Appendix 1A-4: Comments on the Draft *2008 South Florida Environmental Report –Volume I* from Outside Persons and Organizations

In September 2007, these comments were provided to the public on the District's WebBoard (http://www.sfwmd.gov/sfer). With the exception of reformatting some information for better readability, this appendix was not edited or spellchecked by the SFER production staff and appears as posted on the District's WebBoard.

Comments from DOI Everglades Program Team
September 17, 2007

General comments:

1. We are very supportive of efforts to continually review, improve, and optimize designs of water quality monitoring programs in South Florida. Such optimization can improve the quality and the cost-effectiveness of data collected in support of natural resource management and restoration. In addition, we have been very pleased with the process employed by the SFWMD to review these programs, as early and significant stakeholder involvement can help guarantee success and sustainability of these programs.

2. The introduction and concluding sections of the chapter focus on reengineering efforts as a way to better address critical water quality issues. However, the strawman proposal seems to focus on a perceived need to reduce stations and sampling frequency in order to reduce costs. Such cost reduction is not a bad goal – it is just that the document should have that as the major goal elucidated from the beginning. If other goals include providing better data in support of resource management, additional analyses are needed to address those goals. Such additional information needs are suggested throughout this review. If the goals truly include an analysis of the use of data, the possibility of increased numbers of stations or increased sampling frequencies should be considered as real possibilities. As written, the draft only considers reductions in numbers and frequencies.

3. The Introduction provides information on the total cost of water quality monitoring, the total number of stations, and the number of sampling events. However, no information is provided to put the monitoring program in context with the scale of ecosystem management and restoration in South Florida. The national examples presented could be used to examine the percentage of total effort that water quality monitoring programs comprise. It is likely that the South Florida water quality monitoring program is among the largest, if not the largest, of its kind in the country. At the same time, the scale and intensity of our restoration activities also may be the largest, and the number of mandates probably is the largest. The relative amount of resources dedicated to monitoring (and other restoration science support programs) would provide users of this information some comparative information.

4. To truly incorporate the potential value of individual or groups of stations in any reengineering effort, an assessment of the actual uses of data should be included. In the two public workshops preceding this draft report, comments were received with specific examples of how some data are being used. It is likely that there are additional uses of these data, even by the SFWMD, that have not been considered. One useful exercise might be to formally survey potential users (scientists, modelers, engineers, managers, etc.) of these data to ascertain whether or not there are other examples of data use. For example, at an interagency working group meeting after this draft chapter was prepared, four different uses of data for the S-10s were brought to the table that had not previously been considered by the internal SFWMD group – Everglades Landscape Model

[SFWMD]; the Refuge's water quality model [DOI]; impaired waters assessments [FDEP]; previous CERP water quality analyses for planning purposes (e.g., Yellow Book; ICU) use structure data [SFWMD, USACE].

5. Outside sources of data might be useful in the reengineering analysis. For example, EPA's EMAP/REMAP program included WCA-2A. Additionally, several years ago, the RECOVER Water Quality Team (an interagency technical body) underwent a similar exercise to develop a water quality monitoring strategy for the greater Everglades.

6. Given that cost reduction is a major goal, an additional analysis should address the comparison of costs of out-sourced (contract) work versus costs of in-house (staff) work. It is likely that substantial cost savings can be achieved by maximizing in-house efforts, including both sampling efforts and laboratory analyses.

7. In order to better assess the strawman proposal, have there been any changes to the existing WCA-2A monitoring network that have resulted, or are likely to result, from near-term budget decisions? If the "baseline" conditions are likely to change, those changes should be noted and considered.

8. What is the potential impact of this reengineering effort on other, more specialized water quality monitoring programs that include WCA-2A? (e.g., quarterly pesticide survey, mercury monitoring)

9. It would be helpful to know what disciplines and backgrounds are represented in the internal workgroup (e.g., biologists, engineers, modelers, managers, etc.). This information would assist in the assessment of the perspectives presented by the internal workgroup.

10. The Introduction covers a spectrum of arguments implying changing to a regulatory-only based approach (e.g., lines 150-156), to highlighting the value of water quality information in general (e.g., line 229), to focusing only on "essential information" (line 95). In addition to describing the need to re-look at monitoring, there is a need to describe whether and/or how the questions have changed. For example, previous monitoring at the S-10E, and other nearby sites, have documented improvements in water quality early in the life of STA-1W when discharges were at moderately low concentrations – that information would have been lost under proposed changes, raising the question of what is "essential information".

11. While there are benefits to examining a smaller piece of the overall monitoring network pie, there are aspects of neighboring influences that need to be incorporated. For example, proposed changes to the S-11s and S-143 structure need to take into account the monitoring network of WCA-3A. The same issue exists at the S-10s, which are actually monitored in the A.R.M. Loxahatchee National Wildlife Refuge. Currently planned diversions of water away from the Refuge in the future will result in many fewer times that the S-10s flow.

Specific comments:

p. 2, line 27: What data/analyses are used to make the conclusion that the existing network is not optimal? The simple observation that there are many stations in WCA-2A and/or that the program costs a lot is not sufficient support for this statement. The statement may be true, but statistical analyses (beyond what is included) and consideration of data needs and uses should be considered, as well.

p. 7, lines 174-178: The very fact that the Class III monitoring system was the result of a thorough, comprehensive analysis, including a public component, points to the difficulty of making subsequent changes as a result of this reengineering effort. Will some mandates be given higher priority than other mandates? Will some data uses be given higher priority than other data uses? If so, how?

p. 8, lines 202-213: The way this paragraph reads implies that the chapter before the SFER review panel was the status of efforts after incorporation of input from the interagency working group. In fact, as a function of timing to complete this draft chapter, the version of the straw-dog case study for WCA-2A was a product of the internal SFWMD group.

p. 11, lines 295-296: How many times have samples actually been collected more than 55 days apart? This long time interval may be a possibility, but is more important to consider how often it actually has occurred. One could do a cumulative frequency distribution analysis at select stations to illustrate the actual sampling frequency distribution.

p. 11, lines 303-305: Again, it may not be surprising that the present system has been sustainable based on the actual sampling frequency distribution.

p. 12: Need to define “edocs” for the reader.

p. 13, line 342: Is the 10-20% number based on estimates of field crews, or analysis of actual data?

p. 13, entire section: It certainly is understandable that the cost of autosamplers is high, and that they are subject to a number of problems. However, flow-triggered autosamplers are the only way to capture significant but infrequent flow events. It is well known that high percentages of the total pollutant load to a water body can be conveyed by relatively infrequent flow events. It is important to discuss the advantages of autosamplers, as well as the disadvantages.

p. 17, lines 393-397: What would be valuable to include is an example of how this change in monitoring strategy would influence collection at individual stations, and not just a global tally. An example of the 2004 monitoring at S-10D was presented at one interagency group, where a large number of samples would not have been collected under the proposed strategy. Those samples that would have been collected would have encompassed a short period of the calendar year, with no data gathered for about three-quarters of the year. That type of presentation would be valuable in helping translate the proposed changes from the conceptual perspective presented in this chapter to a more empirical perspective.

p. 17, lines 393-397: As stated above, monitoring at the S-10s occur in the Refuge, and there is thus a Consent Decree nexus that is not brought to the discussion in this chapter.

p. 19, line 433: For reasons already stated, we disagree that samples collected during no-flow conditions provide little useful information.

p. 19, lines 441-446: Intriguing information on differences in statistics is presented, but not analyzed in further detail. What, if any, are the significances of these differences?

p. 23, lines 562-565: Don't researchers have to adhere to QA/QC protocols?

p. 23, lines 568-569: Do investigator priorities not align with agency priorities?

p. 23, lines 593-596: On face value, the existence of separate monitoring projects does not have to preclude good integration and coordination. In other words, the mere number of monitoring projects is not a good metric of the degree of network optimization.

p. 24, entire page: An additional approach to zonation would be a simple cluster analysis using readily available software programs. Cluster analysis could be used in conjunction with the more subjective approach employed. The entire discussion here is somewhat general and subjective. As written, the analytical thinking used to arrive at the zonation is a "black box" to readers. There is a suggestion (p. 33, line 815) that cluster analysis will be employed.

p. 24, lines 605-606: Tell the reader in the main text that decisions on stations were made by analyzing three surface water parameters: conductivity, sulfate, and total phosphorus.

p. 24, lines 608-610: Provide the information to allow readers to make their own assessment whether or not conductivity differences are ecological significant. It is likely there could be broad agreement on points like this one, but without the data, such agreement cannot be assessed.

p. 25, Figure 12: The zone names are not as informative as they could be. For example, are the northern and western zones impacted or unimpacted?

p. 27, lines 647-648: Again, let the readers make their own assessment of ecological significance by providing the data, or by presenting more details. The conclusion might be the same by the readers, but as written, that assessment is not possible.

p. 28, lines 658-666: In the second workshop, there was a discussion about relocating stations to better reflect a transect on the observed flow path in western WCA-2A.

p. 30, lines 709-711: We do not agree that a reengineering goal has to include a standard water quality parameter set, although we believe some current water quality monitoring programs are not monitoring enough water quality parameters (e.g., four-part test network). If that is the case, how will less routine, but ecologically critical, parameters such as metals and pesticides be assessed? If they will be incorporated into a separate program, it should be stated so here.

p. 30, lines 700-735: There appears to be a disconnect between a limited list of parameters used for determining whether stations should be dropped or not, but the appearance of an adequate analysis of all water quality parameters to propose a preliminary standard set.

p. 31, first paragraph: Ammonia probably should not be dropped, as it has been shown to be a Class III parameter of concern in WCA-2A and other water bodies.

p. 31, lines 725-735: This paragraph indicates that conductivity measurements may be dropped because of questions about their data utility; however, conductivity was chosen as one of only three surface water chemistry parameters used for the analyses presented in the Appendix.

p. 31, lines 733-735: An intriguing suggestion that merits further investigation.

p. 31, second bullet: It may not be practical or necessary to attempt to resolve the 10 cm issue. The 10-cm sampling limitation comes from Dr. Bill Walker's analysis of data variability in the Refuge. His conclusion was that data variability in samples collected from less than 10 cm of clear water depth was unacceptably high when samples were collected using standard techniques (e.g., hand-dipping a sample bottle). However, it is completely possible to take a good sample where clear water depths are less than 10 cm using careful techniques, such as collection of an undisturbed sample using a syringe, or using tubing and a peristaltic pump. Variability of data from samples collected this way may be low and acceptable. However, these more complicated sampling techniques are not feasible on routine water quality sampling runs. So, we do not support imposing this depth limit on research sampling, nor would we support eliminating this depth limit on routine sampling. A separate, and possibly valid, concern would be that these different sampling techniques might limit data comparability between routine and research monitoring efforts. However, that comparison could, and should, be explored with a well-designed study.

