DEPARTMENT OF THE ARMY
SAVANNAH DISTRICT CORPS OF ENGINEERS
P.O. BOX 889
SAVANNAH, GEORGIA 31402-0889

REPLY TO
ATTENTION OF

August 15, 2003

Navigation & Coastal Projects

Subject: Calibration of a Water Quality Model for the Savannah Harbor, Dissolved Oxygen Water Quality Model, Savannah Harbor Expansion Project, Georgia and South Carolina

L.T. Keegan
Project Manager
Lockwood Greene Engineers
400 Mall Boulevard
Savannah, Georgia 31406

Dear Mr. Keegan:

I am transmitting via this letter comments received to date on the draft Dissolved Oxygen (DO) Calibration Report for the Savannah Harbor Expansion Water Quality Model. The advanced copies of comments were received from all of the technical representatives of the Savannah Multi-Agency Review Team (SMART).

It is clear by the content of these first sets of comments that the DO model's calibration is not nearly as far along and/or as fully documented as the SMART had hoped. Concerns have again surfaced as to whether or not a DO model based on the WQMAP platform will perform in the manner originally purported by your contractor, Applied Technology and Management (ATM).

Samplings of the concerns contained in the memos from the SMART are as follows:

- The hydrodynamic or water quality models are not technically defensible or useable to meet the [EPA] requirements. The [SMART agencies] had hopes to use this model have spent considerable time and resources over the last 3 years in providing technical suggestions and guidance for the development of this model. Based on the August 2003 report, these suggestion and guidance has largely been ignored. [EPA, enclosure 2]

- It is also [the EPA] opinion based on the poor performance and questionable technical defensibility, that this model WQMAP is not the appropriate tool for TMDL and WLA development for the harbor. Also [EPA] would not recommend it, in its present state, for use in evaluation for impacts of Harbor deepening. [EPA, enclosure 2]

- Based on the incompleteness of the report, [USGS] cannot recommend to U.S. Fish and Wildlife Service the acceptance of the model to evaluate impacts of potential deepening of Savannah Harbor. [USGS, enclosure 1]

- The calibration report is incomplete. In addition to the [other noted items], the sensitivity analysis section and the associated Appendix are not complete. [USGS, enclosure 1]
- In a nutshell, the modeling period should cover a certain hydrology condition (ideally low to high flows), meteorological conditions (fair weather and storm events), and tidal conditions (spring-neap cycles). The calibration time period (August-September of 1999, Figure 2-4) represents lower freshwater inflow (5297 – 8828 cfs vs. average summer flow of 9070 cfs). Is this sufficiently long to calibrate the model for the future use? [ERDC, enclosure 3]
- The model does not appear to capture well the range of data observed in Back River. [SAS, enclosure 4]
- The model does not appear to capture well the NH4 and NO3-NO2 loading from the marshes. [SAS, enclosure 4]
- The [draft report] states that the average difference in the 10th percentile is 0.48 mg/l. The [report also] states that the 10th percentile differences in the Back River are 0.56 mg/l. These differences are much larger than desired, particularly since impacts to aquatic species will be determined by using the values the model predicts for the 10th percentile D.O. concentration. [SAS, enclosure 4]
- Based on [SAS'] findings, the present calibration of the Water Quality Model is incomplete and therefore not yet acceptable for use in evaluating environmental impacts on the modeled river system. [SAS, enclosure 4]

My assessment of the SMART's preliminary review is that there is a significant amount of work yet to be done to provide the information that had been requested as well as the necessary refinements to predictive accuracy of the model. Schedule aside, I am very concerned about whether or not the WQMAP models will sufficiently assist the Corps of Engineers and our partners at the agencies in assessing potential project impacts.

In addition to the above, the District Engineer had a telephone conversation with the Region IV Administrator of the Environmental Protection Agency on Thursday afternoon, August 14, 2003. In that call, it was relayed that EPA has decided to begin setting up another water quality model other than WQMAP for the harbor TMDL. I had hoped that such a decision would not be made until after we jointly discussed the modeling status at the meeting scheduled for Wednesday, August 20, 2003.

Although there is a great potential for schedule and cost impacts to the SHE Project, I am confident that by working together with all of the Cooperating Agencies, we will achieve our

stated goal of developing an environmentally compatible project that will provide efficient and cost competitive transportation to meet the increasing demands of worldwide waterborne commerce. It is therefore imperative that as soon after the meeting next Wednesday as possible, that you provide us with specific commitments for completing the work you have underway with ATM (both in terms of schedule and quality).

A copy of this correspondence will be provided to the other members of the SMART for their information.

