APPENDIX 1
Public and Peer Review Panel Comments

Appendix 1-2
Comments from the Public and Consultants

Appendix 1-2b
Comments from the United States Department of the Interior

United States Department of the Interior

NATIONAL PARK SERVICE
FISH AND WILDLIFE SERVICE
Everglades Program Team
A.R.M. Loxahatchee National Wildlife Refuge
10216 Lee Rd.
Boynton Beach, FL 33437-9741

October 16, 2000

Dr. Jeffrey L. Jordan, Professor and Panel Chair
2001 Everglades Consolidated Report Peer Review Panel
Dept. of Agricultural and Applied Economics
University of Georgia
Griffin, GA

Dear Dr. Jordan:

Please find enclosed a series of reviews of the 2001 Everglades Consolidated Report. These reviews were conducted by the staff of the Everglades Program Team (EPT) representing the U.S. Dept. of Interior. The EPT staff consists of Dr. Nicholas G. Aumen (ecologist, NPS), Dr. Lorraine Heisler (ecologist, FWS), and Dr. Michael Waldon (hydrologist, FWS). Dr. Laura Brandt (EPT liaison from the A.R.M. Loxahatchee National Wildlife Refuge) also contributed comments. We divided chapter responsibilities among us, according to our expertise, and the reviews were written separately. However, there may be some redundancy where more than one reviewer commented on the same chapter. Rather than trying to mesh all comments into one document, I preserved each set of comments as unique contributions.

In general, we were impressed with the level of quality of this year's report, and the presentations made at the public workshop in early October. It was clear to us that all of the authors worked hard on their respective chapters, and we commend the SFWMD, FDEP, and the other agencies and entities involved for developing such a comprehensive report.

In addition to providing you with each set of comments, I have attempted below to highlight our major comments within this cover letter, for the panel's information:

1. Regarding DUWC's report and presentation of an ecological basis for a P threshold, we strongly believe that the use of the CART statistical analysis procedure is inappropriate for ensuring protection of downstream resources. We have no argument with the technical process of performing the CART analyses, or the data underlying the analyses. However, we do believe that a P threshold should be determined based on the first indication of an ecological impact – not

the “break-point” of ecological response that CART employs, which is associated more with the mid-points of impacts, rather than first indications.

2. Chapter 2 presents new experimental results on cattail and sawgrass responses to hydrology, nutrients, and fire. It would be very helpful if this section were better integrated with prior work, and provided a synthetic summary of the state of this knowledge in relation to choices about how to manage nutrients and hydrology in the Everglades.
3. Chapter 2 presents the conjecture that increased velocity is needed to maintain the island, ridge, and slough topography. Verification of this conjecture could significantly affect plans for restoration. Although this conjecture is plausible, existing evidence for this hypothesis needs to be more fully developed and documented here or in a future report. Literature on effects of structures on sedimentation and erosion and on accepted techniques for analysis of these phenomena should be reviewed. Study plans should also be documented.
4. Data generated during monitoring the operation of STA-1W are of great value, but do not provide unequivocal information on STA treatment limitations using current designs. It is premature to identify a minimum limit of reliable annual TP removal or effluent concentration for the STAs. It is also premature to conclude that the presence of SAVs in cell 4 is the cause of the improved treatment efficiency of the STA-1W western treatment cells. This difference in apparent efficiency might result from TP inflow and loading of cell 3 from groundwater seepage, increased depth and hydraulic retention time, or other factors unique to the STA-1W installation.
5. Chapter 3 presents a discussion of data variability, which suggests that the Duke University Wetland Center dosing results are too variable, in comparison to data from the WCA-2A transects, to provide a firm basis for defining a numerical phosphorus criterion. While this conclusion may be correct, the argument and illustrations used to support it need to be developed more rigorously.
6. The forested wetlands and pondapple swamps that are proposed by Tetra Tech scientists as crucial habitat areas for restoration may already exist on tree islands in the remnant Everglades. The spatial extent of tree island forests, which regularly include pond apple swamps, is expected to increase under current restoration plans. Creation of additional pondapple or swamp forests in addition to tree island restoration does not appear to be ecologically justified at present.
7. It is the DOI position that any inflows to the EPA must comply with the numeric criterion for Class III waters. If it has been determined that 10 ppb TP is protective of the resource, then inflows to the EPA should meet this standard. It is unacceptable to allow “…a small zone on the marsh periphery where the phosphorus levels are slightly enriched…” as suggested on page 3-44.
8. In Chapter 8, it is apparent that the SFWMD is proceeding with CTSS research at a large scale and high expense, despite overwhelming consensus that such a technology employed at the full scale would not be acceptable to any of the agencies or entities involved in Everglades restoration. We believe that the initial phases of the research have been high quality and informative, and have produced sufficient information to make initial determinations of the lack of feasibility. We believe the technology has several fatal flaws if employed at the large-scale level, including: potential increases in Hg methylation rates due to sulfate enrichment;

concerns regarding effluent marsh readiness; and the economic and ecological impacts associated with chemical transport, and sludge transport and disposal. CTSS research at the present scale diverts critical resources away from expanded research on other, more acceptable passive technologies. This finding is supported by the efforts of the Florida Department of Environmental Protection and the National Park Service to find supplemental funds to augment critical passive technology research efforts that the SFWMD has not funded.

