostatistics literature and software libraries. For example, publicly available *space-time geostatistics* toolboxes (e.g., *STBME*) have been developed under the open-sourced geographical information system framework (i.e., *Quantum GIS*) and, therefore, they can seamlessly operate/interact with many other built-in GIS functions.

17.2 Potential advantages of the STBME toolbox for spatiotemporal water level modeling purposes include the following:

(a) The toolbox is fully integrated with Quantum GIS, and its input/output files can be in multiple formats, for example, text files and shapefiles. In addition, the outputs of the toolbox are easily exported into interactive mapping tools such as Google Earth or Google Map, which are of interest to SFER and SWDAT.

(b) STBME includes realistic CST tools that can express spatiotemporal dependence of water level and other ecosystem attributes directly from a composite space-time dataset. The toolbox can analyze an in situ South Florida dataset in terms of realistic spatiotemporal covariance models rather than purely spatial models.

(c) Spatiotemporal water elevation dependences can be represented in terms of nested variogram models that account for ecosystem dynamics in different yet interconnected spatial and temporal scales.

(d) Both core knowledge bases and uncertain secondary information can be integrated into the ecosystem analysis. The STBME toolbox accounts for core knowledge bases in the form of physical laws and hydrologic models, and for information expressed in interval or probabilistic forms (for example, datasets with upper and lower bounds, or dataset histograms).

Questions for specialized SFER reviewers concerning draft Appendix 1-6:

1. *Is the use of geostatistical methods appropriate for the prediction of water elevation surfaces in each of the major hydrographic/physiographic features presented?*

a. Lakes: Lake Okeechobee (very low relief: ~0.01 inches per mile)
b. Wetlands: Everglades Landscape (low relief: ~0.1 inch per mile)
c. Floodplain: Kissimmee River (low relief: ~0.5 inch per mile)
d. Aquifer: Biscayne/Surficial (variable relief)

The application of a spatial interpolation technique, including those of geostatistics, should be substantive rather than "black-box." Accordingly, a good understanding of the physical characteristics of a site is a key prerequisite before any spatial estimation technique is used, and this includes both technically simple and sophisticated techniques. For example, in many cases it can be difficult to predict elevations in flat terrains. When one needs to assess the interpolation errors against the elevation differences, the advantages of various interpolation schemes, including the OK technique, need to be reevaluated. It is also problematic to mix data from different hydrologic basins in such interpolations. Frequently, the main physical features of the particular phenomenon (hydrologic laws of change, hydrographic/physiographic features etc.) require implementation of advanced, physical model-based geostatistics. Similar is the case, the site is characterized by strong space-time cross effects. Otherwise said, in cases like *a-d* the direct implementation of a mainstream spatial interpolation technique may not be adequate, and often needs to be aided by substantive knowledge. Purely data-driven techniques are replaced by knowledge (core and site-specific) synthesis methods for interpolation purposes. This is a general assessment of the situation *a-d* above, whereas in situ information is necessary to proceed to a specificatory assessment.

Having said that, a knowledgeable choice of a spatial interpolator could decide among a rich set of geostatistical techniques the one that is most appropriate for the prediction of water elevation surfaces. One can consider (*i*) linear vs. non-linear estimators, (*ii*) Gaussian vs. non-Gaussian distribution, (*iii*) data processing vs. knowledge bases (core and site-specific) synthesis, (*iv*) homogeneous vs. heterogeneous variation, (*v*) scalar vs. vector models, and (*vi*) spatial vs. spatiotemporal domain. One technique is more appropriate than another given a particular dataset, because of its inherent modeling assumptions and algorithm design for spatial estimation purposes.

There are techniques that assume different combinations of the (*i*)-(*vi*) above. Simpler techniques of *traditional* geostatistics (e.g., OK and SK) conveniently assume a linear spatial estimator, Gaussian distribution, hard data processing, and homogeneous variation in a spatial domain. The random field representing water elevations should be continuous and homogeneous for a linear function to be accurate (like linear OK ; e.g., lines 107-108, "the relatively homogenous or flat water elevations in impounded areas such as WCA-1"). More advanced techniques of *modern* geostatistics (e.g., BME, EME, and MCMF) do not require restrictive assumptions such as above, i.e., they can use a nonlinear estimator, non-Gaussian distribution, knowledge base synthesis, heterogeneous variation in a composite space-time domain. The matter is discussed in more detail in the attached reviewer's report.

2. *Does the exact kriging (no nugget value) method provide benefits over other interpretation methods (e.g., Multiquadric Spline, Inverse Distance Weighting) with regard to:*

a. Precision (e.g., validation)?
b. Uncertainty (e.g., standard error surfaces)?

Geostatistics accounts for dependence of water level values across space rather than simply fitting a deterministic function to the data. It seeks physical understanding, whereas previous spatial interpolation methods (inverse distance weighted, trend surface etc.) are superficially easy to understand with respect to the way the water surfaces are estimated. The advantage of geostatistics can be particularly apparent in many cases of aquifer head modeling, because high head variations can be easily over-fitted by deterministic functions (including inverse distance weighting etc. techniques).

The weights of the inverse distance weighted techniques depend only on the distances between locations and ignore the actual measurements, and also they do not account for the screen effect, clustering and anisotropy. The use of these techniques can have consequences in both surface precision and uncertainty. Generally, deterministic interpolation tools do not offer meaningful measures of interpolation uncertainty, and are easily affected by uneven monitoring gauge networks since the same weight is assigned to each of the monitoring locations, even if it is in a cluster. Also, maxima and minima in the interpolated surface can only occur at monitoring locations since inverse distance weighted interpolation is by definition a smoothing technique. Non-geostatistical interpolation methods like, for example, trend surface are highly affected by extreme values and uneven distribution of monitoring locations, are smoothers, and rarely pass through data points. The situation is further complicated when some monitoring locations are more informative than others (e.g., data at peaks are more significant than those on a plain or a slope).

Geostatistics estimation does not have many of the above shortcomings. A prime advantage is its ability to account rigorously for surface uncertainty, in which case a certain level of confidence can be place in the water elevation estimates of Figure 8 and others. In practice, there are many applications in which the spatial accuracy of Kriging estimates is better than that of other techniques. This is due to geostatistics properties, such as the systematic consideration of spatial correlation among data values (no nugget is necessary in the case of continuous water level variation) rather than merely using data coordinates as many other techniques do. Other Kriging properties, including declustering and de-screening, are discussed in the reviewer's report.

3. *Should anisotropy (property of being directionally dependent) be incorporated into limited range, water elevation surface interpolations? If so, please explain how this would be best accomplished.*

One has to determine what kind of anisotropy one is dealing with at each site. For example, in zonal anisotropy the sills of the directional variograms vary with direction, whereas in geometrical anisotropy the sills are the same but are reached at reached at different ranges. The two different anisotropies may have different impacts on the in situ implementation of the OK technique and its outcomes.