p. 32, first bullet: It should be recognized that areas not undergoing any management changes still can experience water quality changes.

p. 32, third bullet: Field crews have observed that they end up taking samples at some locations, even when stage readings suggest that all areas would be dry, and vice versa.

p. 32, line 798: Wouldn't the variability of target measurements differ by parameter? Therefore, a sampling frequency that made sense for one parameter might not make sense for another.

p. 34, line 870-872: We believe that you have overstated this point that was raised during the workshops. Participants cited specific examples where data sets proved valuable in unexpected ways. Perhaps the best example was the extensive use of WCA-2A data sets in the development of the Class III TP criterion. Some of those sites existed long before it was understood that they could be useful in the criterion development. That said, most scientists probably would not make the blanket statement that "all data are valuable and that all monitoring should continue" without some specific support and examples.

p. 34, lines 874-876: The fact that data can be used for new, unanticipated purposes does not make those data less useful. If we collectively could anticipate and predict future uses, we probably would have enough ecosystem understanding that actual monitoring would not be needed!

p. 34, lines 877-879: This last sentence implies that the only consideration workshop participants gave to their own input was whether or not the data were free to them. This sentence discredits the time and good will participants have contributed to this effort, and the scientific input they have provided. We doubt this implication was the intention of the sentence, and some rewording might help.

p. 34, lines 892-895: Traditional monitoring networks are not likely to have serious flaws in their products. These networks follow well-tested and supported design concepts, and the data have proven to be useful in many, many contexts.

p. 35, lines 899-900: Again, possibly an overstatement. Up-front objectives are desirable, but they certainly can be broad enough to warrant the application of data to many questions, both present and in the future.

From Ernie Marks, DEP

The following are general comments based on Department staff review of the District's draft 2008 SFER Chapter 5: STA Performance, Compliance, and Optimization.

1. Comment: Line 154 page 5-7. Please include an explanation as to the .14 ac-ft diversion from the G-301 structure.

Response: Operations has been contacted to verify if this diversion occurred or if it is a flow calculation error. As soon as the verification is received, this response to the comment will be updated. The value is very small and occurred on one day in April 2007.

2. Comment: Line 238 page 5-9. Suggest modifying language to read "Vegetation reestablishment in some of the treatment cells had previously been very poor…" and Line 241 to " … an intensive rehabilitation effort was successful in enhancing vegetation reestablishment in the STA."

Response: The original language will be kept and the suggested language will be added.

3. Comment: Line 276 Page 5-10. Please elaborate. What useful info was gained as part of the intensive literature search?

Response: The literature search focused mainly on cattails. We are using this information to help refine the scope and design of our cattail research projects. The research results (and useful information from the literature search) will be used to develop guidelines for STA operations aimed at optimal vegetation performance under different depth-duration conditions, drought, etc.

4. Comment: Line 369 Page 5-14. Please explain how higher mass removal rates can occur but not result in lower outflow concentrations?

Response: Based on our existing knowledge base, most well performing STA cells can maintain long term outflow TP levels as low as about 15 ppb. One experiment that was conducted was to determine whether elevated calcium levels could result in even lower outflow TP levels, but the results did not show this to be true. However, it does appear that elevated calcium may influence the rate at which the wetland attains this outflow of 15 ppb, in other words resulting in a higher K value or mass removal rate.

5. Comment: Line 395 Page 5-14. Please provide additional information on studies that are occurring.

Response: The following text will be added: The first stage of the drought-related studies will focus on the physiological and molecular evaluation of the response of cattails to various water deficiency regimes in order to better quantify the magnitude of *the stress experienced by the plants. The experimental design will be made available to interested parties. In addition to the drought-related study, the District has also started a study evaluating the response of cattail to high water conditions and the same parameters measured in the drought study will be used to evaluate the plant response to high water stress.*

6. Comment: Line 664 Page 5-22. STA-6 Operational information in Table 5-6 is missing.

Response: The table was truncated and the entire table will appear in the final chapter. The entire table was emailed to the DEP prior to the public presentation.

7. Comment: Page 5-37: May want to expand on first paragraph as one may interpret this as a 3x increase to the phosphorus loading rate (PLR) of the facility (" …60 percent higher than the simulated long-term average annual inflow of this STA."), when in fact it is the PLR to the remaining cells as others were off-line.

Response: The following text will be added: "The hydraulic and phosphorus loading rates are calculated by dividing the inflow by the effective treatment area. These rates are affected when areas of the STAs are temporarily taken off-line for construction or rehabilitation".

8. Comment: Line 1147 Page 5-42: Suggest adding a statement after the last sentence to the effect of: " The District and the Department will continue to evaluate the reasons for DO depression at the G-251 pump station."

Response: The suggested text will be added.

9. Comment: Line 2076 Page 5-79: Please add a statement to the effect of " The District and the Department will continue to evaluate DO depression and possible influencing factors…" As there may be some relationship between low DO levels and vegetation types in the flow-way (See Appendix 5-4).

Response: The suggested text will be added.

Author	Message
Ernie Marks	**Subject:** **General Comments**

Total Messages 4

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Author	Message
Ernie Marks	**Subject:** General Comments

Total Messages 4

The following are general comments from Department staff upon review of the draft 2008 SFER Chapter 7A: Vol. 1 Comprehensive Everglades Restoration Plan Annual Report.

1. Line 417 – Statement that design activities are near completion for "all of the Acceler8 projects." Is this just for District A8 projects? Projects that have been given to the USACE have a different schedule. Suggest defining "near completion" (for example-post-60%/Intermediate Design).

2. Line 428+ - Suggest including reference to completion of design and construction of the C-1 Canal Improvements. Should reference to the proposed MOU Section 24 Impoundment and Pump Station for design, construction and operation by VOW be included here?

3. Line 435+ L-31E culverts permit has been issued & pre-final plans for Deering are due in late October. Cutler Flow-way design is scheduled for completion in November 2007.

4. Line 441+ – June 2007? Please clarify what year is being referenced.

5. Line 447+ - pre-final plans submitted in September 2007 with 1502 permit for construction pending.

6. Line 451+ – Please include the status of design activities.

7. Line 454+ – Design of the reservoir has not been "completed". Intermediate plans for structures and pre-final plans for the embankment will be complete by 9/07. Design of bridge for US 27 is still preliminary.

8. Line 464+ –Construction contracts have been delayed. Clearing work is underway or complete. Please update as necessary.

9. Figure 7A-2 – C-43 is 90% complete. C-44 construction is preliminary activities, not the reservoir.

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Review of South Florida Environmental Report – Appendix 2-1

Philip B. Bedient, Ph.D. P.E.

Civil and Environmental Engineering Dept.
Rice University
September 16, 2007

Introduction

I reviewed Chapter 2 and Appendix 2-1 which present hydrologic conditions and updated data with specific analysis of the South Florida environment for Water Year 2007. Chapter 2 provides a conclusion of the active severe drought taking place during 2006-2007 in South Florida based on data from the South Florida Hydrologic Monitoring Network. This severe drought was generally caused by more than a foot rainfall deficit over the region. Appendix 2-1 provides technical details on the network that collected hydrologic and hydraulic data for this study. This hydrologic monitoring network is spatially distributed over the geographic areas of the South Florida Water Management District (SFWMD) with sensors that record data specifically based on a time variant.

Given the significance of hydrologic and hydraulic data to the entire study for SFWMD, Appendix 2-1 illuminates the collection and validation of data for real-time water management as well as for data analysis that results in archival hydrologic records used to evaluate and assess the current status of water resources systems. Appendix 2-1 provides status and inventory of the network as of April 30, 2007, and includes progress on the network optimization or design studies beginning in 2002. The appendix describes the history and evolution of the network, sensors/instrument(s) used, number and locations of instruments, frequency of data collection, time interval of the available data, and optimization or design. Emphasis is placed on five major parts of the SFWMD hydrologic monitoring system: rainfall, meteorological, surface water stage, surface water flow, and groundwater. Brief description of the parts of the monitoring system is presented as below:

- Rainfall data used in the study was obtained from an extensive network of 287 rain gauges that SFWMD actively operates and maintains. Since 2002, SFWMD has been also acquiring radar rainfall NEXRAD data coverage.

- A meteorological monitoring network including 45 active weather stations provides temperature, barometric pressure, humidity, solar radiation, wind speed, and water temperature on breakpoint and daily time intervals. Specially, daily potential vapotranspiration (PET) data are available for 19 weather stations.

- A network of 1,265 active surface water stage gauges provides the surface water stage data for various water bodies.

- SFWMD owns a network of 446 active surface water flow monitoring sites that provide instantaneous and mean daily flow data in 15-minute intervals.

- The groundwater monitoring network has a total of 907 active groundwater wells on various interval basis (15-minute continuous, monthly, or greater than 1-month). SFWMD is responsible for monitoring, maintenance, quality assurance/quality control, data archival, and funding for 568 of these wells; while the U.S. Geological Survey (USGS) is responsible for the remaining 339 wells.

The data management procedure for processing the collected data follows summarizing, deriving, analyzing, storing, and publishing. Processed data are archived into two different databases: Data Collection/Validation Pre-Processing (DCVP) and DBHYDRO. End users can retrieve data from either of these two databases.

Answers and Comments to the Questions about Appendix 2-1

1. **Does the hydrologic monitoring network report provide necessary information on hydrologic monitoring networks of the District?**

   Answer: Yes, the hydrologic monitoring network report does provide necessary information on hydrologic monitoring networks of the District. As a hydrologist, I understand that obtaining data from an existing operational hydrologic monitoring network for such a region is a great advantage for SFWMD to conduct a study like this. This report provides detailed information in five major components of the network: rainfall, meteorological, surface water stage, surface water flow, and groundwater. The information from these five parts is very useful. Especially, real-time data can be used to various hydrologic/hydraulic modeling applications for SFWMD. The data among five components could be correlated to generate empirical equations describing hydrologic conditions of the South Florida region with respect to different variables from five major parts. If possible, information on population, agricultural and industrial consumption should also be considered in this analysis.

2. **How can the existing hydrologic monitoring network be made more efficient and cost effective?**

   Answer: To make the existing hydrologic monitoring network more efficient and cost effective based on the current technologies, optimization of all weather stations, rain gauges, flow and stage gauges, and groundwater wells would be the most feasible means at this point. By optimizing the gauge locations, we can not only screen out unnecessary gauges that are currently employed, but also move those gauges and add more gauges where they are needed. By doing so, the network will be made more efficient and cost effective. Moreover, using NEXRAD radar technology may be more efficient and cost effective than installing more rain gauges and other weather stations to obtain rainfall and other meteorological data. In other words, I believe that fully applying the existing NEXRAD radar functions to the South Florida Hydrologic Monitoring Network will help lower the current maintenance and installation costs for the monitoring network.