9. We believe that sufficient levels of cost estimates are available, particularly for CTSS, for preliminary presentation and assessment as part of the Consolidated Report. It is recognized that these estimates would be refined as new research and engineering information becomes available, but we believe strongly that initial estimates should be provided for public review. On pages 8-39 through 40, it is indicated that Phase 3 of the STSOC, "Development of cost estimates," is completed, and we believe this information, even in draft form, should be included at this time.
10. Chapter 10 provides a lucid summary of the CERP RECOVER process, which aims to insure regional integration of all future restoration projects. However, there is a need for a process to insure that the future hydrologic conditions and restoration goals envisioned by the CERP are incorporated, to the maximum extent possible, into ongoing non-CERP projects such as the Everglades Construction Project.

Sincerely,
The Everglades Program Team

Chapter 2 – Reviewer: Lorraine Heisler

p. 2-26 through 2-34 -- "Vegetation Responses to Hydrology"

This section describes new experimental results bearing on the mechanisms by which muck fire, water depths, and nutrients affect growth and survival in cattails and sawgrass. There are several intriguing new observations that will help in developing a more robust and predictive understanding of the causes for cattail proliferation. However, this section needs to integrate the new results with prior years' work and to provide an update on the implications for management of cattails in the Everglades. Decisions about the operation of the STAs and the implementation of hydropattern restoration are underway. As a technical decision-support document for the coming year, it would be extremely helpful for the Everglades Consolidated Report to include a conceptual framework for weighing the relative importance of water depth, soil composition, water quality, fire history, and species interactions in determining the fate of cattails. The District's Everglades research group clearly has the combined expertise and experience to undertake such a synthesis. I would encourage them to develop a conceptual model for cattail dynamics, if not for inclusion in this year's report, as a task for the coming year.

p. 2-26 -- The study of cattail growth in muck-burned and surface-burned soils from Rotenberger Wildlife Management Area supports the hypothesis that soil mineralization resulting from muck fires is a significant causal factor in allowing cattails to expand into some areas. However, since a full research report for this study is not available at present, this section needs to provide more experimental details in order to be convincing. For example, there is no mention of the degree of replication in the experiment (number of soil samples per treatment; number of cattail seedlings) or of what kind of water was used. This information could be incorporated into the Figure 2-11 caption, and thus be available to interested readers without adding too much technical detail to the text.

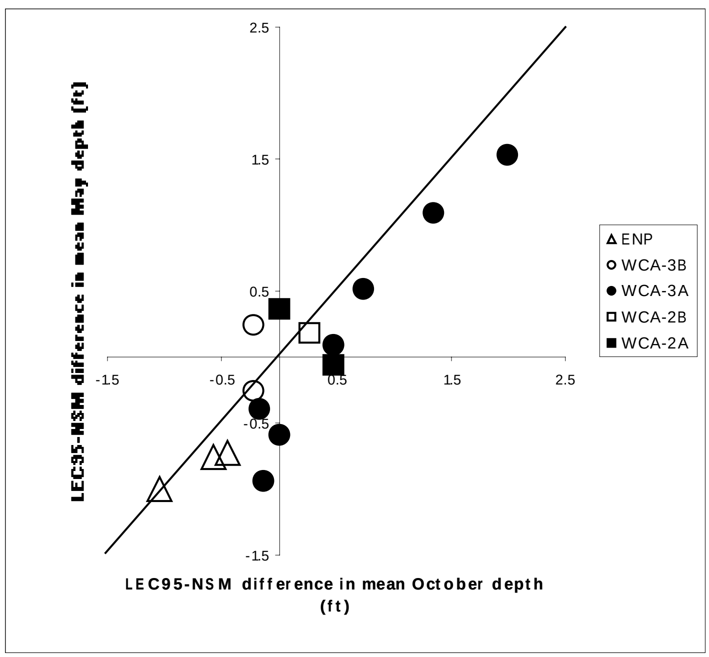
Pp. 2-26 & 2-61 -- Cattails could be absent from the pre-drainage Everglades for reasons other than a lack of muck-fires (e.g., because of range increases owing to dispersal from adjacent disturbed areas). Although an absence of cattail remains is *consistent* with an historic absence of muck fires, I do not think it provides very strong *support* for this hypothesis.

Pp 2-27 to 2-29 – I do not see "genetic concerns" about cattail adaptation as a key area for future research, especially when compared with other research priorities. The hypothesis that *Typha* is subject to natural selection seems to be based on three observations: (1) cattails are spreading into an environment that may differ from those they occupied in the past; (2) the species has very high seed production; and (3) the species has high seed mortality. However, fecundity and juvenile mortality rates do not in themselves provide any indication of the strength or outcome of natural selection. These traits characterize the life histories of many plant and animal species. Whether or not adaptive evolution occurs depends on the genetic covariance between overall fitness and life history traits (Lande 1982). If most germination failure is non-selective with respect to variability in seed traits, the opportunity for selection of specific genotypes may be no greater, or may in fact be less, than that for other species such as *Cladium* that have much lower fecundities. Furthermore, if most cattail reproduction is clonal, the overall seed pool in any location may be relatively low in genetic variation. Certainly it is the case that changes in hydrology can exert directional selective pressure on plant traits; however, native Everglades macrophytes such as sawgrass are also subject to these pressures. Which species will prove more capable of adapting to novel hydrologic conditions will depend more on the underlying capacity for genetic change than on phenotypic differences in life history. I do not mean to argue that natural selection is not occurring in cattails; it may well be. However, the life history characteristics of this species seem to me to make its ecological interactions, and not its genetics, a more important research priority.