As far as water surfaces are concerned, spatial anisotropy may occur due to the nature of data. To gain some empirical insight, one could calculate the variograms along the 4-6 principal directions. On the other hand, once a physical law or an empirical model is included in water surface modeling, the Kriging technique could be applied on the residuals, originally under the isotropic assumption.

Anisotropic behavior is sometimes more profound at short dependence ranges, as is depicted by variogram differences at small lags along different directions. Short-range variogram values contain information about the spatial continuity of the physical process, and are inputs to the OK equations, which implies that if considerably different variogram values characterize the process along different direction, this difference will have an impact on the estimated surface and its accuracy. In the case of smoothly varying processes, it is likely that the difference is negligible, whereas in the case of processes with higher variation, the difference must be taken into consideration. Anisotropy can be more clear in the case of high reliefs in which the longer range is along the direction of the largest hydraulic gradient, e.g., floodplain.

In sum, potential anisotropic features of water level observations should be checked when using geostatistical methods, because they can be consequential in surface estimation.

4. *Should surface water elevation models be interpolated across hydrologically distinct basins or interpolated independently (e.g., Everglades implementation basins)? Please explain why.*

Although parameters such as boundary conditions of water elevation models may be meaningfully interpolated, water elevation interpolation across hydrologically distinct basins is usually not rigorous in terms of linear techniques. In general, the interpolation of hydrogeologic variables across distinct physical domains can be a particularly challenging affair. Naive interpolation between distinct domains is known to often provide physically nonsensical results. In flat surfaces it is problematic to mix data from distinct basins for interpolations purposes. Intuitively, it is rather clear that when the parameters of distinct basins have different continuity characteristics, water elevation models and spatial estimators should be considered independently. If the sites are interconnected with moderate response times that are of the same order, it may be worth trying model-based Kriging.

5. *Are there any other geostatistical approaches that could more appropriately capture the physical or gravitational based properties of water surfaces (e.g., kriging with external drift (KED) or block kriging)? Please explain why these approaches would be more appropriate for capturing these properties.*

Recall that Kriging with external drift (KED) is essentially equivalent to universal Kriging (UK), which has already been used in SFER (lines 91ff) although no physical laws or models have been integrated in UK's structural component. In particular, in the lines 110-112 of the report is pointed out that, "the universal kriging approach most appropriately captured the flat-pool like surface of this basin." On the other hand, SFER also argues that, "the universal kriging method was significantly more complex to implement." These and similar considerations imply that the SFER authors should possess considerable insight about UK/KED's performance in the specific South Florida sites.

Now, since physical models can capture the gravitational properties of water surfaces, KED may account, at least partially, for dominant basin features as long as external drift is linked to the available core and site-specific knowledge about in situ dynamics. Otherwise said, Kriging with physical model-based external drift could perform better than OK, especially in sites with clearly defined boundary conditions, e.g. floodplains. As far as highly uncertain sites are concerned --e.g. wetlands and aquifers— the high uncertainty of physical model parameters may weaken the benefits of physical model incorporation. Block Kriging is useful when one is dealing with areal data.

In modern geostatistics one finds space-time estimation techniques (BME, MCME, MBMF etc.) that have been developed with the specific goal of rigorously integrating physical laws and properties of the basin. This can be a key issue in many real world applications. For example, in the case of highly skewed lognormally distributed sites, where no reliable variograms could be produced on the basis of the available dataset, the variogram could be derived directly from physical laws and empirical models. In addition, modern geostatistics techniques do not make Kriging's restrictive assumptions like normality of the probability model and linearity of the estimators. On the other hand, the implementation of the more advanced techniques requires skilled users. See also the reviewer's report.

6. *Are there (a) maximum water elevation gradients or (b) maximum distances between interpolation points that would preclude the use of geostatistical approaches to predicting water elevation surfaces? Please explain what these maximum gradients and distances should be between interpolation points relative to the different hydrographic/physiographic implementations.*

The given situation should depend on a number of factors, such as physical heterogeneity, directional ranges (defined by water level variograms), data configuration, and tolerance of regional estimation errors. Direct spatial interpolation of water elevations in a highly heterogeneous aquifer can be a poor solution. In general, certain Kriging techniques may not be suitable for datasets characterized by anomalous abrupt changes such as breaklines and sharp gradients. Inadequate interpolation can also happen in floodplain applications with high hydraulic gradients where obstacles in the water disturb the distribution of the elevations. Distances between gauge and interpolation points should not exceed the directional spatial dependence ranges (in which case anisotropy could be a factor). Concerning lakes and wetlands, the application of geostatistics is in many cases appropriate for water surface estimation purposes, in which case even isotropic models may yield satisfactory results.

7. *What are the advantages and disadvantages of applying geostatistical approaches to predict water elevations as opposed to hydrologic or hydraulic simulation models?*

In principle, estimation is a more accurate predictor (in a statistical sense), whereas simulation often offers a better representation of spatial variation. From empirical evidence, Kriging generates smoother surfaces than the actual phenomenon, whereas simulation provides more surface details. While Kriging offers the "optimal" (in a MMSE sense) spatial estimate, simulation analysis provides several statistically equivalent surfaces (realizations) consistent with the data set. In this way, e.g., one can see the effects of possible diversions from the "optimal" surface.

8. *What other issues or types of limitations should be placed on the use of these geostatistically based water elevation tools with respect to their application in (to be provided in the Application section of the draft document):*

a. Water resources planning and evaluation?
b. Operational decision support?
c. Permit support?
d. Emergency management?
e. Communications with intergovernmental and extra-governmental (e.g., print and video-based media) organizations?

Geostatistical analysis and estimation of water elevation and relevant basin variables are obviously very helpful and often necessary in the cases *a*, *b* and *d* above. This is a direct outcome of the fact that hydrologic knowledge is closely linked to all aspects of water management at the District. Indeed, the ecological and physical characteristics of South Florida have been shaped by years of hydrologic variation.

Spatial correlation assessments and estimation surfaces are often the key inputs in a variety of water management situations, e.g., water quality, water supply, coastal discharges impacts of a La Niña event, flood preparations, and water restrictions of agricultural allocations. This essential fact is stressed out in several parts of SFER. For examples, in lines 13-14: “as a resource management tool to illustrate the present ‘state of the system’.”

Several issues could be discussed concerning the use of geostatistical tools in the context of decision-making, effective management and resource planning. These have to do, for example, with the relationship between input-output uncertainties, choice of sensitivity tests. The OK uncertainty measures assigned to the estimated water level surfaces may enter multi-objective decision-making and planning (see Chapter 2 of SFER) in more than one ways.