3. **What additional information should be included in the hydrologic monitoring network report to improve the utility of work product?**

Answer: First, as I mentioned above, since optimization could be the most feasible means to make the network more efficient, I recommend having more detailed methodology on gauge optimization in the report. For example, 332 (154 proposed and 178 existing) rain gauges were suggested by the rain gauge network optimization study (page 62); however, the authors did not describe or explain how this number was arrived at. Additionally, on page 62, I could not determine where the net increase of 53 gauges comes from.

Second, the method used to calibrate NEXRAD radar rainfall data using local rain gauges should also be included in the report. There are four NEXRAD radars in the South Florida region, but relatively fewer rain gauges are located in the South Florida region (150.6 km^2/gauge after adding proposed rain gauges) compared with other regions due to vast areas that are undeveloped. Some areas covered under the radar beams may not have rain gauges to calibrate the radar rainfall data. For these scenarios, the authors may need to give a little more explanation on quality control of the data. The authors should also list specific rain gauges chosen to calibrate specific NEXRAD radars. Quality control of the radar rainfall data is critical to hydrologic/hydraulic modeling applications and flood warning using real-time radar rainfall data as direct input in order to achieve more accuracy.

Third, soil conditions that can be categorized as a variable (such as moist deficit) into groundwater section should also be included in the report. Generation of overland runoff depends on the soil conditions (dry or wet). Including soil conditions in the report will be beneficial to the applications in hydrologic/hydraulic modeling and flood warning.

4. **The report indicates that the longest consistent measurement record is from 1995 to 2005. How can past data be used with current data for longer-term trend analysis? What techniques are used to 'correct' past data to be compatible with value being generated currently? Should information be added about the equipment used in past measurements?**

Answer: This would need to be addressed by a team of specialists including hydrologists and statisticians familiar with this complex problem.

Peer review of
" Hydrological Monitoring Network of the South Florida Water Management District," Appendix 2-1 to the 2008 South Florida Environment Report – Volume 1
by Rafael L. Bras

Introduction

This report responds to a request by the South Florida Water Management District and a Statement of Work that called for:

- **Read and evaluate the draft 2008 SFER – Volume I, Appendix 2-1: Hydrological Monitoring Network of the South Florida Water Management District, and other sections of the Report for additional information, as needed; and**
- **Prepare a detailed technical review of draft Appendix 2-1, including answers to questions noted below and any other comments that the reviewer may wish to contribute.**

The questions posed were:

1. Does the hydrologic monitoring network report provide necessary information on hydrologic monitoring networks of the District?
2. How can the existing hydrologic monitoring network be made more efficient and cost effective?
3. What additional information should be included in the hydrologic monitoring network report to improve the utility of work product?
4. The report indicates that the longest consistent measurement record is from 1995 to 2005. How can past data be used with current data for longer-term trend analyses? What techniques are used to 'correct' past data to be compatible with values being generated currently? Should information be added about the equipment used in past measurements?

The following addresses the questions in the order given and ends with a section on **Specific Comments** on the document.

Question 1: Does the report provide the necessary information?

The report is complete and does provide a good overview of the monitoring network. An interested reader can get a good idea of the various networks and obtain sufficient information to investigate further and obtain more detail.

Although acceptable, the organization of the report can be improved and repetition minimized. There is some repetition caused by the use of separate network and data sections in each chapter. This repetition is most evident in Chapter IV dealing with the rainfall monitoring network where, for example, multiple figures provide the same information. More significantly, consideration should be given to merging Chapters V and VI. It is hard to separate the surface stage network from the flow monitoring network since stage is largely used to estimate flow. The similarity of tables 14 and 13 are evidence of this.

Despite the apparent care taken during writing, I still get confused about the roles of the different databases: DCVP and DBHYDRO.

The **Specific Comments** section of this report will deal with other issues related to clarification or improvement of text.

Question 2: How can the monitoring network be made more efficient and cost effective?

The biggest "inefficiency" of the present approach is that elements are dealt in isolation and treated independently. Precipitation and stage/flow information are intimately related. So are the surface and ground water. Knowledge of precipitation tells us something about flow and vice-versa. The District has invested in very good models that integrate all these variables. Ultimately they should become integrating agents that assimilate data and can be used to augment observations or even substitute observations of one type (i.e. flow) because assimilation of another (i.e. precipitation) produces accurate predictions of the variable of interest.

The above integrative approach can be used within a more limited context. For example the report mentions that radar precipitation measurements have been used to help optimize the raingage network. It is nevertheless too vague on how that was done. Similarly the reviewer is aware of efforts to estimate evaporation using other information from meteorologic and atmospheric model products. The optimization of stage and flow monitoring are naturally linked. As stated before it would make sense to link the precipitation and the flow networks and design them conjunctively.

What is missing in the discussion is a crisp statement of the prioritized uses of the various data sets and how accurate they need to be in order to achieve management, design and operational objectives. This is needed in order to properly evaluate the adequacy of the systems.

Question 3: What additional information should be included in the monitoring network report?

The most glaring piece of missing information is a statement of the accuracy of the various data sets. There is no discussion of how good is the information. This is in part due to the fact that no statement is made of how good does the information needs to be to satisfy the goals of the District. Quantifying accuracy of measurements and products and discussing the implication of that accuracy on the uses is the most important task yet to be done.

For clarity it may be useful to show a few examples of time series and products. An appendix with scripts or other utilities to facilitate access and display of data will also help.

The report could benefit by having more thorough explanations of a few points. For example: why was the radar data provider changed? What is the implication to continuity of data and its quality? Can algorithms used to process radar data be specified? The two providers use different algorithms for processing and calibrating radar and raingages, what are the implications of this switch? Clearly the data quality will not be the same. Why isn't radar becoming the precipitation data of choice? How does the quality of gauge and radar data compare? Why are some stations chosen for post (additional) processing and quality control? What is the logic for the choice?

The report could discuss data uses and lead to a better appreciation of why is the data collected and the quality required.

Question 4: How can past data be used with current data for longer-term trend analyses? What techniques are used to 'correct' past data to be compatible with values being generated currently? Should information be added about the equipment used in past measurements?

I do not believe that it is appropriate to “correct” past data. I am nevertheless in favor of using all available information. The key, as previously stated, is to provide a quantitative assessment of accuracy so that the information can be used intelligently. The accuracy will naturally vary with the data and the sources of data.

Having said the above, the District should seek to augment data records in everyway possible. For example, the reviewer is aware of efforts to use PRISM (Parameter-Elevation Regressions on Independent Slopes Model) as supplemental precipitation information. Similarly the use of model reanalysis products to derive long-term evaporation and transpiration estimates (1895-2005) is the right approach and should be pursued.

The effort performed as part of the conceptualization of the Natural System Regional Simulation Model provide a wealth of historical information which at least qualitatively can be used to augment existing data records (clearly the error associated with these data must be larger than those obtained from direct measurement).

It is difficult to speculate on methodology that could be used for augmenting data without an in depth knowledge of what other historical data sources may b used. Nevertheless and consistent with previous comments I would venture to say that engaging in data assimilation with models, in a hindcast framework, could lead to complete utilization of all available data. In theoretical jargon this would be akin to a backwards-filtering process. There are also various Bayesian and non-parametric tools that may be used. The difficulty with all this is that generally assumptions of statistical stationarity of the system must be made. For some variables this is clearly not the case. I am partial to the use of models as integrating elements; models are a strength of the District.

Information on equipment used in past measurements should always be given in order to supplement and explain error estimates.

Specific Comments

The following comments are organized by report page and line numbers.

P 2-1-1

Lines 14-16. Re-write, it is expressed strangely, particularly the use of “time variant”

Lines 28-29. Estimates of PET are not data.

Line 33, “mean daily flow data at 15 minutes intervals” sounds like an impossibility.

P 2-1-2

Line 82. The expression discharge “to tide” is unclear; why not simply discharge to the ocean.

P2-1-3

Line 112. Acceler8 and CERP should be described/defined.

P2-1-4

Lines 156-157. Does not make sense to me.

P-2-1-5

Lines 161. At this point DBHYDRO is not defined.

Line 163. Use described instead of recorded

Line 165. Why use the word “however”.

Lines 165-173. This paragraph really makes no sense; it is full of jargon

Line 177. Missing data ….equipment malfunction including sensor ..

Lines 177-180. Should be at the end of that paragraph

Line 199. “…the network is defined as the collection”

Line 200. “… and record time varying data, i.e. ….

Line 201. “For each network the report includes the …”

P2-1-6

Line 227. “ Microsoft Excel spreadsheets …”

P2-1-11

Line 358. “from the DCVP”

P 2-1-13

Line 379. This sentence makes no sense to me. I would hope that all intervals “use the full extent of data available”

P2-1-15

Line 460. “its large size”

Line 463. Eliminate the first section of the sentence, start “The district ..”

Line 472-473. The use of “Potential evapotranspiration (PET), or reference evapotranspiration” sounds like they are the same. They are not.

P 2-1-16

Line 477. “Actual or crop …” implies they are the same, they are not. The use of crop evapotranspiration and crop coefficients that commonly appear in District literature should be discouraged.

Line 483. “the Simple Method”

Line 499. “a cumulative”

Line 517-519. Something is missing, makes no sense.

P 2-2-18

Line 576. “the measurement accuracy is variable”

P 2-1-23

Line 672. What is “capacitive” pressure?

P 2-1-28

Line 809. It is confusing that 11 active pan evaporation stations are mentioned here while there are 12 in Table 2. I imagine it is the arbitrary date but it is confusing.

Line 811. How do the simple method results compare to the pan evaporation? How is the pan evaporation used? No adjustments are made to those measurements?

P2-1-29 Table 4 never defines what DBKEY is.

P 2-1-35

Line 946. As I understand it the 19 weather stations are used to obtain PET estimates, there is no direct data collected on PET.

P 2-136

Line 977. “In that study…”

P 2-1-52

Line 1305. It would be good to say something of why was the radar data provider changed. How are the records of the two providers reconciled given that they probably use different processing algorithms?

P2-1-53

Line 1321. should you use the past tense “provided”? Past tense should be used throughout this section.

P2-155

Figure 25 is unreadable

P2-1-59

Table 12, can you explain the logic, if any, of the naming of stations?