2-30 **–** The results on vegetation responses to hydrologic factors suggest that a useful future research area would be the effect of variation in depth on survival and growth of cattails and sawgrass. The seasonal change in depth between wet and dry seasons appears to have increased under water management in the WCAs, owing to the combined effects of dry season withdrawals for water supply and wet season pumping for flood control. This hypothesis is supported by comparison of the South Florida Water Management Model and the Natural Systems Model. If one calculates, as a measure of seasonal depth change, the difference between the SFWMM and NSM in simulated mean October and May depths for model “indicator regions” in the ridge and slough landscape, the average seasonal amplitude in the managed system exceeds that of the pre-drainage system throughout WCA-3A, in southern WCA-2A, and in downstream sections of Shark River Slough. Specific values for ridge and slough “indicator regions” are listed in the table below, along with a graph illustrating the differences between simulated pre-drainage and current depths in May and October. For the majority of indicator regions, the points on this graph fall below the line of unit slope, indicating that in areas that have become wetter under the managed system, wet season depths have increased more than have dry season depths, while in areas that have become drier, dry season depths have declined more than have wet season depths. As one might expect, this increase in seasonal amplitude is most pronounced in WCA-3A and WCA-2A, which are managed for both flood control and water supply.

Simulated Differences Between Pre-Drainage (NSM) and Current (SFWMM LEC 1995 Base) Ridge and Slough Landscape in Wet/Dry Season Depths and Amplitudes

		May mean depth (ft)		Difference in May mean (ft)	October mean depth (ft)		Difference in Oct. mean (ft)	Seasonal Amplitude (Oct - May) (ft)		Amplitude change (ft)	Amplitude change (% over NSM)
Area	Indicator Region	NSM	LEC95	LEC-NSM	NSM	LEC95	LEC-NSM	NSM	LEC95	LEC-NSM	LEC-NSM
ENP	9	0.7	-0.1	-0.7	1.7	1.3	-0.5	1.1	1.3	0.3	27
	10	0.9	0.1	-0.8	2.1	1.5	-0.6	1.2	1.4	0.2	13
	11	1.0	0.0	-1.0	2.2	1.2	-1.0	1.2	1.2	0.0	-3
WCA-3B	15	0.3	0.5	0.3	1.7	1.4	-0.2	1.4	0.9	-0.5	-34
	16	0.5	0.2	-0.3	1.9	1.6	-0.2	1.4	1.4	0.0	1
WCA-3A	14	0.1	1.2	1.1	1.3	2.6	1.3	1.2	1.5	0.3	20
	17	-0.2	0.4	0.5	1.1	1.8	0.7	1.3	1.5	0.2	17
	18	-0.1	0.0	0.1	1.0	1.5	0.5	1.1	1.5	0.4	32
	19	-0.2	1.4	1.5	0.9	2.9	2.0	1.1	1.6	0.5	42
	20	0.0	-0.6	-0.6	1.1	1.1	0.0	1.1	1.7	0.6	53
	21	-0.4	-0.8	-0.4	1.0	0.8	-0.2	1.4	1.6	0.2	15
	22	0.0	-0.9	-0.9	1.2	1.0	-0.1	1.1	1.9	0.8	72
WCA-2B	23	0.2	0.4	0.2	1.5	1.8	0.3	1.3	1.4	0.1	7
WCA-2A	24	0.1	0.1	-0.1	1.3	1.8	0.5	1.2	1.7	0.5	46
	25	-0.2	0.2	0.4	0.8	0.8	0.0	1.0	0.7	-0.4	-36



Differences between simulated pre-drainage and current mean depths for wet (October) and dry (May) seasons. The graph plots mean depths for each of 15 indicator regions in the ridge and slough landscape, with values listed in the table. Values were taken from the “Seasonal Amplitude” and “Seasonal and Inter-Annual Variability” summary tables posted on the hydrologic performance measures website for the Modified Water Deliveries Project (www.sfwmd.gov/org/pld/hsm/reg_app/mwd/).

Although these model comparisons are only estimates of the actual historic changes in seasonal variability, they suggest an overall shift toward more extreme seasonal depth variation as a result of water management. The ecological significance of this change is that a widespread increase in seasonal variability would be expected to favor, in *all* wetland vegetation communities, species with broader tolerances for depth variation. Loveless (1959) suggested this forty years ago as an explanation for the decline of beakrushes and the concurrent increase of maidencane in the wet prairies of the central Everglades. A similar effect might favor the present proliferation of cattails over sawgrass, if seeds of both species germinate under similar depth conditions, but cattail seedlings are more tolerant of rapidly rising water. Such effects could be an important consideration in identifying appropriate hydropatterns to support native Everglades vegetation.

Pp. 2-35 to 2-41 – "Tree Islands"