Deriving different possible surfaces in advance can help decision-makers gain valuable insight about the effects of different water level scenarios in future planning. This can be a crucial matter in the case *d* above. For example, the predicted water surfaces play a vital role in adequate resources management and the preparation for situations in which too much or too little water creates flooding, water shortage, and ecological impacts.

Available software can turn the maps of estimated water levels and relevant site variables into powerful visualizations that are useful in the transfer of scientific knowledge to the public and private sectors, the effective communication with local governments etc. (see, cases *c* and *e* above).

Posted: 09/19/11 at 07:22 AM by J. Stedinger

1. Is the use of geostatistical methods appropriate for the prediction of water elevation surfaces in each of the major hydrographic/physiographic features presented?

a. Lakes: Lake Okeechobee (very low relief: ~0.01 inches per mile)
b. Wetlands: Everglades Landscape (low relief: ~0.1 inch per mile)
c. Floodplain: Kissimmee River (low relief: ~0.5 inch per mile)
d. Aquifer: Biscayne/Surficial (variable relief)

Yes, though for Kissimmee the use of a trend term with ordinary kriging to become universal kriging would be particularly attractive. When there is little or no anticipated change of water elevation values over space, the ordinary kriging is very appropriate. However in some locations where water is flowing, we do expect a downward gradient in the direction of flow. This may or may not follow the terrain.

2. Does the exact kriging (no nugget value) method provide benefits over other interpretation methods (e.g., Multiquadric Spline, Inverse Distance Weighting) with regard to:

a. Precision (e.g., validation)?
b. Uncertainty (e.g., standard error surfaces)?

Inverse distance weighting does not have the statistical justification of the other methods. It failures to reflect the density of points across space; this can be very important when the density of stations varies spatially. Given SFWMD is going to do a good job, inverse distance would be a disappointing choice. Inverse weighting also lacks the statistical interpretation that allows kriging to generate standard errors of estimation.

Figure 4 top clearly shows that EDEN allows too much flexibility in the fit and does not produce an essentially flat water surface. Perhaps this problem could be overcome with a different kernel. Moreover, RBF is not a statistical method, but an interpolation method. Thus it does not seem to fit the needed requirements of the analysis which should address the distribution of water elevations over space and the possible errors in the interpretation. Moreover, RBF methods are known for their advantages when the x-space is multidimensional. [http://www.scholarpedia.org/article/Radial_basis_function] Here we have just a 2 dimensional flat plane, where isotropic behavior is a reasonable expectation. That RBF methods seem like overkill with the possible dangers associated with over-fitting.

A nice advantage of kriging is that the approach is well established. The method provides a relatively simple statistical analysis of spatial data, such as we have here. It naturally provides the standard error of the estimates, which can be combined with the standard error of estimated land surface elevations to give the standard errors of water depths.

3. Should anisotropy (property of being directionally dependent) be incorporated into limited range, water elevation surface interpolations? If so, please explain how this would be best accomplished.

Isotropy seems like a reasonable assumption. It is an assumption that can be checked with the data by computing the semivariance in the north-south and east-west directions. If an significant different is detected, then there should be no difficulty using ellipses to model the semivariance as a function of distance in the two directions.

4. Should surface water elevation models be interpolated across hydrologically distinct basins or interpolated independently (e.g., Everglades implementation basins)? Please explain why.

If two basins are hydrologically distinct, which I take to mean disconnected by a levee or high ground, then it seems best to interpolate water surface elevations independently in the two basins because they are disconnected. Why should water in two separate glasses (or basins) rise to the same elevation? It is true that seepage across the barrier might limit the difference, but it is unlikely to make the difference zero, and the seepage rate might be small compared to other flows and forces. (For example, wind could build up the elevation on one side of a barrier while decreasing the elevation on the other side.)

5. Are there any other geostatistical approaches that could more appropriately capture the physical or gravitational based properties of water surfaces (e.g., kriging with external drift (KED) or block kriging)? Please explain why these approaches would be more appropriate for capturing these properties.

Block kriging is not the appropriate method because we are trying to generate point values, not areal averages. KED would be a reasonable method to allow incorporation of a trend. I do not understand the relative advantages of KED versus universal kriging. Both incorporate standard kriging, but attempt to handle the drift term differently; implementations of the two methods may also differ. The basic idea is to use Kriging to capture the spatial properties of the data so that the interpolation is efficient (unlike inverse square weighting, and I think RBF as well). One can add a drift or trend term to reflect the likely decrease in surface water elevations as one moves toward the ocean or a bay and away from the source of flowing water.

6. Are there (a) maximum water elevation gradients or (b) maximum distances between interpolation points that would preclude the use of geostatistical approaches to predicting water elevation surfaces? Please explain what these maximum gradients and distances should be between interpolation points relative to the different hydrographic / physiographic implementations.

One does what they have to. Indeed if the distance were long and the flow process in between sufficiently variable, then one might want to go to a flow routing method, rather than spatial interpolation. A stream channel is a good example wherein over miles the elevation of the channel bottom is the most important indicate of stream depth. However, the Everglades is much more like a big relatively flat pond or lake than a rapidly flowing stream, and the flow of the water is not drive by variation in water depths caused by local variations in the surface of the ground. That is the issue. Is the elevation of the water driven by variations in the elevation of the bottom, or are the flows very modest so that the water surface elevation at any point mostly an average of the water surface elevations in at neighboring points. I think for South Florida the issue is where there are barriers to flow that make interpolation unwise across such boundaries because the water bodies were not connected.

7. What are the advantages and disadvantages of applying geostatistical approaches to predict water elevations as opposed to hydrologic or hydraulic simulation models?

If you have enough data, then you have measured the quantity you want. You do not need a hydraulic model, which might provide the wrong water levels because the volume and location of rainfall has been incorrectly measured. If one updated a hydrologic/hydraulic simulation model so that it matched the observed water elevations where one had them, the that model would provide a very good means for interpolating water elevations between water elevation gauges. But that could be a lot of work, and is not necessarily easily done. How does one adjust all the inputs (rain at different points at different times) so that a hydrologic/hydraulic model appropriately matches so many water elevation measurements?

8. What other issues or types of limitations should be placed on the use of these geostatistically based water elevation tools with respect to their application in (to be provided in the Application section of the draft document):

a. Water resources planning and evaluation?
b. Operational decision support?
c. Permit support?
d. Emergency management?
e. Communications with intergovernmental and extra-governmental (e.g., print and video-based media) organizations?

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?

Other comments

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.

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.

However, I think the rejection of the global model for a local model based upon the point at 18,000 feet is unfortunate. As figure 5 shows (see below), the local model is entirely inconsistent with the data for distances over 20,000 feet. A rapidly raising global variance model fits very well except for the point at 18,000 feet, which does not fit the values for shorter or longer distances. 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.