P2-1-61

Line 1414. “Rainfall data since 1955 has been stored within DBHYDRO. It includes …. stored in the DCVP database.”

Lines 1423-1431. Why repeat this so many times?

Lines 1434-1440. Why repeat this so many times?

Line 1449. “study of the existing”

P 2-1-67

Line 1631. From this point on references are made to figures 29,30,31, etc. It is confusing that the text sometimes refers to hundreds of stations when clearly the figures do not show that many. Is it resolution? You must explain and make it clear. Figures should show the same number of stations referred to in text or an explanation is warranted.

P 2-1-71

Line 1669. Same comment as above, figure 32 does not show 300 stations.

Line 674-1683. This is repeated.

P 2-1-75

Lines 1780 –1785. This is a discussion of the use of stage data, which goes to the general comment that chapter V and VI should be combined.

P 2-1-84

Line 1921. Figure 41 does not show 253 culverts.

P 2-1-92

Line 2108. Figure 50 does not show 446 stations.

P 2-1-100

Lines 2313 – 2316. The figures do not show the number of points mentioned in the text.

P 2-1-112

Table 18 is cut off in the left margin, at least on my version of the material.

P2-1-114

Line 2474. “are being transferred into DBHYDRO”

Review of Appendix 2-1 of Volume 1 of the

2008 South Florida Environmental Report:

Hydrological Monitoring Network of the South Florida Water Management District.

Submitted by:

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September 18, 2007

Introduction:

The SOW for this review asked for a detailed technical review of the draft Appendix 2-1 of the 2008 South Florida Environmental Report. This review was to specifically address the following questions:

1. Does the hydrologic monitoring network report provide necessary information on hydrologic monitoring networks of the District?
2. How can the existing hydrologic monitoring network be made more efficient and cost effective?
3. What additional information should be included in the hydrologic monitoring network report to improve the utility of work product?
4. The report indicates that the longest consistent measurement record is from 1995 to 2005. How can past data be used with current data for longer-term trend analyses? What techniques are used to 'correct' past data to be compatible with values being generated currently? Should information be added about the equipment used in past measurements?

Accordingly, this review is divided into five sections: The first provides a general technical review. The next four sections address the each of the above four questions.

General Technical Review:

This Appendix describes one of the most thorough hydrologic monitoring systems in the country, namely that of the South Florida Water Management District. The monitoring, collection and management of hydraulic and hydrologic data supports the water supply and ecological planning, management and control activities of the District. Such information is critical to the success of those activities. The Appendix describes the monitoring network as it existed at the end of April 2007, and includes progress on the District's network optimization or design studies that began in 2002.

Sections of the report focus on each of the different hydrological parameters being monitored. These parameters include rainfall, water stages (both surface and groundwater), water flows, and groundwater. It does not describe any water quality monitoring. The methods used to obtain, collect, and manage the data, are described. Data management includes processing the data collected, creating the appropriate meta data describing how, when and where the data were obtained, summarizing, analyzing, correcting, storing, and publishing. The Appendix does not discuss in much detail how data quality assurance/quality control (QA/QC), is accomplished. References, however, are provided.

The system as it exists today has evolved over the past decades. Only relatively recently have efforts been made to design the networks in ways that can provide the needed temporal and spatial resolution and accuracy in cost effective ways. This 'network optimization' effort continues, as budgets permit. Monitoring instrumentation (sensors) and data management technology are also advancing and upgrading to more cost effective and accurate technologies is a continuing expense. The issue to address is just what upgrades are justified given the cost and the added benefits of such improved data. The Appendix does not address just how such tradeoffs are being identified.

The Appendix provides a clear description of the history and evolution of each part of the monitoring network. It provides information on the number, types and locations of the various sensors/instrument(s) being used, the frequency of data collection, and the time interval of the available data. For each network component the Appendix indicates whether or not design optimization is taking place. It does not provide much detail on these optimization efforts.

I <u>Introduction</u>

The introduction to this Appendix describes the mission of the South Florida Water Management District (SFWMD or the District) which clearly identifies why such extensive hydrologic (water quantity) monitoring networks are needed over this vast area of Southern Florida. It also makes this reader wonder why networks measuring water quality and ecosystem parameters are not also as extensive, but these topics are not discussed in this Appendix.

Line numbers 109 to 112 state that "The District requires accurate data collection, processing and archival of the data collected by the hydrologic monitoring network for many purposes. There is a constant need to add new stations/sites with instrumentation for hydrologic data collection within the District, and this need will grow faster as the Acceler8 and CERP projects are implemented." While I believe this is true, I also believe the Appendix would be enhanced if some explanation of why this is true were included. Data costs money. For those making these budget decisions some analyses showing that the benefits, however measured, of improved information justify the added costs would be beneficial.

The text on lines 118 through 131 focuses on the optimization of data monitoring networks and suggests that there is some optimal number and location of such monitoring sites and the sensors used. Isn't there a tradeoff between cost and the value of improved monitoring networks? Just what is optimal may be difficult to determine unless the value of improved information can be expressed in monetary terms – which I think is problematic at best. If different criteria are to be used for different hydrologic parameters, as suggested in lines 127 – 129, what are these different criteria? The problem of deciding just what is optimal gets even more difficult given the different uses of such data, as expressed on lines 133 – 147. What spatial and temporal data resolution is 'best' for each different use will no doubt differ. And finally, the problem gets even more complex when the needs for current data by future researchers are taken into account, since we don't know what these needs may be with any degree of certainty today.

Section D beginning on line 149 highlights the need for meta data that includes some estimates of the errors associated with different sensor technologies. When new sensors are introduced, it is often beneficial to keep the old sensors operational for a while just to compare the different data values obtained by both sensor technologies at the same site. (This issue is discussed again under question 4.)

II Hydrologic Data Management

This section of the Appendix describes the District's data collecting and monitoring network and the Supervisory Control and the responsibilities of the Data Acquisition (SCADA) and Hydro Data Management (SHDM) Department that is responsible for data collection and management. The section summarizes what happens to the data once it is created. Once the data are processed and verified, or quality controlled, they are stored in appropriate data bases. Instantaneous (breakpoint) data are stored in the Data Collection/Validation Pre-Processing (DCVP) database and daily summary and15-minute interval datasets are published in the DBHYDRO database.

I did not see any discussion of what happens to the original data prior to its being modified if found in error. It seems to me the original data, no matter how much it may be in error, should be stored somewhere so that it can be made available to those who may sometime in the future want to refer to those data should questions arise on how those data were modified prior to being placed in the appropriate data bases.

Lines 424 to 428 state "In post-processing, missing data may be estimated with data estimation techniques and processes such as spatial and temporal interpolation, and statistical or simulation-model applications. Erroneous data can be replaced with higher quality data, be deleted, or qualified and tagged." Once again I would think there would be occasions when analysts may want to see the original data just to check if procedures to correct erroneous data were properly used, or if indeed newer correction procedures became available, compute improved values based on the original measured data (see the discussion in question 4).

III Meteorologic Monitoring Network

Meteorological monitoring involves measuring and managing information on rainfall, wind speed and direction, air and water temperature, relative humidity, barometric pressure, and solar radiation. Such information is useful in estimating soil moisture, evapotranspiration and wildfire potential. There are relatively few full-service weather stations present on the District's lands, but the spatial resolution of parts of them, such as rainfall, is much greater. In other words, there are many more rainfall monitoring sites than weather stations. Most of this section of the Appendix is devoted to the types of sensors used to collect meteorological data.

Apparently the present meteorological network has evolved without any overall plan for consistent District-wide coverage at levels of accuracy that are considered desirable. The Appendix (line 984) suggests that meteorological network design studies are being planned to address the question: If more meteorological stations should be installed and maintained, I would like to see some discussion of the results of the analyses justifying decisions on where they should be located, and when and why.

IV Rainfall Monitoring Network

South Florida's hydrology is driven by rainfall. Because rainfall plays a dominant role in water management, and because knowledge of rainfall in near real time substantially contributes to improved water level management, there exist numerous rainfall gauges located throughout the District. Deficiencies in the proper locations of the current gauges are being addressed based on a recent 'network optimization' study.

This section of the Appendix also describes some of the common sources of errors in rainfall measurements associated with various types of rain gauges in South Florida. Wind velocity is a major one. In addition, because of the variability of rainfall in South Florida, especially in the summer when thunderstorms often occur, the density of rainfall gauges becomes more important, as do the statistical methods of converting point data to area-wide data. These technological and analysis limitations and issues are discussed, but to the extent they are resolved, or not resolved, is not as clear as I would like. Does it make any sense to implement a project that would enable the accurate measurement of rainfall over a relatively small area and compare it to various rainfall gauge data obtained from the same area?

The section ends with an insightful discussion of the use of radar to supplement gauge data. I can envision the use of radar-based data to be the input to models such as the Regional Simulation Model that has a variable grid size, as well as the 2x2 hydrologic simulation model that apparently is already taking place.

Part E (beginning on line 1442) of this section discusses the recent optimization studies for rainfall network design. I would be interested in having more detail presented here, or at least have some references to any documents giving more detail. For example, how is the number and location of rain gauges influenced by the demand for more detailed data at some locations compared to others, and how is the cost of obtaining and processing this information used in the optimization analyses?

V Surface Water Stage Monitoring Network

Since the early 1900s when the first manual surface water stage gauges were installed by the USGS, the surface water stage gauge network has evolved up until the present time, again without much thought of it being a system, but more to meet local needs. These gauges are critical for the operation of local water control structures and to determine whether or not stage targets are being met. The accuracy of the data again depends on the type of sensors being used to measure the stages. This section describes the types of sensors being used and some of their limitations or causes of errors.

The last part of this section discusses network optimization. Water stage guage network optimization has been performed (a reference is provided), and subject to funding, will be implemented in the future. Without knowing the details of this study, it seems to me funding might become more likely if it is shown that the benefits of such optimization outweigh or exceed the costs. There are tradeoffs. The Appendix does not discuss, even qualitatively, the benefits to be derived from implementing an ‘optimization’ solution. Will there be a cost savings from eliminating duplication? Will there be better – more accurate – data on which to base operational decisions? Even if such a discussion is included in the referenced report, it would be good, I think, to include it in this Appendix as well.

VI Surface Water flow Monitoring Network

Just as stage data are critical to many operational decisions, so are flow data. Flows are influenced by rainfall and management decisions at flow control stations. The flow monitoring network has grown substantially since the 1950s, and the number of flow gauges is expected to continue to increase in the future due to the increasing demand for such information for ecosystem restoration efforts.