If there are any questions or concerns regarding this correspondence, please feel free to contact me at (912) 652-6119 or via e-mail at douglas.h.plachy@sas02.usace.army.mil.

Sincerely,

Signature Not Verified

Douglas H. Plachy

Digitally signed by Douglas H. Plachy
DN: cn=Douglas H. Plachy, o=USAED, Savannah, ou=CESAS-PM-CM, c=US
Date: 2003.08.15 22:32:36 -05'00'
Reason: I am the author of this document
Location: Savannah District, U.S. Army Corps of Engineers

Douglas H. Plachy
Senior Project Manager
Navigation & Coastal Projects

Plachy, Douglas H SAS

From: Paul A Conrads [pconrads@usgs.gov]
Sent: Wednesday, August 13, 2003 9:15 AM
To: William.G.Bailey@sas02.usace.army.mil; Kim, Sung-Chan; jimgepa@mindspring.com; Douglas.H.Plachy@sas02.usace.army.mil
Cc: Ed_Eudaly@fws.gov; Greenfield.Jim@epamail.epa.gov; KitchensW@wec.ufl.edu
Subject: Preliminary Comments on DO Calibration Report

Preliminary
ıments on DO mc

Attached are my comments for Doug on the Calibration Report.

Take care,

Paul

(See attached file: Preliminary comments on DO model.doc)
Paul Conrads
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Memorandum

To: Douglas Plachy, U.S. Army Corps of Engineers, Savannah District

From: Paul Conrads, U.S. Geological Survey, South Carolina District

Date: August 13, 2003

Subject: Preliminary comments on "Calibration of a Water Quality Model for the Savannah Harbor"

Below are my preliminary comments on the review of the subject report. Based on the incompleteness of the report, I cannot recommend to U.S. Fish and Wildlife Service the acceptance of the model to evaluate impacts of potential deepening of Savannah Harbor.

1. Section 1.1, page 2, 1st paragraph – "*The purpose of this report is to present the work performed to provide an improved water quality model of the Savannah Harbor.*" None of the material presented in the report offers a comparison with the EIS I model developed using the 1997 data set. The intent of the data collection effort in 1999 was to improve the calibration of the DO model. The report needs to document these improvements.
2. The Federal agencies requested that statistical analysis be performed on the 1997 and the 1999 data sets for specific spring/neap tidal cycles. The requested analysis has not been done for either data set.
3. Section 1.2 – Discussion of the hydrology for 1997 needs to be added and the 1997 flows need to be shown in figure 1.2
4. Section 1.2 – Minor point: Some would argue whether the reduction of maximum flows in the river was due to the dams or climate changes.
5. Figure 2-2 – Provide an explanation/legend for the figure.
6. Section 2.2.4- The increase in flow below Clyo would be distributed throughout the Savannah Estuary. How does this artificial increase in the flows affect the transport of materials in the system? How sensitive is the salinity transport to the upstream flow condition?
7. Section 2.2.7. A better explanation of why an empirically derived mixing scheme is defensible needs to be added since the model will be used to predict changes due to geometry changes. Presumably, a mechanistic model was applied to the

system because it is assumed that the physics of the system will change after a channel deepening and/or "re-plumbing." Using the empirically derived vertical mixing, the model assumes that the mixing will not change under different channel depths and configurations. Is this contradictory? Other questions with the approach need to be addressed. Is the mixing scheme based on 1997 measured data or 1999 measured data? If 1999, was it compared with 1997? How is the vertical mixing distributed throughout the model domain? How sensitive are the DO simulations to the vertical mixing?