When the data in Figures 6 and 7 have precision [WCA – 3B lacks precision to make a determination; need to group with a larger region to determine semivariance], they again suggest a semivariance that continues to increase with distance. And this is very important for the results.

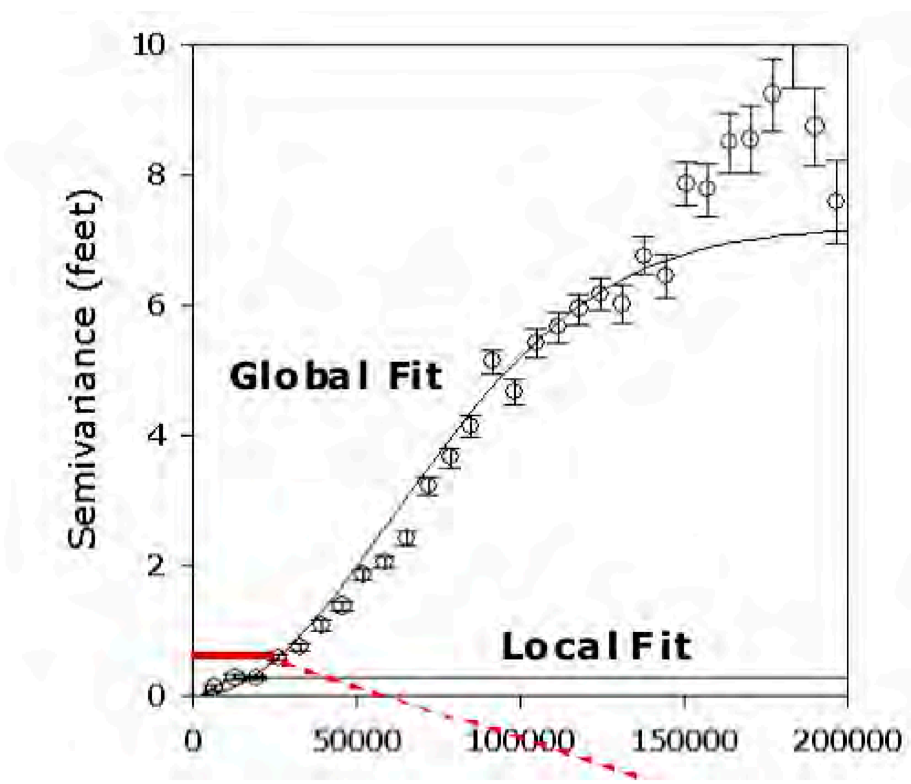

Figure 9 is impressive.

Large errors on the boundary are generally expected. However, if there is a trend which ordinary Kriging ignores, then one would particularly expect larger errors on the boundary because the ordinary kriging model neglects such trend. Thus I would suggest investigating universal kriging with a linear trend to see if it does better at boundaries. Still, the errors you are getting are very small.

Page 8: Figure 11 does indeed illustrate where levees may provide a constraint that would allow for a discontinuity in water elevation levels at the levee location. It seems this might be handled by considering areas on each site of such a levee to be different bodies of water.

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?

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.

On the web I found

http://sofia.usgs.gov/publications/fs/2006-3087/index.html

The EDEN water-level network consists of hourly water-level data from 253 gaging stations (230 existing gaging stations and 23 new installations) and includes freshwater (nontidal) marsh gaging stations, boundary gages on canals, and coastal (tidal) gages operated by the Big Cypress National Preserve (BCNP), Everglades National Park (ENP), South Florida Water Management District (SFWMD), and USGS (fig. 2). All gaging stations have telemetry equipment that provides data on a daily basis. Several of the new gaging stations are co-located where ongoing landscape studies have been conducted for several years. A 6-year (October 1, 1999, to September 30, 2005) dataset of baseline conditions was established for the EDEN water-level network prior to the implementation of CERP, offering scientists a single repository for historic, hourly water-level data.

Posted: 09/13/11 at 12:10 PM by Y. Rubin

In response to Godin's Comment #1: There is no indication that the variable defined as [interpolated ground surface elevation – interpolated water depth] is stationary. Stationarity is a key requirement and it is important to show that it is a reasonable assumption to employ.

In response to Godin's Comment #2: Stationarity cannot be proved. Rather, it could be assessed. A simple-minded but effective approach to get some confidence in this assumption would be to plot some contour maps of the hydraulic head and see if there's trend (see figure below. The figure below (from Changnon, 1987) demonstrates stationary and non-stationary processes. Although it is not based on groundwater levels, it provides a clear idea on what is a stationary domain, in terms of the mean and variance (which is the top view) and non-stationary domains (the rest). In geostatistical applications one should focus on stationary domains (such as described on the top view). Alternatively, one should either model or filter out the nonstationary components of the system under investigation.

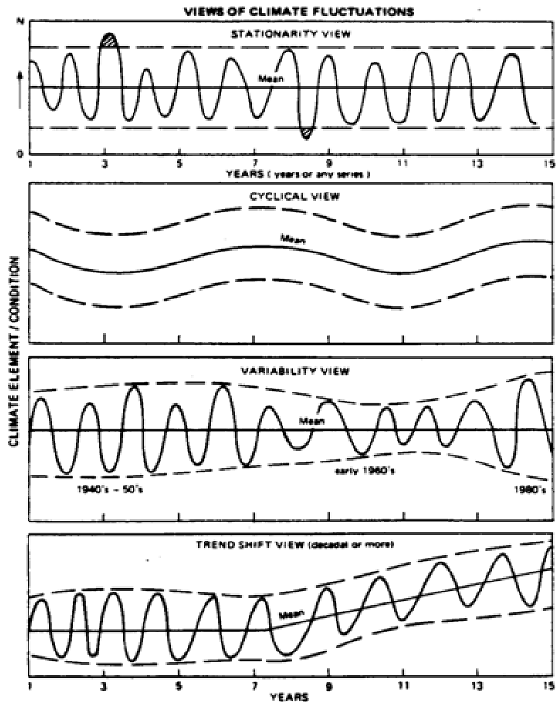

Figure 1. A schematic illustrating varying views of the temporal behavior of climate conditions.

A second option would be to break the domain into sub-domains and compare the moments (e.g., mean, variance). In a stationary domain, they should be quite close. Another approach would be to compare the results of cross-validation performed using ordinary kriging (where trend is assumed to be non-existent) vs. universal kriging (which accounts for the presence of a trend). Cross validation is most valuable when used to compare options.

I cannot recommend an off-the-shelf package because I have not been using any. But a quick web search suggests several options. They need to be tested before application.

In response to Godin's Comment #3: See my response to comment #2. Correcting for the presence of a drift would be helpful.