As in the other sections of the Appendix pertaining to other hydrologic parameters, this section describes the types of monitors or sensors and their location. However this section also mentions, but does not present, the equations needed to derive flows from what can be measured, such as stage, velocity, pressure, gate openings, pump speed, etc. Do meta data exist for flow data that describe in detail how they were derived? It seems to me these data upon which flows are based should also be stored or archived so that it is possible to check on these derived flow data at some future date should the need occur. It is not clear from what I read that these measured data are saved somewhere.

Part D of this section (lines 2144 – 2166) discusses the optimization of this network of monitors, but without much detail on the results. Perhaps some of the motivation and results of this optimization study would be of interest to readers of this Appendix.

VII <u>Groundwater Monitoring Network</u>

This Appendix concludes with a description of the groundwater aquifers in South Florida and the types of sensors used to monitor groundwater supplies. Like the other sections on the other parameters, this section presents (1) the development of the groundwater monitoring network and history and evolution of the network; (2) the existing groundwater network, as of the end of April 2007, (3) the QA/QC procedures for the water level data and data availability; and (4) the future groundwater network design. monitoring network.

Based on the list of uses of such data (lines 2259-2269) it appears groundwater quality data are not being collected. Is groundwater quality an issue? Is it being monitored? If not, should it be monitored? Isn't groundwater quality an issue of concern in the vicinity of ASR operations? I found groundwater quality mentioned only on line 2513, and this is in relation to developing a groundwater monitoring network design project for future implementation.

Lines 2428 to 2434 mention that a "consultant performs additional QA/QC to groundwater data series used in support of the District's reporting, modeling, and regulatory programs. The consultant evaluates groundwater level measurements, performs temporal and spatial statistical analyses of the data, verifies reference elevations, fills missing data, resolves hydrogeologic problems, and documents all analyses in technical reports. The groundwater data is then assigned a new database key (dbkey) that indicates that it has gone through more thorough QA/QC procedures. This project is done annually subject to funding availability." I have two questions that I think could be usefully addressed in the Appendix: What procedures are used by the consultant to improve the accuracy and completeness of the time series data? Are the same procedures used by the USGS regarding the data they collect and manage? If not, is this an issue? Second, what if there is not sufficient funding available in any particular year or years; do the data get placed in a data base without having gone through QA/QC procedures?

Question 1: Does the hydrologic monitoring network report provide necessary information on hydrologic monitoring networks of the District?

The report provides a comprehensive description of the main hydrological data monitoring networks in the District's lands in South Florida. The monitoring network of each hydrological parameter, whether by the District or by another organization as of the end of April, 2007, is discussed and illustrated on maps of the District. The equipment used in each network is described and illustrated, along with a discussion of its limitations and possible errors. Finally, each section of the report discusses how the data are managed and stored in databases. In my opinion the Appendix provides an excellent, but general, overview of the District's hydrologic monitoring networks, One can always find some mistakes or inconsistencies or think of information that could have been included in the report. I have indicated in the discussion above various topics that I would be interested in having in this Appendix. For example, I think the Appendix could be improved if it devoted a little more attention to each network's optimization initiative, clearly defining the multiple criteria used for evaluating alternative network locations, for deciding on the numbers and types of monitors, and the accuracy of the data obtained. Similarly, I would have liked to have in the report more discussion of the QA/QC procedures used to improve the accuracy of the data.

Whether or not the monitoring network report provides the necessary information, surely depends on what the reader wants or expects from the report and what the authors intended to cover, their intended audience, and the amount of detail appropriate for that audience. As a general overview this Appendix seems to me to be a very informative and readable document. For those like me that perhaps would like some additional detail in certain topics, I guess we can always refer to the references.

Question 2: How can the existing hydrologic monitoring network be made more efficient and cost effective?

The optimization studies for network design appear to be addressing this question. I believe there is no single solution that will be considered optimum by all users of these monitored data. Water managers will want the most current and accurate data available to better regulate the system and meet expected flow and storage (stage) targets throughout the District. Yet this near-time information costs money, and money used for other activities can also bring value or benefits to the District. Hence I see the tradeoff between the benefits achieved from having more comprehensive and accurate data from each monitoring network, however measured and what ever metrics (units of measure) used, and the costs of doing that. The actual decision on how much to spend for improved monitoring systems could be better informed if such benefit data are available. Such analyses involve considerable scenario generations and sensitivity analyses.

Clearly if some existing monitors can be eliminated without significant loss in spatial and temporal coverage and data accuracy one should do that to achieve increased cost-effectiveness. If one were to employ cost-effectiveness optimization, the objective would be to minimize the costs of achieving a specified level of timely knowledge of the values of particular hydrologic parameters. The constraints of this optimization model will need to define the relationship between the level of knowledge (amount, timeliness and accuracy of the data) obtained as functions of the monitoring network. The unknown decision variables are the locations, types and frequency of monitoring devices or sensors that provide this knowledge. I suspect that rather than a formal optimization model, simulation and sensitivity analysis procedures might be a more practical way of addressing this cost-effectiveness question.

Question 3: What additional information should be included in the hydrologic monitoring network report to improve the utility of work product?

The additional information I would like to see included pertains to the previous question. How are the relationships between the amount and quality of information made available, and its timeliness, impacted by changes in the network of monitors, the types and locations of monitors, and the frequency of monitoring, data communication, and data management, and what are the resulting costs? These relationships it seems to me are needed to make informed decisions regarding the allocation of funds needed to improve the cost-effectiveness, or even the net benefits, obtained from these hydrological monitoring networks. (Of course, if legal regulations impose requirements on the data needed, then the need to identify the benefits of better information may be mute. It's simply a cost effectiveness problem.)

The report serves two purposes. It provides a summary description – an overview – of the existing (as of April, 2007) hydrological networks, and it does this very well. The second purpose is to indicate that during the past few years there has been an effort to view these monitoring networks as a system, and that the system needs to be made more cost effective. Various networks are now being upgraded accordingly, but the details of just how upgrading decisions are made, or of the basis by which such decisions are made, is not described in this report. Since this is one of my particular interests, I guess this is why I would have liked to have seen a more complete description of these efforts.

In addition, very little is said about water quality or ecosystem parameter monitoring. Is the monitoring of these important sets of parameters included in other reports?

Question 4: The report indicates that the longest consistent measurement record is from 1995 to 2005. How can past data be used with current data for longer-term trend analyses? What techniques are used to 'correct' past data to be compatible with values being generated currently? Should information be added about the equipment used in past measurements?

I find the first part of this question difficult to answer in general. Much depends on the particular situation.

Past data can be used with current data if they are of the same, or at least nearly the same, precision. The question pertains to situations where they are not similar with respect to their errors. Adjusting past data to account for possible bias errors due to less precise equipment or methods of measuring may be difficult at best unless those errors, relative to current measurement data, are known. Without the original measurements this task is especially challenging, and this is one of the reasons for the suggestion made earlier in this review that original data, no matter how much in error and before any adjustments are made to account for errors, should be saved somewhere. Comparing the measured values of the same data (such as rainfall or stage or flow velocity or relative humidity) using both the old and new sensor technology is a reasonable way to estimate the possible errors associated with the older sensor technology relative to the values obtained with the newer, and presumably more accurate, sensor technology.

If such overlapping and comparable data do not exist, then the correction procedures will have to be made based on models that define the error bias in the older data - based an understanding of the cause and extent of the errors - with possibly a random component that creates the same variance in the errors as existing in the newer data. Once this is done the records can be considered, with some but not complete confidence, as being from the same source, and having the same errors. Then trend and other analyses, as desired, can be performed. One should be careful not to confuse changing sensor technology errors with changing trends in the actual hydrologic parameters. Changing trends could result from changing management policies as well as natural causes, depending of course on the parameter being measured.

Once past data have been made comparable with current data, one can check if the parameters of the probability distributions of these data are changing. For example, if daily values are being recorded, the data could be divided into 5-year periods (or an n-year time series record into n-1 overlapping 5-year periods) and the mean, variance and skewness (the first three moments) of each of these 5-year data sets could be examined for trends. Exactly what can be done, and how it can best be done depends upon the statistical characteristics of the various series, including perhaps their cross-correlations.

For example, analyses might show that the observations at the new gauge have the same distribution (daily mean and variance) as the old observations. Or the observations may have changed by some fixed percent because of local weather effects. If such a relationship can be established, then a composite extended series can be constructed which would serve as the basis of a trend test.

For example, one might construct a model of the two (old (i =1) and new (i =2)) time series of observations X_{it} so that both extensions and trends can be addressed together, and thus the statistical implications of extension can be addressed when a test is made for trend. Consider the model:

$$X_{it} = mean_i + c\ trend(t) + E_{it} \qquad \text{for } i = \text{series 1 or 2}$$

where

$mean_i$ = mean of series i (to be estimated), i = 1,2

trend(t) = trend function such as b*t

c = coefficient of trend function.

E_{it} = independent error terms for each observation.

Can we assume that Var[E_{1t}] = Var[E_{2t}}? If not is there a trend? It would probably be necessary to include cross-correlation between E_{1t} and E_{2t}, describing the correlations between observations at the two sites in the same period.

Such extension ideas are also discussed in

Vogel, R.M., and J.R. Stedinger, Minimum Variance Streamflow Record Augmentation Procedures, *Water Resources Research, 21*(5), 715-723, 1985.
and chapters 17-18-19 of D. Maidment (ed.), *Handbook of Hydrology*, McGraw-Hill, Inc., New York, 1993

Basson, M.S., R. B. Allen, G.G.S Pegram and J.A. van Rooyen, 1994, Probabilistic Management of Water Resource and Hydropower Systems, Water Resources Publishers, Highlands Ranch, Colorado. Chapter 3.

A classical analysis of this model would be reasonable. If uncertainty in the variance and covariaince terms for E are a concern, a Bayesian analysis might be feasible. See Gelman, A., Carlin, J.B., Stern, H.S., and Rubin, D.B., Bayesian Data Analysis, Chapman & Hall/CRC, Boca Raton, 1995.

If several series were measuring the same hydrologic parameter in an area, then one might extend this model to i = 1, 2, ...k. It might also be possible to add other explanatory variables to make the analysis more powerful (such as rainfall added to a model of flow or stage) as in:

Patricia L. Bishop, W. Dean Hively, Jery R. Stedinger, Michael R. Rafferty, Jeffrey L. Lojpersberger, and Jay A. Bloomfield, Multivariate Analysis of Paired Watershed Data to Evaluate Agricultural Best Management Effects On Stream Water Phosphorus, *J. Environ. Qual. 34:* 1087-1101, May/June 2005.