p. 2-35 – The issue of tree island soil subsidence is not well understood. There are two complicating issues that make it difficult to generalize about how tree island elevation has changed relative to rest of the landscape. First, the loss of tree island soil has to be considered in relation to the simultaneous loss of soil in the ridges and sloughs. Peat-consuming fires on tree islands have generally occurred in conjunction with muck fires in the surrounding marshes. Whether or not the islands or the marshes have lost more soil to historic fires has not, to my knowledge, been resolved. Because the island surface is more elevated with respect to groundwater, one might expect peat fires on tree islands, once started, to potentially persist longer. However, since tree island vegetation is not as flammable as sawgrass, the frequency of such fires may well have been lower. Hence, the change in island elevations relative to the surrounding marsh could be either positive or negative. Second, tree island soil loss would be expected to vary from place to place depending on the degree of historic drainage and fire. For example, the highest islands in northern WCA-3A have been severely altered by peat fires, with almost complete loss of their near-tail and sawgrass tail regions (Heisler et al., in review). Islands in the wetter, southern parts of WCA-3A do not show such dramatic alterations in shape and spatial extent. Oddly enough, the maximum elevation of these islands relative to the surrounding sloughs is similar in both northern and southern WCA-3A. I do not think we are yet able to generalize, with confidence, about the magnitude or spatial extent of historic changes in tree island elevation relative to the ridges and sloughs of the Everglades landscape. Hence, I very much agree with the statement, "supplemental indicators are needed" for evaluating appropriate water levels for the overall Everglades landscape. However, the statement that "tree island health may be as much an indicator of tree island subsidence as it is of appropriate water levels" seems to invert cause and effect. Tree island subsidence has itself been caused by inappropriately low water levels that promote intense wildfires. Thus tree island "health," including impacts from soil loss, may well be the most sensitive indicator of appropriate depth extremes at both ends of the hydrologic range.

p. 2-35 (top) and 2-40 (bottom) – The finding that tree island health is more connected with relative than with absolute water elevations seems rather obvious. I don't think a detailed argument is needed to support this point. An exception to this, of course, is the southeastern saline Everglades, where tree island vegetation is strongly influenced by proximity to salt water (Armentano, Jones, and Ross, in press).

P. 2-36, Figure 2-16 – What are the blue dots on the map in this figure?

p. 2-41, Figure 2-19 – How may trees were used to compute the means? This information could be included in the figure caption or legend.

p. 2-40 & Figure 2-19 – A point developed here is that vegetation composition, "health," and hydrology differ between the "head" communities of the two study islands but not between the "tail" communities. While the point is clearly made that the head community on island 3AS4 shows signs of stress associated with longer periods of inundation, the discussion of the near-tail communities seems to give the impression that these communities are not affected by lengthened hydroperiods. However, since the near-tail community on 3AS4 is situated approximately 1.5 ft higher in elevation than its counterpart on 3BS1, it is likely that there has been an historical shift in the location of the near-tail communities on one or both of these islands. I believe it would be helpful to include a brief discussion of whether or not the pre-drainage depths around these islands were similar, and how this might relate to the current elevations at which head and near-tail communities occur.

p. 2-55, Figure 2-26 – Both the ELM and the SFWMM appear to under-estimate groundwater recession depths at the NP-206 site in ENP. This could have consequences on ecological evaluations, since minimum groundwater depths have been used as a performance criterion in evaluating hydrologic model output for the southern Everglades.

Literature Cited

Armentano, T, D. Jones, and M. Ross. (in press) Tree islands of the southern Everglades. In A. van der Valk and F. Sklar, eds. *Tree Islands of the Everglades.* Kluwer Academic Press.

Heisler, I. L., D. T. Towles, L. A. Brandt, and R. T. Pace. (in press) Tree island vegetation and water management in the central Everglades. In A. van der Valk and F. Sklar, eds. *Tree Islands of the Everglades.* Kluwer Academic Press.

Lande, R. 1982. A quantitative genetic theory of life history evolution. *Ecology* 63(3):607-615.

Loveless, C. M. 1959. A study of vegetation of the Florida Everglades. *Ecology* 40(1):1-9.

Chapter 2 – Reviewer: Michael Waldon

Mass Balance

Calculations presented in Table 6-1 provide a valuable summary of STA operational experience. It is suggested that the table be split into separate tables for the water budget and phosphorus budgets. It is strongly suggested that, in addition to the phosphorus budget, a mass balance should be presented for one or more conservative constituents. Chloride, TDS, and conductivity as a surrogate for TDS concentration are often selected for this purpose. This is an easy calculation which, assuming the mass budget is reasonably balanced, will greatly increase the credibility of your findings. If data are available, budgets might also be presented for sulfate, sodium, hardness, and total nitrogen. At a minimum, a budget for one conservative should be added to this and all future reports.

Groundwater loading

A research highlight (page 6-2, Nungesser et al. 2000) states

> "The eastern and western flow-ways differ significantly in total phosphorus (TP) retention even when accounting for differences in TP loading. ... Treatment Cells 3 and 4 are different. These results may be due to differences in vegetation, hydraulic retention time, water depths, and/or seepage inflow into the eastern flow-way."

In the 2000 report (Chimney et al. 2000), this difference is attributed to a difference in the limiting treatment concentration for TP between treatment cells in the eastern and western flow ways. An understanding of the factors controlling the limiting treatment concentration, c^*, is of crucial importance in management of STAs, future wetland STA design, and technology selection for future STAs (page 6-42, Chimney et al. 2000). Chimney *et al*. estimate a value of 24.2 µg/L for the eastern flow-way, and <0.1 µg/L for the western flow-way.

Here it is suggested that this discrepancy results from an underestimation of TP concentration associated with groundwater loading into the final stage of the eastern flow-way, cell 3. The authors in both the 2000 and 2001 draft reports apparently use TP concentration data from shallow (10-20 m) wells to estimate concentration of water seepage from the region of the L-7 Canal (page 6-32, Chimney et al. 2000). Using well water quality values greatly underestimates the effect of seepage into surface water. Because the simple mass balance model applied here does not include a benthic component, groundwater concentration must reflect the concentration as the seepage enters the water column. Seepage into a water body enters as a convective pore water flow out of the sediments. Concentration of groundwater seepage should therefore be reflective of sediment pore water concentrations, not deeper groundwater.