In response to Godin's Comments #4 and #5: Detailed documentation with graphics is needed (as requested above) to assess the quality of the cross-validation exercise. Detailed information on cross-validation is provided in Rubin (2003), p. 345-346. Additional discussion is provided in Isaaks and Srivastava (1989, Chapter 15).

In response to Godin's Comment #6: There could be some contention if the measurement error is ignored. I think it would be easier to defend a decision to recognize the measurement error. A decision to ignore measurement error could also be defended if the error is very small.

In response to Godin's Comment #7: Heads are correlated over longer distances along the mean flow direction. The anisotropy could be masked by not removing the trend. Cross validation could be used to compare working with or without anisotropy (as well as other decisions such as ordinary kriging vs. universal kriging).

In response to Godin's Comment #8: See previous comment on cross-validation. Rubin (2003) makes suggestions on cross-validation. I suggest to evaluate the performance and functionality of commercial packages or to develop an application in-house.

In response to Godin's Comment #9: See my previous comments on cross-validation for testing of stationarity and other assumptions related to the geostatistical moments. Using water levels affected by pumping or other types local activities can introduce bias and should be removed. Geostatistical analysis intends to capture patterns, and patterns could be destroyed by local activities (consider for example, what would happen in figure 1 if there is a spike of some sort at some locale. Furthermore, wells could be turned on and off, and how would that be accounted for in the semivariograms? If the developers consider the potential for bias due to local activity to be minor, this needs to be shown (by comparing results obtained with and without removal of the non-stationary effects).

With regard to maximum distance for interpolation. The common wisdom is to avoid estimating variogram models for lags larger than 50% of maximum separation distance between measurements. So this creates an upper bound. There are no universal guidelines, to my knowledge, about the maximum distance for interpolation. But the question could be address by defining an optimal radium for a search neighborhood for kriging. Search neighborhood defines the area where measurements could be used for kriging. The optimal radius of the search neighborhood could be determined by comparing estimates obtained with different radii and by assessing the quality of the estimates at the test points (see Isaaks and Srivatava, 1989, Chapter 14).

In response to Godin's Comment #10: The behavior described in in Godin's #10 is well known. This is always the case for the number of pairs to decrease with lag distance. There is no lower threshold for a number of pairs if a strategy for dealing with uncertainty quantification (UQ) is adopted. With UQ, poor model quality is recognized by a larger estimation variance. There is no UQ in place now, and a poor quality model is treated same as a good quality model, and that could be misleading. UQ should be considered and adopted for implementation.

In response to Godin's Comment #11: Please see my earlier comments on this topic. Without UQ and with changing physical conditions and the other issues mentioned earlier, I would be careful before putting any confidence in the geostatistical model. An important point to remember is that geostatistical interpolators are exact, which means that they would always provide estimates that are equal to the measured values at the measurement locations. But that does not mean much with regard to the quality of the estimates between measurements, which is where the real challenge lies.

In response to Godin's Comment #12, 13: See my earlier discussion.

In response to Godin's Comment #14: Comment unclear.

In response to Godin's Comment #15: A strategy is needed to develop a solid UQ, with the goal of providing estimation error for each estimate. Note that the kriging error cannot be used for UQ. What is needed specifically is a statistical model that could be used to bracket the estimates with confidence intervals. Cross validation is a method to assess the quality of the geostatistical models and compare alternative approaches and assumptions. It could be used as a basis for determining the statistical distribution of the estimation errors.

References

Changnon Jr., S. A., 1987, An assessment of climate change, water resources, and policy research. *Water International* 12: 69-76

Christakos, G., 2000, Modern Spatiotemporal Geostatistics, Oxford University Press.

Dagan, G., 1989, Flow and Transport in Porous Formations, Springer.

Hubbard, S., Chen, J., Peterson, J., and Y. Rubin, 2001, Hydrogeological characterization of the South Oyster Bacterial transport site using geophysical data, Water Resour. Res., 37(10), 2431-2456.

Isaaks, E.H., and R.M. Srivastava, 1989, An Introduction to Applied Geostatsitics, Oxford University Press.

Rubin Y., and G. Dagan, 1988, Water Resour. Res., 24(10), pp. 1689-1697, 1988.

Rubin, Y., and G. Dagan, 1989, Water Resour. Res., 25(4), pp. 707-712.

Rubin, Y. and K. Seong 1994, Water Resour. Res., 30(11), 2901-2912.

Posted: 09/13/11 at 02:06 PM by G. Christakos

Apantisis #1: Actually, the self-reference problem applies quite generally including spatial interpolation, estimation, smoothing, and prediction. To put it in metaphorical terms, the problem may be loosely described as follows:

1. By consulting a group of pizza lovers D (corresponding, say, to our attribute data set) a favorite pizza recipe V (corresponding to the variogram) is constructed that represents the group D's taste structure, so to speak.

2. Then, by using the recipe V a pizza K is produced (corresponding to the attribute kriging surface).

3. Finally, the group D is again used to taste how good the pizza K is. But this is a pizza that was constructed precisely according to the taste V of the group D in the first place! Does anyone think that the D folks will not favor their own kind of pizza?

However, the self-reference problem can be solved in two ways (From their response, it seems that the SFWDAT modelers have successfully applied the second way in the specified case study.):

First way: In real world studies, probably the most adequate solution to the self-reference problem is to construct the water elevation variogram V based on physical models, and use the data set only for kriging purposes. The matter is discussed, in both theoretical and practical contexts in the literature (e.g., Christakos G., 2000, "Modern Spatiotemporal Geostatistics," Oxford Univ. Press; and 2010, "Integrative Problem Solving in a Time of Decadence," Springer). Physical model-based estimation provides a safe framework within which one can assess the performance of geostatistical water modeling.

Second way: Another, rather ad hoc, solution is to first divide the data set into two subsets 1 and 2, and then use subset 1 for variogram modeling and subset 2 for kriging estimation purposes. This is a useful approach as long as one keeps in mind potential methodological issues. For example, it is possible that a different choice of a subset 1 (varying data distributions, relative distances, uncertainty links etc.) may reveal a different spatial dependence structure, which could affect the kind of kriging estimates one obtains. Similarly, the choice of subset 2 may have an effect on the kriging results (spatial data orientation, distances with respect to the estimation point, etc.).

Apantisis #2: Ok. Yet, keep in mind that accounting for multi-sourced uncertainties is important in spatial and spatiotemporal modeling and attribute mapping (e.g., Savelieva et al., 2005. "BMEbased uncertainty assessment of the Chernobyl fallout," Geoderma). Also, nugget effect is a useful way to account for uncertainties in common kriging-based spatial or spatiotemporal estimation techniques.

Apantisis #3: Ok.