I know this sounds very general and obvious, but it is hard for me to be more specific without knowing and analyzing specific data from specific sensors. General hydrological data checks to ensure that there are no major bias errors in individual measurements compared to the set of measurements, whether in error or not, include double mass plots and regression based outlier identification methods. These and similar methods are not able to detect a consistent bias error that can result from measurements using faulty equipment or incorrect measurement procedures, for example.

To address the last part of the question, in my opinion definitely meta files should be created for all data collected from monitoring sites. This meta data should include all factors that govern the accuracy of the measured data, including a description of the equipment used, the particular site characteristics that could lead to errors, and the way the data are collected and analyzed or modified before they are placed in a data base.

October 22 2007

LimnoTech Comments on

2008 Draft South Florida Environment Report

Appendix 3B-3: Approaches to Modeling Sulfate Reduction and Methylmercury Production in the Everglades

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SUMMARY OF LIMNOTECH COMMENTS

We preface our comments by noting that LimnoTech reviewed only the material in Appendix 3B-3 and did not have access to any of the actual data, model inputs, or model outputs described in the appendix.

LimnoTech concludes that even though there does not exist a complete understanding of the controlling biogeochemical processes, especially those involving sulfide, the science underlying the diagenetic model appears sound and reasonable.

LimnoTech concludes that the study results do not support the claims that the diagenetic model and the Everglades Mercury Cycling Model (E-MCM) could now be applied to predict the distribution of MeHg across the Everglades Protection Area (EPA) under changing sulfate loading scenarios, or that reduction in Everglades surface water sulfate will help mitigate MeHg production across the EPA.

LimnoTech concurs with the study conclusion that further analysis is needed of the relative contributions of changes in sulfate, Hg loading, chloride, dissolved organic carbon (DOC), and other factors (both before 1995 and during 1995-2003) to the observed declines in Hg concentrations in fish.

INTRODUCTION TO LIMNOTECH COMMENTS

LimnoTech is under contract to the Everglades Agricultural Area Environmental Protection District (EAA/EPD), through the Community Watershed Fund, and was directed to conduct a critical review of the 2008 Draft South Florida Environmental

Report (SFER), Appendix 3B-3: Approaches to Modeling Sulfate Reduction and Methylmercury Production in the Everglades.

The objective of the work described in Appendix 3B-3 was to develop a sediment diagenetic model for incorporation into the Everglades Mercury Cycling Model (E-MCM) to better account for the effect of sulfate reduction and sulfide presence on methylation of mercury (Hg) in Everglades sediments.

Appendix 3B-3 contains references to development of the E-MCM and its previous applications to Water Conservation Area (WCA) 3A-15. It should be noted that we have not reviewed the E-MCM itself or any of these previous applications. Appendix 3B-3 contains only the modeling sections of a larger study on sulfur and its relationships to mercury in the Everglades. The modeling study itself is in an early phase of development and is still a work in progress.

APPENDIX 3B-3 SUMMARY

The specific modeling objectives were: (1) to develop a digenetic transport-reaction model that is capable of predicting the depth distribution of Hg methylation as a function of soil biogeochemistry; and, (2) to explore how that model could be used to improve the ability of the E-MCM to predict responses to changing sulfate concentrations in the Everglades. Once developed, it is envisioned that one or both models would then be available to help predict how ecosystem restoration and potentially, sulfate management practices, could affect MeHg production and bioaccumulation in the Everglades Protection Area (EPA).

The specific goal of this study was to construct a model that is capable of predicting the depth distribution of Hg methylation in Everglades soils across a range of surface water sulfate concentrations.

In our opinion, even though there still does not exist a complete understanding of the controlling biogeochemical processes, especially those involving sulfide, the science underlying the diagenetic model appears sound and reasonable. Although the model is still in an early phase of development it appears to be a promising first step towards incorporation of the influence of sulfate reduction and sulfide on methylation of Hg in Everglades sediments.

It was asserted that outputs from the diagenetic model for site 3A15 "accurately predicted the concomitant declines in sulfate reduction rate and MeHg production at this site over time, providing mechanistic support for the hypothesis that sulfate declines are driving at least part of the observed decline in MeHg at this site."

This assertion is at odds with the more qualified interpretation of these same results on Page 11 in the body of the report:

"Although the predictions do not precisely match the highly variable observed data, the basic trend of decreasing methyl-Hg with decreasing sulfate abundance is similar. These results suggest that changes in the abundance or activity of Hg-methylating SRB (sulfate reducing bacteria) could be responsible for the decline in methyl-Hg abundance in 3A15 sediments observed over the past 10 years."

In addition, if our understanding of the results in Figure 5.8 is correct, predicted sulfate concentration was compared with observations, not predicted sulfate reduction rate.

It was asserted that both the diagenetic model and the sulfate-dependent versions of the E-MCM were able to reproduce the decline in MeHg concentrations at 3A15 over the 1995-2003 period, and that these results are consistent with the hypothesis that sulfate reduction affects net methylation rates in Everglades marshes.

Within the context of the above qualified interpretation of results from the sediment diagenetic model at 3A15, we have no disagreement with this assertion, but do note that much more could have been done with both the diagenetic model and the E-MCM in terms of sensitivity and diagnostic analyses to support this claim.

The authors qualified the E-MCM results at 3A15 by noting that further analysis is needed of the relative contributions of changes in sulfate, Hg loading, chloride, DOC and other factors both before 1995 and during the simulation period to the observed decline in fish Hg concentrations. We strongly concur with this need and note again that much more could have been done with the model to investigate these factors.

Another stated goal of this modeling work was to create models that can reproduce MeHg concentrations across the large sulfate and sulfide gradients found in the Everglades. It was found, however, that in order to predict MeHg concentrations at high sulfide sites, both models required empirically-fit routines for either Hg speciation and/or methylation routines, suggesting that current understanding of Hg complexation chemistry is insufficient to model from first principles. It was asserted that empirical fits to existing data can be used to apply these models now to higher sulfide sites, however, more basic information on Hg-S and Hg-OM complexation under anaerobic conditions will be needed to produce first-principles models that can be broadly applied to areas for which little field information is available.

On Page 39 in the body of the report the authors elaborated on these limitations by noting that their existing model is being force fit with a mechanistic understanding of actual complex formation. They noted further that these empirical work-arounds make it difficult to translate results from one site to another, or to model one location as its biogeochemistry changes through time because complexation coefficients would need to be recalibrated to each new set of conditions. Finally, the authors stated that they were missing important complexes in these models, and there are likely to be situations where the models will not function well.

In our opinion, these statements confirm that the goal of creating models that can reproduce MeHg concentrations across the large sulfate and sulfide gradients found in the Everglades has not yet been achieved.

The final paragraph in the summary contains several significant claims that warrant close scrutiny and critical comment.

First, it was asserted that "a diagenetic model that relates Hg methylation to microbial sulfate reduction rate was able to accurately reproduce MeHg concentrations at a number of chemically different sites in the EPA"

See our comments above on the asserted accuracy of outputs from the diagenetic model at site 3A15.

Second, it was asserted that "The fit of the (diagenetic) model to the time series of declining sulfate and MeHg at site 3A15 supports the hypothesis that sulfate is a primary driver of methylation at that site, and that reduction in Everglades surface water sulfate concentrations will help mitigate MeHg production across the EPA."

Third, it was asserted that "These (diagenetic and E-MCM) models could now be applied to predict the distribution of MeHg across the EPA under changing sulfate loading scenarios."

Again, see our comments above on the outputs from the diagenetic model in terms of relating sulfate declines to observed decline in MeHg at site 3A15.

In our opinion, the results in this report provide no basis whatsoever for the claims that the models could now be applied to predict the distribution of MeHg across the EPA under changing sulfate loading scenarios, or that reduction in Everglades surface water sulfate will help mitigate MeHg production across the EPA. Furthermore, unless sulfate concentrations in the Everglades are quantitatively linked back to their primary sources, it will not possible to use the models to investigate the potential impacts of sulfate management practices on MeHg production and bioaccumulation in the EPA. Additional research and development are needed to incorporate the complete sulfur cycle in the E-MCM, including sources, transport pathways, and fate.

In our opinion, the claim of being able to predict the distribution of MeHg across the EPA under changing sulfate loading scenarios is completely unwarranted in the face of statements by the authors that the empirical work-arounds required for sulfide make it difficult to translate results from one site to another, or to model one location as its biogeochemistry changes through time. The additional statements by the authors that the models are missing important sulfide complexes and there would be situations where the models will not function well also serve to undermine this claim.

In our opinion, results from the site-specific applications of the diagenetic and E-MCM models in this study show promise; however, overstated claims of the scientific credibility or applicability of these models in their present form could potentially jeopardize the overall modeling effort.

APPENDIX 3B-3 INTRODUCTION

Methylation appears strongly linked to sulfate reduction (Figure 2) but there also appears to be an optimum concentration above which high concentrations of sulfide can cause a decrease in bioavailable Hg and thus a decrease in methylation. It is not known how site-specific factors might influence this optimum concentration.

TASK 5. CONSTRUCTION OF A DIAGENETIC MODEL OF SULFATE REDUCTION AND METHYLMERCURY PRODUCTION IN THE EVERGLADES

Description of Sediment Hg Cycling Model

The sediment diagenetic model was developed for incorporation into the E-MCM to better account for the effect of sulfate reduction and sulfide presence on methylation of Hg in sediments. Diagenesis processes are critical to methylation because they determine the sediment redox conditions that control sulfate reduction. This approach makes sense in terms of updating and enhancing the capabilities of the E-MCM.

In our opinion, even though there still does not exist a complete understanding of the controlling biogeochemical processes, especially those involving sulfide, the science underlying the diagenetic model appears sound and reasonable. Although the model is still in an early phase of development it appears to be a promising first step towards incorporation of the influence of sulfate reduction and sulfide on methylation of Hg in Everglades sediments.

The model is a reasonable representation of the major diagenetic processes in the vertical dimension including advective transport, diffusion and particle mixing; major microbial terminal electron accepting processes (i.e., oxygen, iron, nitrate and sulfate respiration); equilibrium speciation of dissolved ferrous Fe, Hg, dissolved inorganic carbon (DIC), sulfide and Hg; kinetically-controlled sorption of ferrous Fe, inorganic Hg, and methylmercury (MeHg) to the sediment solid-phase; and kinetically-controlled precipitation of FeS and HgS. Again, it should be noted that there still does not exist a complete understanding of the controlling biogeochemical processes, especially those involving sulfide.