8. Section 2.2.10 – The first ten days of the simulations showing the transport between the BFHYDO and BFWASP appear consistent. However, the complete 71-day simulation is shown and there are obvious discrepancies between the simulations. For some stations, BFWASP over predicts the surface data but agrees for the bottom stations. For other stations, BFWASP under predicts the bottom stations but is okay for the surface stations. Why are there differences between the models and between the stations? How does this effect the water-quality simulations? Why is this acceptable?
9. In the previous "Approval Package", comparison of simulated DO saturation was presented. A similar presentation (along with DO deficit) is absent from this report. How does the discrepancy between the BFHYDRO and BFWASP affect the expected minimum DO error?
10. Section 4.2, page 20. A better explanation of how O'Connor Dobbins is being applied to a layered model with respect to the issue of actual and effective depth is needed. A showing plot simulated and the measured reaeration values is needed.
11. Section 4.2 – The discussion at the May 7th meeting was that the assumption of light limitation would be evaluated with the model. Was this done? What was the outcome?
12. Section 5.3.2, page 28, Appendix B- A better explanation of why a simulated boundary is used instead of the measured data is needed. If the RIV1 simulations do not match the measured data, why is it justified to introduce known error at the boundary? Measured data points need to be included on the DO, BOD, NH4, NO3, ON, and temperature plots. Figure numbering is not consistent in Appendix B.
13. Section 5.3.2, page 29 - *"This output generally agreed with data and provided reasonable values for the upstream but adjustments were made to the average level of the CBODu output from the RIV1 model. The reduction of this value was necessary to get the model to simulate the concentration levels at the upstream extents of the area of concern (above I-95)."* This approach to boundary conditions and calibration needs further discussion. The measured data from intensive data collection is replaced with simulated values with known errors as compared to the measured data. These values are then adjusted as a calibration tool to be able to adequately simulated concentrations at an interior point. Are the kinetics within the model unable to simulate the processes occurring in the system? If the model cannot simulate the BOD transformation in the riverine segments of the model domain, how can the end-user believe that it can simulate the transformation in the more complex estuarine segment of the domain? Why

isn't this same approach taken for other constituents? Do the plots in Appendix B show the upstream boundary inputs or are the values shown prior to calibration adjustments?

14. Page 30. The table number is missing.
15. General comment – In many of the figures, the explanation is covering up data on the plots.
16. Section 7.0, page 42, last sentence. *"In essence very little alteration of the measured rate kinetics, constants, and inputs was performed to achieve this calibration, and the results reflect the best available knowledge of the loads to system without significant manipulation to achieve best fit statistics, i.e. curve fitting."* I don't understand the point that is being made. Is it that the calibration process was minimal and was based on estimates of loadings to the system rather than adjustments of model parameters? In my reading of the report, and the previous "Approval Package", measured rate kinetics, constants, and inputs have been changed to improve the calibration or "curve fit." From the previous "calibration", the following rates and constants have been altered – temperature coefficient for reaeration, SOD, organic matter settling velocity, organic carbon decomposition rate, temperature coefficient for nitrification rate, denitrification rate and its temperature coefficient, and the nitrogen/carbon ratio. My understanding of the upstream BOD boundary, was that it was used as a calibration parameter which would appear to be "significant manipulation to achieve best fit…" Is this contradictory to the final statement of the report?
17. Perhaps including a section on calibration process and targeted goals would clarify the last remarks in the report (Item 16). In the early meetings of the MTRG, discussions highlighted a need for absolute predictions. Now the adequacy of the model is based on its ability to reproduce "trends." Do the model developers think this is satisfactory and why?
18. The calibration report is incomplete. In addition to the item noted above, the sensitivity analysis section and the associated Appendix are not complete.

Plachy, Douglas H SAS

From: Jim Greenfield [jimgepa@mindspring.com]
Sent: Wednesday, August 13, 2003 11:17 AM
To: 'Paul A Conrads'; William.G.Bailey@sas02.usace.army.mil; Kim, Sung-Chan; Douglas.H.Plachy@sas02.usace.army.mil
Cc: Ed_Eudaly@fws.gov; Greenfield.Jim@epamail.epa.gov; KitchensW@wec.ufl.edu
Subject: RE: Preliminary Comments on DO Calibration Report

Memo wqMAP
v Harbor calibrat

Here is my first (and may be last) cut on comments

Not a happy camper

Jim

Memorandum

To: Douglas Plachy, U.S. Army Corps of Engineers, Savannah District

From: Jim Greenfield, U.S. EPA Region 4

Date: August 13, 2003

Subject: Preliminary comments on "Calibration of a Water Quality Model for the Savannah Harbor" and overallr Savannah Harbor WQMAP Calibration Process

I have completed an initial review of the Draft Savannah Harbor Water Quality Model (WQMAP) Report submitted by ATM on August 4,2003, the WQMAP model update submitted on August 11, 2003 and the Draft Hydrodynamic Calibration Report submitted August 2002.

These products were being developed to assist USACE, USEPA, USFWS and USGS in preparing an EIS that would evaluate the environmental impacts of the Savannah Harbor Deepening Project. And to answer the questions on how the deepening may impact the harbor system salinity and Dissolved Oxygen regimes and if impacts are noted what mitigation actions can be enabled to mitigate the potential impacts.

In addition this model was potentially to be used to develop D.O. TMDLs for the harbor system, evaluate potential water quality standards changes and used by EPA and the States of Georgia and South Carolina to develop wasteload allocations for the wastewater dischargers in the Harbor area.