Apantisis #4: This is a good question. Unfortunately, there is not a generally valid method to determine the adequate number of data-pairs required for reliable variogram estimation. Rather, the choice of the minimum number of pairs required to derive reliable estimates is a matter of experience and insight. The SFWDAT modelers may recall that the following two recommendations are rather widely implemented in geostatistical variography (e.g., Olea R.A., 2009, “A six-step practical approach to semivariogram modeling,” J. Stoch Envir. Res & Risk Ass.):

1. The minimum number of pairs used in variogram estimation is about 30, according to an American school, and 50 according to a French school of geostatistics.

2. The maximum lag considered in variogram estimation should be no more than half the extreme distance in the sampling domain along the direction of interest.

In light of these two recommendations, the following tips are useful in practice:

a. The number of data-pairs used in variogram estimation depends on the spatial dependence structure of the attribute of interest, the data distribution, and the data uncertainty (measurement noise). A smoother attribute dependence may require a smaller number of data at short distances (within areal units), a heterogeneous dependence may require more data at larger distances than for spatial homogeneity; also, the number of pairs may increase with anisotropy etc.

b. If one needs to obtain variogram estimates at large lags, say close to the diameter of the sampling domain, the corresponding data-pairs will include only pairs located at opposite extremes of the domain, ignoring central data points.

c. The “data set-estimation accuracy” association for a water surface can be evaluated in terms of the kriging error map that is a function of the variogram. The measurement noise also affects the kriging error variance, and as a consequence, the gauge number needed in the specified case.

Apantisis #5: Indeed, the benefits can be considerable. The SFWDAT modelers may find it useful to visit the website of modern space-time geostatistics:

http://homepage.ntu.edu.tw/~hlyu/software/STBME/

Some useful software packages can be freely downloaded from this website, among other things. There are rigorous theories as well as many published case studies that deal with the matter of the superiority of space-time surface estimation vs. purely spatial or purely temporal estimation. See, the book “Modern Spatiotemporal Geostatistics” mentioned earlier. Also, papers by Kolovos A. et al., 2004, “Methods for generating non-separable spatiotemporal covariance models…,” Adv. Water Resour.; Christakos G. et al., 2000, “On the physical geometry concept at the basis of space/time geostatistical hydrology,” Adv. Water Resour.; Vyas V.M. and Christakos G., 1996, “Spatiotemporal analysis and mapping…,” J. Atm. Environ.; and elsewhere). And several other works found in the literature of stochastic space-time analysis.

Surely, temporal dependence plays an important role in the study of attributes governed by physical (partial differential) equations involving both spatial and temporal coordinates. Incorporating temporal dependence could improve the performance of the geostatistical approach (e.g., Kolovos A. et al., 2002, “Computational Bayesian max entropy solution of stoch. Advection reaction equ in light of site-specific info,” Water Res. Resour.; also, Yu et al., 2007, "A composite solution method for physical equations and its application in Nea Kessani geothermal field….,” J. Geophysical Res.-Solid Earth).

For a numerical illustration, see in the table below some results from an actual PM10 case study. The composite space-time kriging (STK) PM10 surface estimation error was found to be always smaller than the spatial ordinary kriging (OK) PM10 surface estimation error and the PM10 time series (TS) estimation error. At the monitoring station no. 129, the STK error was only 28% of the TS error; and at the station no. 167, the STK error was only 58% of the OK error. The same was the case with all other monitoring stations.

Monitoring Station No.	Improvement in PM10 surface estimation using composite space-time (ST) Kriging vs.	
	Spatial Ordinary Kriging (OK) e_{STK}/e_{OK}	Time series (TS) analysis e_{STK}/e_{TS}
9	0.62	0.43
111	0.59	0.58
129	0.64	0.28
143	0.76	0.44
167	0.58	0.55

Apantisis #6: Ok. In any case, since the present approach is commonly used, it is reasonable that one assesses the effects of temporal weighting applied "before" vs. "after" interpolation. The effects are considerable (e.g., Yu H-L *et al*., 2009, "BME estimation of residential exposure …. At multiple time scales," J. Envir. Health Persp.).

Apantisis #7: In these cases, indeed, one can employ standard statistical tests, and, also, well thought sensitivity analyses. The former seek to check whether the modeling assumptions apply formally, whereas the later rather focus on the substantive implication of these assumptions. It is possible that a modeling assumption is not exactly valid in a formal sense, yet the in situ implication of this diversion is not serious (this happens in some cases; e.g., Fourier transformations sometimes do not exist in a strict mathematical sense, yet useful approximations can be derived).

In the statistics/geostatistics literature one can find a set of standard procedures to test the normality of a given data distribution. In addition to the standard statistical procedures, performing a carefully designed sensitivity analysis is often appropriate. If normality is not proven, there are different kinds of data transformation (normal score transformation, Gaussian anamosphosis techniques etc.) that a modeler can use.

An easy way to check whether the constant mean assumption is appropriate for OK estimation purposes or not is to examine whether the variogram is spatially homogeneous or not. If not, one should be very careful when applies OK techniques in spatial modeling and prediction. In the UK case, the assumptions of constant mean, homogeneity etc. are not appropriate for the original data set. If the above analysis and testing are not reliable or even possible on the basis of the available data set, it may be safer to use surface estimation techniques that are not restricted by Gaussian, low-order spatial statistics etc. restrictions (e.g., BME techniques instead of OK and UK techniques).

Apantisis #8: Ok.

Apantisis #9: I see your point. What I meant is that it is always useful and sometimes even necessary to incorporate secondary information for spatial estimation purposes, especially when a limited number of data is available. There are many research papers discussing the relationship between landuse and water table that can be obtained, e.g., from Google scholar or Web of Knowledge (see, Zhang Y.K. and Schilling K.E., 2006, "Effects of land cover on water table, soil moisture, evapotranspiration, and groundwater recharge....," J. of Hydrology; and other research works). These relationships can be valuable in water surface modeling.

Apantisis #10: Nested models can be used in spatial estimation in the same way as other kind of models. The real issue is how to properly fit nested models to the raw data. This fit is based on insight and expertise. One can chose from a list of nested models available in the geostatistics literature, which are often linear combinations of elementary models. Concerning the in situ implementation of nested model in the context of UK or OK estimation techniques, a modeler can consult standard geostatistics books, like Olea R.A., 1999. “Geostatistics for engineers and earth scientists,” Kluwer.

Apantisis #11: The kriging techniques are affected by the attribute’s natural anisotropy ranges, rather than the other way around (a typical “technique-nature” association). In a geostatistical study one mainly refers to data anisotropy. The ranges of variograms along different directions represent the degree of directional dependence (spatial correlation) between pairs of points. These ranges enter the kriging system and affect the number of data used at each estimation point. Commonly, a variogram exhibits a correlation range in the case of spatial homogeneity. The variogram has no range in the case of spatial non-homogeneity. Universal or intrinsic kriging has been designed for non-homogeneous attribute variability, in which case the conventional variogram is replaced by a generalized variogram or generalized covariance. The analysis is rigorous but not trivial.