Application of Diagenetic Model to Everglades Sediments

The diagenetic model was first tested by attempting to reproduce observed vertical profiles of porewater sulfate and sulfide. It was found that simple diffusive transport could not account for the required input of sulfate to the sediment. The authors concluded that additional transport processes must have been active in order to provide the required supply of sulfate. To correct this problem, the effective rate of diffusive flux was increased to represent the assumed influence of sediment macrofauna and methane gas bubble ebullition. This was implemented by an "irrigation coefficient" (D_{irr}) that was added to the sediment diffusion coefficient for each dissolved species in the model. The D_{irr} values required to reproduce observed sulfate and sulfide profiles were approximately 10 to 100 times higher than the average solute molecular diffusion coefficients.

This approach is not unreasonable and is consistent with the well-known phenomenon of bio-enhanced, sediment-water diffusion. It should be noted, however, that this irrigation coefficient represents transport processes that are not well-characterized or understood, and it could confound the predictive capability of the model if calibration values turn out to be highly site-specific and vary over a large range among different application sites.

The diagenetic model was applied in steady-state mode to several sites within WCA 2A (sites U3 and F1), STA 3/4 (Cells 1-3), and WCA 3A (site 3A15). In addition, the model was applied in time-variable mode to generate input values for E-MCM simulations of the response of site 3A15 to decreases in sulfate input over approximately the last 10 years (1995-2005).

Simulation Results

Steady-State Vertical Profiles

Results were described as "quite promising" for WCA 2A sites (Figure 5.4) for sulfate reduction rates, dissolved inorganic Hg concentrations, and total MeHg concentrations. Results for STA 3/4 (Figure 5.5) were described as "accurately reproducing" observations.

We disagree with the assertion that steady-state results for 3A15 (Figure 5.6) fit the observed data from 1996 and 1998 "quite well." It is important for there to be good correspondence between computed and observed values for aqueous-phase concentrations. The model results in Figure 5.6 do not appear to capture the trends in these concentrations with depth in the sediments.

Table 5.4 contains a list of key model parameters. It should be noted that surface water sulfate concentrations were specified directly from observations and were not quantitatively linked to mass loadings from external sources. This is not a limitation of the diagenetic model per se, but of the overall modeling study. Unless sulfate concentrations are quantitatively linked back to their primary sources, it will not be possible to use the models to investigate the potential impacts of sulfate management practices on MeHg production and bioaccumulation in the EPA.

A fundamental concern with the diagenetic model is that in all of the applications, adjustable variables were scaled as needed to fit the observed sulfate and dissolved Hg profiles. Specifically, the diagenetic model has four adjustable parameters, three of which were substantially altered across the four application sites. The four adjustable parameters are Dirr, surface water dissolved oxygen (DO) concentration, a scalar for calculation of Hg-S methylation (λ), and the aqueous/solid-phase partition coefficient for

MeHg (Kd). Across the four application sites, surface water DO was varied over two orders of magnitude, Dirr was varied over one order of magnitude, and λ was varied over a factor of three. Only Kd was held constant across the four sites. Results were not presented for any sensitivity analyses for these model parameters.

Our most serious concern is the adjustment of surface water DO from 0.001 (3A15) to 0.1 µmol/ml (U3) across the four sites. This is a red flag. There should be some explanation of why this was necessary, especially when these DO values are already very low. It should also be explained why DO is treated as an adjustable parameter and not specified directly from observations as was done for surface water sulfate concentrations.

This ad hoc approach to specification of surface water DO concentrations substantially weakens the model results and compromises the study goal of constructing a model that is capable of predicting the depth distribution of Hg methylation in Everglades soils across a range of surface water sulfate concentrations.

Time-Variable Depth-Averaged Profiles (WCA 3A-15 Sediments)

Results are shown for only depth-integrated sulfate reduction rates and Me-Hg concentrations over the 10-year simulation period for the 3A15 sediments (Figure 5.8). Figure 5.8A shows the changing sulfate concentrations over time that were used as inputs to the E-MCM and Figure 5.B shows the predictions from the diagenetic model in terms of depth-averaged fraction of total sediment Hg accounted for by MeHg.

Figure 5.8 is difficult to interpret because Panel A contains observed sulfate concentrations, the inset in Panel A contains predictions from the diagenetic model that were used as inputs to the E-MCM, and Panel B contains a different set of predictions from the diagenetic model that was used for comparison with observations.

It was asserted that although the diagenesis model predictions in Figure 5.8B do not precisely match the highly variable observed data, the basic trend of decreasing MeHg with decreasing sulfate abundance is clear. This qualified interpretation is at odds with the claim in the report Summary that "model outputs accurately predicted the concomitant declines in sulfate reduction rate and MeHg production at this site (3A15) over time, providing mechanistic support for the hypothesis that sulfate declines are driving at least part of the observed decline in MeHg at this site." In addition, if our understanding of the results in Figure 5.8 is correct, predicted sulfate concentration was compared with observations, not predicted sulfate reduction rate.

In our opinion, the accuracy of the diagenetic model results claimed in the report Summary is overstated and does not provide mechanistic support for the stated hypothesis. It is entirely possible that the observations are due to temporally coincident declines in Hg loadings and not to a cause-effect relationship based on sulfate reduction. Results from the diagenetic model might be more informative for this case if they were presented in terms of full vertical profiles instead of only depth-averaged values.

TASK 6. EXAMINING THE ROLE OF SULFUR ON METHYLATION: E-MCM SIMULATIONS AT WCA 3A-15

Everglades Mercury Cycling Model (E-MCM)

References to development of the E-MCM and its previous applications to WCA 3A-15 are presented in this section. It should be noted again that we have not reviewed the E-MCM itself or any of these previous applications.

Study Objectives and Approach

The study objective was to explore ways to improve the ability of the E-MCM to predict relationships between sulfate and MeHg production and bioaccumulation in the Everglades. The specific study objective was to test whether a more mechanistic treatment of sulfur cycling improves the predictive strength of the E-MCM, using output from the diagenetic model. The specific objectives pertaining to examination of sulfide-Hg complexation were not stated.

The overall modeling approach was to examine potential linkages between two aspects of sulfur cycling and methylation: (1) sulfate reduction; and, (2) sulfide effects on Hg(II) complexation.

The specific study objective is somewhat ambiguous as stated because the tests conducted with the E-MCM evaluated only the effects of externally specified sulfate reduction rates under the assumption that these rates control MeHg production in the sediments. The stated modeling approach is an accurate description of what was actually done and, in our opinion, is a more appropriate statement of the specific study objective.

It should be noted that there is no direct coupling between the diagenetic and E-MCM models. A linkage between the two models was used for testing the effects of sediment sulfate reduction rate on MeHg production in the E-MCM. The diagenetic model was used to predict a time series for sulfate reduction rate and this time series was then fed forward to the E-MCM.

The E-MCM was tested in this study using three different approaches:

Approach A: No linkage between the diagenetic model and the E-MCM. Methylation in the E-MCM is not a function of sulfate concentration.

Approach B: Sediment porewater sulfate concentrations in the E-MCM are specified using observed data. MeHg concentrations in the E-MCM in this approach are a function of sulfate concentration, overall decomposition rate, available Hg (total dissolved Hg), and a tunable methylation rate constant.

Approach C: Methylation in the E-MCM is related to the sulfate reduction rates predicted by the diagenetic model. MeHg concentrations in the E-MCM in this approach are a function of sulfate reduction rate, available Hg, and a tunable methylation rate constant.

A major concern is that the inputs to the E-MCM, especially the external Hg loadings, are either incompletely documented or not documented at all. Also, the "tunable methylation rate constant" used in Approaches B and C appears to be important but is not described, nor are its numerical values presented in the report.

WCA 3A-15 receives external Hg loads from watershed inflows and direct atmospheric Hg deposition. External loads for total Hg (HgT) and MeHg in the E-MCM were inferred from limited data (n=7, USGS data) at site 3A33 for 1996-99. These data were extrapolated backward and forward in time to provide the loading time series for the E-MCM test applications to the period 1995-2003.

Specification of external Hg loadings is a weak link in the overall modeling analysis. Only seven (7) observations were available over a nine-year application period, these observations were not from the E-MCM application site, and neither the data nor the actual Hg loading time series are presented in the report. Other time variable inputs to the E-MCM are presented, but either only in summarized form (Tables 6.1 and 6.2) or they are not adequately described. For example, atmospheric wet deposition is described as being based on data from MDN site FP11, but this description appears only in the discussion section of the report.

Because approximately 95 percent of the total Hg loading is from wet deposition, the uncertainty in Hg loads from watershed inflows may not be a significant factor. However, as noted above, the actual Hg loading from wet deposition used in the E-MCM is not documented in the report. There are plots of these Hg loading data elsewhere in the SFER 2008, but not in a form that would have been used for input to the E-MCM in these test applications.

This is an important omission because Hg loads from wet deposition are highly variable and appear to have increased substantially in some recent years (circa 2004) and now appear to have stabilized. In any case, uncertainty in the total input loadings for Hg from watershed and atmospheric sources confounds the interpretation of results from the E-MCM test applications and weakens the ability of the model to discriminate among different hypotheses.

Another concern is that determination of boundary conditions for the diagenetic model and the linkage with the E-MCM are not well documented. Specifically, it is not clear how water-sediment transport for dissolved phase species (e.g., sulfate or MeHg) is represented in the diagenetic model versus the E-MCM. Also, the E-MCM represents deposition of organic matter that can potentially drive sediment diagenesis and promote sulfate reducing bacteria. It was not described how these processes are represented in the two linked models.

Still another concern is the potential impact of nitrogen cycling in the sediments on redox and pH, and hence on sulfate reduction rate. It is not clear how either the diagenetic model or the E-MCM represents these processes.

For sulfide-Hg complexation, E-MCM simulations were first carried out for WCA 3A-15 porewater where sulfide levels are generally <1 uM. Sulfide concentrations were then specified in the E-MCM test applications at three levels that were considered environmentally relevant: 1 nM, 1 µM, and 1 mM.

Before reviewing the E-MCM modeling results, below are some of the major implicit assumptions in the use of the linked diagenetic and E-MCM models to simulate the effects of sulfur cycling in this study:

- The linkages of sulfate and sulfide effects on MeHg production were evaluated in the E-MCM without simulating the complete sulfur cycle. These effects were examined independently and only with respect to conditions in the sediments.

- Sulfate effects on MeHg production were simulated based on the simplifying assumptions that either the sulfate concentration or the sulfate reduction rate in the sediments controls MeHg production.

- Sulfide effects on MeHg production were simulated based on evaluation of the formation of Hg-S complexes at three constant levels of specified porewater sulfide concentrations (1nM, 1uM, 1mM). There was no evaluation of the potential influence of temporal variations in sediment sulfide concentrations.