Seepage water concentrations of roughly 30 µg/L were used by Chimney *et al*. This value can be calculated from values presented in Tables 6-5 and 6-6:

Surficial seepage		
area loading	0.02	g/m^2-y Table 6-6
	0.0000548	g/m^2-d
flow	0.0017 m/day	Table 6-5
concentration		
	0.032 g/m^3=mg/L	
	32	µg/L
Groundwater seepage		
area loading	0.04	g/m^2-y Table 6-6
	0.0001095	g/m^2-d
flow	0.0032 m/day	Table 6-5
concentration		
	0.034 g/m^3=mg/L	
	34	µg/L

Similar seepage loadings were used by Nungesser *et al*. in Table 6-1 of the 2001 report.

Seepage concentration needed to account for the anomalous c* value estimated in cell 3 can be roughly calculated from reported flows. Assume a purely conservative mixing occurs between the treated water entering cell 3 and the seepage water. Then:

$$c_{est}^{\,*} = \frac{c^* q_i + c_s q_s}{q_i + q_s}$$

where c^* is the actual limiting TP concentration,
q_i is the flow entering cell 3 other than seepage
q_s is the seepage flow entering cell 3
and c^*_{est} is the limiting value calculated in the report for cell 3.

Following the estimate for cell 4, and consistent with the assumption of Kadlec and Knight (page 463, Kadlec and Knight 1996), take c* to be zero. Solving for c_s then provides an estimate of the seepage concentration required to produce the observed results:

$$c_s = \frac{q_s}{q_s + q_i} c_{est}^{\,*}$$

These calculations are performed below using values from (Chimney et al. 2000):

Cell 3

Total inflow	5.23	cm/day	Table 6-5
Total L-7 seepage	0.49	cm/day	Table 6-5
Limiting conc. c_{est}^{*}	24.2	μg/L	page 6-42
c_s seepage	258	μg/L	

Comparing this calculated value, 0.258 ppm, to sediment concentrations (Figure 6-39, Chimney et al. 2000) which are in the hundreds of ppm suggests that this concentration is not unreasonably high for sediment pore water TP. Pore water concentrations data were not reported, but may be available for comparison. It is also noted that the reported lower TP level in cell 3 sediments relative to cell 4 is consistent with the hypothesis that seepage is convecting sediment TP into the overlying water of cell 3.

If the conjecture suggested here is correct, seepage control in cell 3, as well as in future designs, may be needed. Such control could involve installation of a physical barrier to groundwater flow. Alternatively, one or more wells could be installed and operated to reduce the local water table (piezometric) surface. Produced water could be discharged, returned over the L-7, or routed to the treatment headworks.

Suggested changes: At a minimum, it is recommended that a more realistic pore water TP concentration be used in Table 6-1. If, as an example, a ten-fold increase were applied, only the inflow TP load for cell 3 would be affected. This would result in the following revision in Table 6-1:

	Inflow TP Load		
TP Sources	kg	g/m2/yr	%
Treatment Cell 3 - 05/01/95 to 04/30/2000			
G253	11,709	0.72	55.0%
Rainfall	389	0.02	1.8%
Surficial Seepage	3080	0.20	14.5%
Groundwater Seepage	5890	0.40	27.7%
Dry Deposition	209	0.01	1.0%
G251-G256			
Seepage			
TOTALS	21,277	1.35	100.0%

Total phosphorus retained

Comments and questions:

In general, the methods used to develop Table 6-3 are inadequately documented.

The report uses the terminology “three-month rolling average” on page 6-10, and “3-month moving unit-area values” in Table 6-3. These terms should be defined, and any differences explained.

Why were averages used in the statistical analysis? The moving average causes the monthly terms to be statistically dependent. Moving averages which “look ahead” can cause an apparent loss of causality, that is, effects can appear before causes.

Should the degrees of freedom in the analysis of covariance be reduced to account for the use of averaging?

Why is N=46 rather than 48 (number of months in period from May 1995 to April 1999)? Is this caused by the use of 3-month averages?

Was the change in TP storage in the water column over the calculation interval included in the calculation of retained TP? If not, is this significant?

<u>ENRP Test Cell Research</u>

The sentence (page 6-13):

> “We designed experiments to determine boundary conditions and system response of experimental cells at the north and south sites.”

is unclear. What are the boundary conditions?

The sentence (page 6-13):

> “Concurrently, the HLR in the remaining north and south test cells are being incrementally increased by 50 percent, decreasing hydraulic residence time, every 15 weeks to approximately 20 cm/d (high HLR experiments) (Table 6-4).”
> is awkward and might be rewritten.

Table 6-5 should also report TN. This will facilitate comparison with TP values.

Lumping the 3 lowest HLR values (0.27, 0.72, and 1.27) under the term LOW, and the 3 highest HLR values (4.92, 10.72, and 19.04) under the term HIGH, obscures patterns in the experimental results. It is suggested that HLR be added as a row for each Exp in Table 6-5. Figures 6-4, 6-5, 6-6, and 6-7 should plot concentration, percent, or mass versus 1/HLR. The choice of the reciprocal of HLR allows the inflow to be plotted at the origin because HLR for the inflow concentration is effectively infinite (i.e. the outflow concentration will approach the inflow concentration as HLR becomes large). This plot is also suggested by Equation 9-153 in Kadlec and Knight (1996). Distinctive lines and plotting symbols can be used to distinguish the chemical constituents in these plots.

Figure numbers on page 6-20 may be wrong.