In practice, there are several ways to study anisotropy depending, again, on the particular in situ situation. Geostatisticians do not lack imagination when it comes to effective practical solutions. For example, one could plot the variogram rose to investigate the attribute’s anisotropy features. Useful automatic schemes of anisotropy testing can be found in the relevant literature, see, e.g., Chorti A. and Hristopulos D.T., 2008, “Non-parametric identification of anisotropic (elliptic) correlations in spatially distributed data sets,” IEEE Trans on Signal Processing.

Apantisis #12: Ok.

Posted: 09/19/11 at 07:22 AM by J. Stedinger

The comments by the other reviewers, and the responses by SFWMD, were very informative. I have contributed my responses below. A general discussion of statistical issues is included in the section that nominally responds to concerns raised by Reviewer 1 because they fit best there.

My basic recommendation is the same:

Use of a trend term with ordinary kriging to become universal kriging would be particularly attractive. When there is little or no anticipated change of water elevation values over space, the ordinary kriging is most appropriate. However in some locations where water is flowing, we do expect a downward gradient in the direction of flow. This may or may not follow the terrain.

Inverse distance weighting does not have the statistical justification of the other methods. It failures to reflect the density of points across space; this can be very important when the density of stations varies spatially. Given SFWMD is going to do a good job, inverse distance would be a disappointing choice. Inverse weighting also lacks the statistical interpretation that allows kriging to generate standard errors of estimation.

Appendix 6-1 in Figure 4 top clearly shows that USGS EDEN (http://edis.ifas.ufl.edu/uw278) allows too much flexibility in the fit and does not produce an essentially flat water surface. Perhaps this problem could be overcome with a different kernel. Moreover, the Radical Basis Functions employed by EDEN are not a statistical method; it is an interpolation method. Thus EDEN does not seem to fit the needed requirements of the analysis which should address the stochastic variation of water surface elevations over space (due to variations in rain depths as a result of the vagaries of different storm paths, but which should otherwise be relatively flat), and the likely errors in the interopolated surface. Moreover, RBF methods are known for their advantages when the x-space is multidimensional. Here we have just a 2 dimensional flat plane, where isotropic behavior is a reasonable expectation. The RBF methods seem like overkill and is subject to the dangers associated with over-fitting.

A nice advantage of kriging is that the approach is well established. The method provides a relatively simple statistical analysis of spatial data, such as we have here. It naturally provides the standard error of the estimates, which can be combined with the standard error of estimated land surface elevations to give the standard errors of water depths.

Kriging is a widely-used statistical methodology, with the associated statistical methods described in textbooks and manuals, and supported by software. Appendix 1-6 provides nice examples. Anisotropy (property of being directionally dependent) can be explored by examining variograms computed in different directions.

REVIEWER 1 (Rubin)

I fear that Dr. Rubin does not entirely understand the hydrologic situation in the Everglades (See Comment 1 and 3 as numbered by SFWMD). The basic assumption is that the entire area is mostly covered with water, and that the water surface elevations, away from critical barriers, are relatively flat. We are not expecting a water table elevation that is below the land surface. Here the water table elevation is the water surface elevation, which could be considered the hydrologic head, though that term head seems misleading in this context. SFWMD Response 1 helps to clarify the situation.

While we expect the surface water elevation to be relatively flat spatially, the water depth (water surface elevation minus land surface elevation), could be quite irregular (particularly on a scale of 1-10 meters) because of variation in the land surface elevation.

Comment 8 – Cross-validation - Confirmation of Assumptions

Cross-validation allows computation of the precision of this interpolation methodology by determining how well (in a leave-one-out comparison) a method can predict each observation given the other observations. That does not determine what assumptions are correct. Incorrect assumptions might still result in good estimators. Direct statistical tests (such as those requested in SFWMD Response 2-3 and 8-11) are better able to check assumptions.

Assumptions in Statistics - Estimators

A critical assumption in statistics is that if one adopts a reasonable representation of the statistical variability in the data (this entails what can be called assumptions), and employs a good estimator for such a situation, then one will have a reasonably good estimator for the problem at hand.

Stationarity

There is much discussion of stationarity in the comment & response. I am not sure everyone is understanding the word in the same way. A random walk is a classic example of a non-stationary process (imagine the progression of a point on the real line where at each minute one steps to the left or the right one foot based upon the outcome of a flip of a coin) in that as time progress the variance of the location of the walker continues to grow without bound. However, the rules of the process do not change with time, and in that sense the process is stationary.

Use of a variogram for surface water elevations that increases linearly with distance produces a model that is, like a random walk, non-stationary. However, the process is also stationary in the sense that the distribution of the observations follow a probability law that is the same across time and across space: or we might stay that the variogram is stationary (whereas there is no mean that applies to all locations).

My understanding based upon the data is that for the Everglades the SFWMD has a variogram that increases linearly, suggesting that our best model of water surface elevations is described by a non-stationary model. However, we can still use the data with a Kriging estimator to compute an estimate of the surface water elevation AT ANY LOCATION, as well as the standard error of our estimate for that location.

Comment 10 – SFWMD 10 – One is fitting a variogram to the entire data set, so the estimated value for any distance is not based just on the site-pairs at that distance. The error structure is more complicated. One hopes that the function is computed reasonably well. Binning, Bootstrapping and other techniques can be used to assess precision.

One should also recognize that the variogram is used to weight the available observations. If the variogram is subject to modest estimation error, that error should have only a modest impact on interpolated values. This can be checked with sensitivity analysis. Furthermore, points that are very distance from the point of interest correspond to relatively larger variogram values, and thus should get very little weight in the construction of an estimate of the water elevation at the location of interest. Limiting the distance of points employed in the interpolation is a more straightforward way to insure that distance points have little or no influence on interpolated values. The also add a reasonable limit on the size of the estimation problem, which has technical advantages.

Comment 15 – The kriging estimator is a weighted sum of a large number of observations. And we certainly hope that the standard error of the estimator is not very large. Thus the Central Limit Theory suggests that the estimator should have something approaching a normal distribution about the true value. That should allow computation of reasonably accurate confidence intervals.

SFWMD Response 3 – I do not understand how a decision tree is relevant here. I agree that one should correct for drift and/or relief features by selecting appropriate kriging assumptions.

SFWMD Response 6 articulates an important constraint that was not apparent to me before. If the interpolation method is constrained to actually interpolate reported elevations so they agree with all the measured values, then indeed a zero nugget is required. Moreover, a zero nugget (no measurement error) was consistent with the data.