- It was assumed that the calibrated, steady-state (10-year) parameterization of the diagenetic model at site 3A15 was technically sound.

- It was assumed that the time variable application of the diagenetic model at site 3A15 was technical sound and represented all important time varying inputs.

E-MCM Results

Effect of Linking Methylation to Sulfate

We concur with the assessment in the report of the comparisons between computed and observed HgT in surface waters in Figure 6.3. We do note that these comparisons, and the comparisons between computed and observed MeHg in Figure 6.4, would have been more informative if they had included observations for Hg in the sediments as well as in surface waters.

The results in Figures 6.4 to 6.6 show comparisons between computed and observed Hg concentrations in surface water and fish for the three above E-MCM test approaches. In our opinion, these results are not compelling evidence for the need to include sulfate dependence in the E-MCM computations for methylation. Inclusion of sulfate dependence does seem to qualitatively improve the results, but without quantitative confidence limits on the observed data, it is not possible to draw a definitive conclusion.

Another confounding factor is that computed and observed values for Hg concentrations in fish are not properly compared. Figures 6.5 and 6.6 show E-MCM results for the entire computed time series. For proper comparison of these results to observations for Hg in fish, computed and observed results should be paired up for the same times or at least for periods that reflect the actual fish collections. The comparisons in Figures 6.5 and 6.6 confound interpretation of results, especially for YOY, because there are large Hg concentration shifts between different age classes.

A serious omission is the lack of any results for the sensitivity of E-MCM outputs to changes in any of the key model parameters or external forcing functions. Without such results it is impossible to evaluate the relative importance of the various controlling environmental factors. Combined with the uncertainty in input loadings for Hg, the lack of sensitivity analyses confounds interpretation of results from these E-MCM test applications and substantially weakens the ability of the model to discriminate among different hypotheses for sulfate dependence.

Effects of Sulfide on Methylation

In general, this section of the report confirms that sulfide effects on MeHg production in sediments can not yet be modeled in a realistic, mechanistic manner. At the present time there is insufficient scientific understanding of the underlying biogeochemical processes.

Discussion

Sulfate Dependence

It was asserted that E-MCM results are generally consistent overall with the hypothesis that sulfate reduction affects methylation and subsequently MeHg concentrations in the Everglades marshes. We have no disagreement with this statement in a qualitative sense, but do note that much more could have been done with both the diagenetic model and the E-MCM in terms of sensitivity and diagnostic analyses to provide some quantitative support for this claim.

It was asserted that for MeHg in water or largemouth bass, E-MCM results were similar whether methylation was linked to surface water sulfate concentration or to predicted sulfate reduction rates in the sediments. We note some differences in the trends for these two options, especially in the fish, that could have been further investigated by more closely evaluating predictions in the sediments and by conducting sensitivity analyses.

It was asserted that E-MCM model results suggest that while reductions in sulfate contributed to declines in MeHg at WCA 3A-15, other factors also played a role. The other factors listed in the report include:

- Hg loading rates (there was slightly declining wet deposition at the MDN site FP11 used for this study, except for an increase during 2003).

- Declining DOC concentrations (this factor is mentioned, but does not appear to be discussed).

- Declining chloride concentrations (this decreases bioavailability of MeHg uptake by phytoplankton).

- Slightly increasing pH (mentioned, but not discussed).

- Legacy effects due to conditions prior to 1995 (not clear what was meant, but perhaps refers to uncertainty in sediment initial conditions).

We concur with the assessment that factors other than reductions in sulfate also contributed to declines in MeHg at WCA 3A-15.

In the end, the report concludes that further analysis is needed of the relative contributions of changes in sulfate, Hg loading, chloride, DOC and other factors both before 1995 and during the simulation period to the observed decline in fish mercury concentrations. We strongly concur with the need for further analysis of the relative contributions of these factors, but note again that much more could have been done with the model in terms of sensitivity and diagnostic analyses to investigate these factors.

Sulfide Dependence

We strongly concur with the statement that there remains much uncertainty about the quantitative effect of sulfide and its complexation and precipitation reactions on methylation.

The goal for this portion of the study was to develop a mechanistic model for Hg complexation and bioavailability under the sulfidic conditions found in the more highly sulfur-impacted areas of the EPA. The authors found that through large adjustments of the measured complexation coefficients for Hg with dissolved organic thiols in E-MCM, they were able to fit the model to observed Hg concentration data. They stated that this modeling, coupled with recent laboratory and field data on Hg-D-DOM interactions, suggests that the models being used do not capture one or more important Hg complexes.

The authors went on to state that while a Hg speciation model that includes both dissolved and solid phase organic matter and sulfides can be made to adequately predict dissolved Hg concentrations, their existing model is being force fit with a mechanistic understanding of actual complex formation. They noted, however, that these empirical work-arounds make it difficult to translate results from one site to another, or to model one location as its biogeochemistry changes through time because complexation coefficients would need to be recalibrated to each new set of conditions.

It was asserted that because significant field data are available to calibrate the E-MCM for the higher sulfide areas of the Everglades, an empirically-fit model may be adequate to model the impact of sulfide on MeHg production across the EPA. It was noted, however, that important complexes are missing in these models, and there are likely to be situations where the models will not function well.

In our opinion, the claim of being able to model the impact of sulfide on MeHg production across the EPA is completely unwarranted in the face of statements that the models are missing important complexes and there would be situations where the models will not function well. The statement by the authors that the empirical work-arounds required for sulfide make it difficult to translate results from one site to another, or to model one location as its biogeochemistry changes through time, also serves to undermine this claim.

In our opinion, results from the site-specific applications of the diagenetic and E-MCM models in this study show promise; however, overstated claims of the scientific credibility or applicability of these models in their present form could potentially jeopardize the overall modeling effort.

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Ernie Marks	**Subject: Appendix 7A-2 Comments**
Total Messages 4	These comments were submitted after Department staff review of the draft 2008 SFER Appendix 7A-2: Picayune Strand Restoration Baseline Report. 1. Please include preliminary conclusions on success of efforts to date.
	Posted: 19 Sep 2007 04:22 PM Originally Posted: 19 Sep 2007 04:20 PM

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Ernie Marks	**Subject: Appendix 7A-3 Comments**
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The following comments are offered based on Department staff review of the draft 2008 SFER Appendix 7A-3: Permitted Conditions Annual Reports for A8 Projects.

1. Reporting section does not mention Acme. C-44 TIWCD add "reconfiguration."

2. Table 1 – Please clarify. Ten Mile Creek is not an Acceler8 project.

3. Table 1- Please include correct permit numbers (EAA = 0242172-003-EM; TIWCD = 0254895-003-EM, etc.).

4. PSRP: Verify May 4 date. Volume 1, 7A has February for initial road work completion and construction meetings have June 2007 for substantial completion date of the larger road removal. Please correct. Revised statement that "this work has been completed as part of the removal of 65 miles" to "this work was completed in conjunction with" since there were 2 contracts. DEP needs to receive close-out documents for permitted activities on PSRP.

5. EAA Reservoir: Suggest deleting "to date" since this report period is through 9/07. Provide updated completion information and verify %complete (35% is low based on numbers in report – construction meetings have 66%). For "problems encountered/actions" there should be something brief on the blasting (i.e. coordination with FDOT on shutting down US 27 and adjustments to ensure all canisters ignite).

6. C-44: Add "Temporary Reconfiguration". The statement that the project was built according to the permitted plans with no deviations is not entirely correct. Original plans had the Minute Maid bridge to remain, but it was replaced with culverts when bridge issues arose during testing phase. This should be captured as a "problem encountered". Also, the Running W canal needed to be extended based on the results of the testing (maybe this is a "conclusion regarding success" item).

7. Note that culverts on Janes were shortened to avoid impacts so there were changes to permitted plans. Need to receive close-out documents. For the "conclusion regarding success", it would seem that a sentence or two on preliminary results would be appropriate. Was there was in issue with burning during the dry season?

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Annet Forkink	**Subject:** Appendix 7A-4 Comments
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The South Florida Water Management District Resources Assessment Department prepared a Draft Annual Water Quality Assessment Report for the L-8 Reservoir Project to evaluate the results of the Testing Project monitoring program. The report covers the period from May 1, 2006 through April 30, 2007.

Review Comments:

1) Executive Summary, second paragraph:
To clarify permit conditions, please change the following sentence as follows: " The discharge ~~only~~ water from the L-8 reservoir Project ~~that~~ is required to meet Class III water quality criteria at ~~is~~ the discharge location, and it shall ~~can~~ not adversely impact water quality in the M canal which is classified as Class I water."

2) Page 2, second paragraph:
It is stated that the criteria limit for copper cannot be calculated because hardness data were not collected. Why were copper data collected without also collecting the hardness necessary to calculate compliance? It is agreed that there is most likely not an issue with copper within the pits or downstream in the L-8 canal. Given the high conductivity in the pits and canal the hardness is probably high enough to prevent an exceedance of the copper criteria. In the future, all necessary ancillary information needs to be included in order to determine compliance.

3) Page 9, third paragraph:
Please explain the abbreviation Qftg.

4) Page 15, Table 2:
Please include Mercury criteria as mentioned in the text.

5) Data Summary Tables:
Please provide summary tables for pesticides and sulfate as well

an evaluation/ summary of the data.

6) Summary Figure:
Please provide a summary figure for flow and stages.

7) Page 21:
The report notes that Gross Alpha was elevated at the L8 canal inflow site, just after the August 2006 outflow event. The District and the Department should continue to work together to evaluate the conclusions as discussed in the report.

8) Page 30, first paragraph:
Please replace the word "Bass" with "game fish".

9) Conclusions:
High chlorides and conductivity levels that occurred during the discharge event are not mentioned. The extent of this draw down, the influence of groundwater seepage and the effect on water quality deserves some discussions.

10) General comments:
A. The report covers the period from May 2006 through April 2007. During the period of this report (May 2006 through April 2007) there were no inflow events and only two outflow events. Only the August 2006 outflow event extended beyond a one-day discharge period. It needs to be considered that there was only one major outflow, as it appears that this discharge event elevated Gross Alpha, Chlorides and Conductivity in the L-8 Canal (at the inflow monitoring station). The report does not discuss this. Furthermore, the project has not yet captured surface waters from the L-8 Canal. Since this is the ultimate goal of this project, it should be explained that the water quality data collected may not be indicative of future operation. The section that discusses the operation should provide a discussion about the actual operation that took place. These items should also be included in the results section and the conclusions.

B. The District may want to consider not to include all custody records and field notes in appendix C, but to include a "available upon request coversheet", to help reduce the number of pages and size of this document.

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