Table 6-7 and the related discussion are provided to demonstrate the consistency and credibility of the data used to estimate HLR. Similarly, a mass balance should be routinely performed on the test cell data for one or more conservative constituents. Chloride, TDS, and conductivity as a surrogate for TDS concentration are often selected for this purpose. This is an easy calculation which, assuming the mass budget is reasonably balanced, will greatly increase the credibility of your findings.

The first sentence on page 6-24 is unclear:

> "Higher inflow loading rates resulted in more phosphorus mass exported from the test cells than from wetlands with lower loading rates."

Does this refer to mass or hydraulic loading? What wetlands are being compared, test cells, STA cells, or natural wetlands in general?

Reference to Figure 6-6 on in the first paragraph of page 6-24 is in error.

The term "operational response" in paragraph 3 of page 6-24 is vague and not defined. The specific meaning implied here should be defined.

The caption of Figure 6-9 refers to inflow concentration but the axis label is TP outflow concentration. Which is correct? Why is a similar plot for south test cells not presented?

Literature Cited

Chimney, M. J., Nungesser, M. K., Pietro, K., German, G., Lynch, T., Goforth, G., and Moustafa, M. Z. (2000). Chapter 6: Stormwater Treatment Areas - Status of Research and Monitoring to Optimize Effectiveness of Nutrient Removal and Annual Report on Operational Compliance. 2000 Everglades Consolidated Report, G. Redfield, ed., South Florida Water Management District, West Palm Beach, Florida.

Kadlec, R. H., and Knight, R. L. (1996). *Treatment Wetlands*, Lewis Publishers, January 1996, Boca Raton.

Nungesser, M. K., Newman, J. M., Combs, C., Lynch, T., Chimney, M. J., and Meeker, R. (2000). Chapter 6: Optimization Research for the Stormwater Treatment Areas. 2001 Everglades Consolidated Report Draft for Peer and Public Comment, September 1, 2000, G. Redfield, ed., South Florida Water Management District, West Palm Beach, Florida.

Chapter 3 – Reviewer: Lorraine Heisler

General comment – This is a well-written chapter that clearly presents the status of research directed toward defining the numerical phosphorus criterion for the EPA, and provides a lucid description of the rationale for the approach taken.

p. 3-9; Figure 3-2 – In the 1999 FDEP support document, I found tables listing a total of 18 metrics for which change points had been estimated. These included three for periphyton taxonomic composition, four for macrophyte frequency and/or biomass, six measures for invertebrate communities, and five measures for dissolved oxygen. It would be very helpful if Figure 3-2 included labeling (e.g., along the vertical axis) to identify the order in which different indicators appear in the cumulative distribution of the change points. This would be a useful summary of the ecological results. Also, it is possible that the use of multiple, presumably correlated, indicators as distinct measures of the same ecological feature might overemphasize change points because of "redundancy" of the measures. For example, the DEP report uses 25%, 50% and 75% percentiles of the DO distribution in the change point analysis, and all three measures yield a change point of 8.17 km (FDEP 1999). If these are all included in the graph of Figure 3-2 as distinct change points, they can create a "step" in the cumulative distribution that may not be as ecologically meaningful as it appears. Perhaps I misunderstand how the set of change points were assembled to create this figure; some additional information about the figure would help avoid confusion.

p. 3-11; Figures 3-3&3-4 – I agree with the statement that a relatively invariant time series of phosphorus concentrations provides a stronger basis for quantitative determination of the numerical phosphorus criterion than does a temporally-variable time series that leaves unresolved which of several different concentration values best measures the ecologically relevant nutrient conditions. However, the comparison provided by Figures 3-3 and 3-4 is not a convincing demonstration that the TP concentrations from the DUWC dosing study are significantly more variable than values from the WCA-2A gradient. Figure 3-3 plots the DUWC high-dose treatments, which are quite variable, on the same graph with the lower-dose treatments and controls, which are relatively invariant on the chosen scale. In contrast, Figure 3-4 presents only the TP concentrations for the "unimpacted" sites in WCA-2A transects. This makes the DUWC values appear to be extremely variable. However, the difference in apparent variability may simply be a scaling effect, in which the variance of a sample increases with its mean. To truly compare the amount of data variability, one needs to compare time series having comparable mean concentrations. A simple approach would be to include in Figure 3-4 the data from the "impacted" sites in WCA-2A. A more rigorous comparison would be to plot the standard deviations of the data for each site (or flume treatment) against the site (or treatment) means. One could simply "eyeball" these graphs to see if the two studies show similar or different patterns of covariation. More rigorously, the regressions of the standard deviations on the means could be used to test for differences between the two data sets, and a difference in intercept would indicate that one data set was intrinsically more variable than the other.

p. 3-14 – The second to last paragraph notes that the effect of water levels on P concentrations needs to be better understood before an upper limit on the P concentration can be identified. I would like to emphasize this point, especially considering that the data for defining the numerical P criterion are being gathered in an Everglades that may be subject to increased depths and/or flows in the future. This is especially the case for northern WCA-3A, where southward flows along the northern edge of the WCA are predicted to increase two-to-five-fold with implementation of the CERP D13R plan.

p. 3-15 – In the conclusion of the status summary for the WCA-2 P criterion, it is stated that "it is unlikely that additional research will identify a precise technicallydefensible concentration at which imbalances . . . will be statistically or measurably different from the EFA default criterion." This statement has a tone of finality that may not be warranted. For one thing, any difference from 10 µg/L can be statistically detected with sufficient sample size. More relevant, however, is that our current understanding of the Everglades ecosystem is far from complete, and under the changed conditions of the CERP, with increased depths and flows through much of the system, and reduced short-circuiting by interior canals, unanticipated changes in the flora and fauna are probable. Whether these will be desirable or undesirable, or will be associated with higher or lower TP concentrations, will be a matter for future research. For this reason, I would add the qualifier "in the next few years" to the concluding statement above.