REVIEWER 2 (Christakos)

Reviewer comments were supportive. I entirely agree with his comment 2. SFWMD has provided reasonable clarification where appropriate. Responses 4 and 7 request suggestions for additional statistical procedures, or criteria. OK means Ordinary Kriging.

Comment 4 and response agree that at least 30 pairs, if not 50 pairs, are desirable to obtain a reasonable estimate of a variogram. Implicit in that discussion is that the pairs provide a reasonable spread across the distances of interest so as to define the variogram as a function of the distance between observation stations, and locations of interest for interpolation.

SFWMD Response 1

I was surprised that a new variogram was computed for each day. This increases the concern of self-reliance raised by Reviewer 2. I would have anticipated that data from several similar days could be combined when computing a variogram. This would increase the number of pairs (each physical pair could contribute cross-correlation values from different days) and make the elevation estimator more robust.

Comment 7.1 – The issue is raised if in the interpolation process, SFWMD might make use of cross-correlations over time to improve the precision of the estimates. On one hand, because there is no measurement error (by assumption and none turned up when fitting a variogram), for points at and near a observation, use of observations from other periods will provide no benefit. On the other hand, for points some distance from measurement points, observations in the same region earlier or later in time might provide useful information about water levels in the region and how water is moving. Unfortunately to correctly interpret that information would require a substantially more complex model, whether it be a larger statistical analysis (using a cross-time variogram) or a physical model. This is an opportunity that could be explored. My personal concern is that the additional information that data from earlier and later periods will provide is relatively small, while a very substantial effort would be required to find and to take advantage of the useful links.

REVIEWER 3 (Stedinger – me)

Comment 1 – Response – 1: We have no disagreement.

Comment 2 – Response – 2: We have no disagreement. Nice graph.

Comment 3 – Response – 3: We have no disagreement.

SFWMD articulates an important constraint that was not apparent to me before. If the interpolation method is constrained to actually interpolate elevations so they agree with all the measured values, then indeed a zero nugget is required. Moreover, a zero nugget (no measurement error) was consistent with the data.

Comment 4 – Response – 4: We have no disagreement. If the local model is only used with points within its radius of applicability, everything is fine. That was not clear.

Comment 5 – Response – 5: We have no disagreement.

Comment 6 – Response – 6: We have no disagreement.

It sounds like SFWMD is collecting the information it needs.

While my original comment is not incorrect, it fails to represent another critical issue. SFWMD wishes to compare water depth over time at a single location (either different seasons or different years) to reveal trends, impacts of changes in operations, and seasonal patterns. Then errors in in the land surface cancel out, whereas random errors from month to month in the interpolated water surface elevation are critical. Thus to construct reliable descriptions of variation in water depth over time it is critical to have good estimates of water elevation (regardless of the precision of ground surface elevation estimates).

REVIEWER 4 – Burkholder

Reviewer asked questions but did not raise concerns with the SFWMD analysis.

Comment 2 – Reply 2 - I had thought that monitored canal observations would have been observations. Perhaps the hydraulics in and around canals requires special consideration.

COMMENTS ON DRAFT VOLUME I, APPENDIX 10-2

Posted: 11/22/11 at 02:58 – 0.3:01 PM by D. Thomas

General comments: Overall, a great update to the Plan. Many valuable projects put forth.

PDF is poor quality with abnormal spacing that makes the document difficult to review. Recommend reformatting the document and allowing additional time for review.

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?

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.

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.

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.

Posted: on 12/5/11, from e-mail dated 11/30/11 at 03:42 PM by J. Ott, SFWRFC

From: Judy Ott [mailto:jott@swfrpc.org]
Sent: Wednesday, November 30, 2011 3:42 PM
To: Balci, Pinar
Subject: RE: Combined NE Interagency and FDEP Tidal Caloosahatchee Basin Management Action Plan Meetings, December 14th

Hi Pinar,

I'm sorry I missed your Nov 22 deadline – wasn't even on my radar somehow – my bad.

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. It is my bad that I missed your deadline – I'll have to bring it up at the meeting on Dec. 14. I'll pass your mess along to our TAC, ok? Thanks, Judy

Posted: on 12/5/11, from e-mail dated 11/22/11 by Lee County

Intro

Please accept this as Lee County‟s formal comments on the draft 2012 Caloosahatchee River Watershed Protection Plan Update (Plan Update). Lee County wishes to express its continued and full support for the participating agencies‟ efforts to carry out the intent of the Northern Everglades and Estuaries Protection Program (NEEPA). The recent drought and resulting adverse water quality conditions highlight the need for continued vigilance in protecting the Caloosahatchee River Estuary. The Caloosahatchee River is the lifeblood of Lee County that provides economic benefit and quality of life to its residents. Protecting this precious natural resource is critical to the wellbeing of Lee County and its residents.

Coordination on TMDL/BMAP – Development and Need for Greater SFWMD Involvement

The FDEP's 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.

Construction Project

Despite the fiscal constraints since the initial plan was adopted in 2009, significant progress has been made toward the completion of the Phase I Construction Projects. Several local stormwater projects have been completed with cost-sharing from the SFWMD. The design is complete for the centerpiece water storage project (C-43 West Reservoir) and it is now awaiting Congressional authorization and funding.

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.

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). Lastly, there has been no noted progress with the Comprehensive Planning and Growth Management Project.

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.

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.

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).

Specific Comments by Line and Project

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.

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 holder‟s stormwater management jurisdiction, such as ERP residential permitted areas that are presumed compliant under current State stormwater rules.

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.

Line 127. (FDEP and SFWMD): There appears to be a lack of continuity in defining agencies from section to section.

263/268: Planning Boundary: should also state that the watershed shrinks in the dry season to west of Ortona Lock

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...")

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.

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.

CRE 45. It should be noted that while Phase I and Phase II were completed, the project land stewardship management plan is still pending.

CRE 142. FY 2011 is over. The conceptual design and is either complete or pending in FY 2012.

CRE 146/148. Powell Creek spelled incorrectly – "Powel."

Closing

Lee County is proud of the commitments and accomplishments made by all parties involved in the development and implementation of the 2009 Caloosahatchee River Watershed Protection Plan. The fiscal constraints on government agencies, has made implementation and updating this Plan an even more difficult task. However, such constraints should not prevent the coordinating agencies from taking the necessary regulatory actions (strong BMPs, Basin Rule, Regulatory Nutrient Source Control Program, etc.) outlined in the 2009 Plan. Furthermore, these regulatory actions must be carried over into the Plan Update to ensure implementation within a reasonable timeframe. Following through on these regulatory goals and adding new goals (Water Reservation) are critical to carrying out the intent of NEEPA. Also, critical to the intent of NEEPA, is continued vigilance by the participating agencies to ensure timely construction project completion and development of a robust collection of data within the entire watershed.