p. 3-22, Figure 3-12 and 3-35, Figure 3-21 – Additional information is needed to explain the elements these box-plot figures.

p. 3-27 – The first paragraph is a copy of the preceding one on p. 3-26.

p. 3-29; Second-to-last sentence of first paragraph under "Macrophyte Response" -- I believe the intended word was "monotypic," not "monolithic."

p. 3-24 ff – The section on periphyton response is very well written. However, sites are referenced by ID codes ("X3," "Y4," etc.), whereas the supporting figures on pp. 3-22, 3-23, 3-26, and 3-27 are all graphed using distance from the canals. A short table, or labels in the figures, would help with translation between these two ways of classifying the sites.

p. 3-44– Three key assumptions of the Tetra Tech report are that extensive pond-apple swamp forests provided crucial habitat around Lake Okeechobee, that such forests would function similarly if located further south in the system, and that this habitat will not otherwise exist in the Everglades unless actively promoted by nutrient enrichment. However, many tree islands in the central Everglades already have pond apple swamp forests as a community type that occurs on the wet fringes of the island. For example, pond apple occurred in transects on 20 out of 27 elevated tree islands sampled in WCA-3 (Heisler *et al.,* in press). The highest frequencies of pond apple were observed in the relatively unimpacted areas of south-central and southwest WCA-3A and WCA-3B (all 15 islands sampled had pond apples), while the lowest frequencies of the species (two of six islands) occurred in WCA-3A north of Alligator Alley. I note also that increased phosphorus has been found in tree islands soils, possibly a result of natural concentration by vertebrates. Hence, restoration of Everglades tree islands alone may be sufficient to ensure that productive forested wetlands, include pond apple swamps, occur in the Everglades. Re-creation of additional forested wetlands would only be justified if future research identified a distinct ecological need.

p. 3-44, last paragraph -- The last sentence appears to set up an "either-or" situation in which the choice is between "small" impacts to the EPA marshes using only green technologies or undesirable "forced" chemical treatments. It may be premature to conclude that these are the sole available options or that the consequent zone of enrichment will be small.

Literature Cited

Florida Department of Environmental Protection. 1999 (in preparation). Everglades Phosphorus Criterion Development Support Document, Part 1: Water Conservation Area 2. Everglades Technical Support Section, Division of Water Resource Management , Tallahassee, Florida.

Heisler, I. L., D. T. Towles, L. A. Brandt, and R. T. Pace. (in press) Tree island vegetation and water management in the central Everglades. In A. van der Valk and F. Sklar, eds. *Tree Islands of the Everglades.* Kluwer Academic Press.

Chapter 3 – Reviewer: Michael Waldon

Utilization of DO concentration as an index of impairment resulting from nutrient loading is not widely applied because (1) both high and low DO may result from nutrient enrichment (Rast and Lee 1978; Rast and Marjorie 1988), and (2) low DO is often the result of allochthonous organic loading. For wetlands, the most diagnostic observation for identifying eutrophic conditions is occasional DO values greatly exceeding DO saturation. This was observed at station X1 (Figure 3-18). The recovery of DO along the gradient in Figure 3-18 is consistent with a hypothesis of organic loading into the canal, and organic oxygen demand from BOD decay along the gradient.

Literature Cited

Rast, W., and Lee, G. F. (1978). Summary Analysis of the North American (US Portion) OECD Eutrophication Project: Nutrient Loading-Lake Response Relationships and Trophic State Indices. *EPA-600/3-78-008*, USEPA ERL, Corvallis, Oregon.

Rast, W., and Marjorie, H. (1988). Eutrophication of Lakes and Reservoirs: a Framework For Making Management Decisions. *Ambio*, 17(1), 2-12.

Chapter 3 – Reviewer: Laura Brandt

In the conclusions in Chapter 3, Loxahatchee National Wildlife Refuge (WCA-1) updated findings, five stations from the SFWMD transect study are identified as reference sites. While these sites may be indicative of reference sites for that study, they do not include potentially useful data that have been collected throughout the interior of the refuge (16 station sampling). Values from these stations provide additional information on conditions in the interior (minimally impacted area) of the marsh and therefore information on natural background conditions.

In addition, three of these interior marsh stations have been identified for determining compliance in the Consent Decree. The Consent Decree indicates that “ Effective December 31, 2006, the long-term total phosphorus concentration levels for the Refuge will be the 10% rejection level of stations CA1-5, CA1-6 and CA1-16 at a given mean daily stage.” This value may be lower than 10 ppb default value of the EFA, and as stated in the Consent Decree: “Compliance with these concentration levels is expected to provide a long term average 14 station interior marsh concentration of approximately 7 ppb.”

Chapter 3 – Reviewer: Nick Aumen

General comment: Although I recognize the difficulty in addressing the following comment in the short term, I do believe that it should be addressed in the long term. The majority of data in this chapter are more structural in nature than functional. An increased emphasis on functional data, such as productivity, respiration, etc., would provide a better picture of the effects of elevated P on Everglades biota.

Page 3-45: The last two references, FDEP 1999 and FDEP 2000, are important references for this chapter, but are not generally available. The 1999 report was never officially released to the best of our knowledge, and the 2000 report was unavailable at the time these comments were written.

Chapter 4 – Reviewer: Michael Waldon

How will TP and DO standards be used to set NPDES permit limits? Would mechanical aeration of the STA effluent ever be required to meet permit limits? The specific methodology for application of standards to permit limit calculations should be fully documented prior to proposal of a TP site specific standard.

Chapter 8 – Reviewer: Nick Aumen

Page 8-2: It is stated that the US Army Corps of Engineers (USACE) is evaluating ATTs. To my knowledge, this reference is to the PSTA research planned in conjunction with STA-1E, and with the construction of an additional detention area downstream of S-322B. I do not believe that any research or data collection has begun in these areas, but if it has, this information should be included in this chapter. If the research has not begun, we question whether or not research results will be available in a timely enough fashion to contribute to the selection of an ATT or a combination of ATTs. If the research will not be available in time, we suggest that funds planned for these activities be diverted into other ongoing ATT research, particularly the green technologies.

Page 8-3: Under “Impacts of Section 404 Permit,” the SFWMD states that it, and/or its partners, are investigating all technologies listed the 404 Permit. We believe that sufficient information is available on several of these technologies to dramatically decrease, or perhaps even to curtail, further research. We also urge the SFWMD to work with the USACE to address the specific permit conditions to reflect new information and knowledge that has been gained since the permit was issued. Further research on technologies that virtually no agencies or entities support for wide-scale application draws very limited research funds away from the more promising and acceptable technologies.

Page 8-9: It is stated that a limerock substrate less prone to macrophyte growth may be necessary to support a viable PSTA system. This chapter should include a preliminary analysis (subject, of course, to refinement as new information becomes available) of the potential costs of various ATTs. For example, there is the possibility that it would not be economically feasible to construct a limerock base over the acreage that may be necessary for PSTA implementation. This is the reason that a fourth experimental cell with a peat substrate is planned for construction at the PSTA site just west of STA-2 Cell 3.

Page 8-10: In the final version of the Consolidated Report, the SFWMD should include the description and research plans for the fourth, peat-based PSTA cell described immediately above. The concept for this additional cell arose after the draft report was issued, and the information should now be included.

Page 8-14: It is stated that FIU's SERC has a contract with SFWMD that includes, among other things, efforts to coordinate across the different PSTA initiatives. We believe that coordination across all ATT research initiatives is absolutely essential, and would like more information regarding these ongoing coordination efforts.

Page 8-14: More information should be included regarding the 10-acre raised limerock bed constructed in STA-2 Cell 3 to investigate the development of a periphyton community. We would like the SFWMD to include information on the status of this project, summary data, and any other information from this project, and to describe what the future research plans are.

Page 8-25: We are concerned that the SFWMD is proceeding with CTSS research at a large scale and high expense, despite overwhelming consensus that such a technology employed at the full scale (post-BMP or post-STA) would not be acceptable to any of the agencies or entities involved in Everglades restoration. We believe that the initial phases of the research have been high quality and informative, and have produced sufficient information to make initial determinations of the lack of feasibility. We believe the technology has several fatal flaws if employed at the large-scale level. These flaws include: concerns over the use of S (in alum or ferric sulfate) as a coagulant in light of evidence linking sulfate increases to increases in Hg methylation rates; lack of information on the economic or ecological impacts of sludge disposal issues; transportation and storage issues related to bringing sufficient amounts of chemicals on-site, and storage and transportation of sludge by-products; and potential marsh-readiness issues (chronic rather than acute) related to chemical scrubbing processes potentially removing other ecologically essential chemical constituents (e.g., trace metals). At the public workshop, SFWMD personnel acknowledged these concerns, and indicated that they were proceeding because CTSS may have smaller scale, basin-specific applications. We agree with this assessment, but believe the research should then be focused only on these potential, smaller scale applications, which probably would not be in the footprint of the EAA. We believe that too much money is being focused on this technology, to the detriment of other, more passive technologies.

Page 8-33: Originally, it was our understanding that the SFWMD had completed research on LICD technology, and had no plans to pursue further research. However, SFWMD personnel recently indicated that they are continuing in-house research efforts on a subsequent phase of this project. As with CTSS, we believe that chemical addition technologies are not desirable at the large-scale level, and urge the SFWMD to focus resource and personnel commitments on the green technologies.

Page 8-38: We would like to have more data presented from the MWTS research that has been conducted in the test cells. Operation began in February, 2000, yet only narrative, preliminary results are provided.

General comment: We recognize that the Consolidated Report authors and editors struggle with the determination of what constitutes an appropriate level of detail to present in the Report. However, we are concerned that Chapter 8, representing approximately $10 million dollars of very good research, is relatively small, particularly in comparison with other larger chapters reporting results of much smaller magnitude research or data collection efforts. The upcoming decisions associated with selection of, and funding of, phase II P removal technologies are likely to have tremendous economic and policy implications. Therefore, we urge the SFWMD to provide more detail in the revision of this chapter, and to take into account the comments that we have provided.

Chapter 10 – Reviewer: Lorraine Heisler

Chapter 10 provides a lucid summary of the CERP RECOVER process, a process that is intended to insure system-wide integration of all future restoration projects. However, there is a need for a process to insure that the future restoration objectives of the CERP are incorporated into work on non-CERP projects, such as projects included in the Everglades Program. It seems especially urgent that projects that are going on now, such as the Everglades Construction Project, be reviewed in light of the additional features and future hydrology they will experience under the CERP. I would encourage the development of a CERP/non-CERP coordination process as a priority future task.