The hydrodynamic or water quality models are not technically defensible or useable to meet the above requirements. The above parties who had hopes to use this model have spent considerable time and resources over the last 3 years in providing technical suggestions and guidance for the development of this model. Based on the August 2003 report, these suggestion and guidance has largely been ignored.

It is also my opinion based on the poor performance and questionable technical defensibility, that this model WQMAP is not the appropriate tool for TMDL and WLA development for the harbor. Also I would not recommend it, in its present state, for use in evaluation for impacts of Harbor deepening.

That said, this project and the time and effort spent by the consultant and the MTRG and the SMART has not been a complete waste. We now have a good 1997 and 1999 data set of the harbor, a good 1999 wastewater characterization of the dischargers, projects in place to conduct an neural net analyses of the system to determine cause and effects, a better understanding of the marsh loads, good longterm BOD sets and a good technical working group of affected stakeholders. Bottomline a good basis to develop a working tool to evaluate the harbor and its stressors.

The following are detailed comments on the reports and model. Again these are comments to illustrate the technical deficiencies not a list of items that if corrected would

make the reports and models technically acceptable. That is the responsibility of the authors and developers of the report and model. Also note most of these comments were previously brought up during the past 3 years – these are not new comments or concerns General comments on the models' technical defensibility

The hydro model was described by Spaulding and Muin in a 1997 paper. This paper provided the technical defense of the hydro model. I believe that the model authors also conducted benchmark testing to demonstrate the models capabilities and to assure the model was technically correct. The model as applied to the Harbor, as I understand, was this model with the second order advection terms turned off. If this is the case then further technical information included and benchmarks run and documented that the model as applied to Savannah Harbor is equivalent to the model documented by Spaulding Muin and a detailed discussion of the impacts of turning off the momentum advection terms.

The hydro model solution technique was modified to incorporate a timeseries site specific adjustment to the Richardson Number and the mixing equations. This adjustment must never bee adequately explained or justified. Detailed explanation on how the adjustment was determined for both 1999 and 1997 and how or if this adjustment is valid for any other time periods.

No technical documentation was provided on the changes or adjustments made in the original BFHYDRO or the WASP5 codes. These documentations along with the modified models' fortran code (not the WQMAP interface) should be included.

BFHYDRO and BFWASP salinity comparison illustrates that BFWASP is not transporting mass the same as BFHYDRO at times over 20% difference.

If BFWASP uses the WASP5 kinetics and solution techniques, then the Euler solution technique is being used. For a system that has large concentration gradients, the Euler solution technique is not applicable; it will flatten out the results and not represent the gradients.

The 2002 hydro calibration report concluded that the model performed well when the State Space modeling adjustment was applied and then the model met the overall goals of the expectation document. The model without the State Space adjustment did not meet the salinity goals or provided an adequate salinity calibration. However the hydro model output without the necessary State Space adjustments were used to drive the water quality model. The water quality model results are automatically flawed by using 1) the non adjusted salinity values and 2) a model whose transport mechanisms are not adequately calibrated.

The hydro model was only run for summer 1997 and summer 1999 and the water quality model only for summer 1999. This does not provide a good time period to make an assessment on whether or not the model can handle a wide range of flow and tidal

conditions. The model should be run for multiple years to demonstrate its stability and usefulness over time.

The model, both hydro and water quality, take a long time to run – anywhere from 1 to 1.5 hours for everday of simulation time. (faster computers will decrease this rate by may be 50%). So for a 90 day run we can expect a 2 day turn-a-round time and a year run may take up to 7 days. This type of model performance, even with faster computers, is not acceptable when dealing with the hundreds of runs that will be needed to develop the EIA and TMDL results.

Both Bill and Pauls' preliminary comments address the short comings of the report and of the model calibration. I have the same concerns, but until the model itself is documented to be a valid application any development of model results or model calibration is a waste of resources. Again these points have been presented and discussed over the past 3 years.

The report does not demonstrate why the August 2002 hydrodynamic calibration is adequate for driving the water quality model.

The report does not address DO saturation or DO deficit as request at about every meeting for the last 2 years.

The report does not address the algal issues or potential nutrient impacts and the impacts of light limitation.

The sensitivities in the report seem to be an after thought. These and more sensitivity analyses should have been completed a year ago to show what is or what is important to the model. And for each parameter a case made why the boundary and rate selection is the appropriate number and how sensitive are the model results to each boundary condition. As suggested in the 2002 EPA and ATM memo a weight of evidence approach could have and should have been taken. But because of the possible transport errors in the model any calibration plots are meaningless, if a model can not predict the physics of a system then it can not be used to predict the chemistry or represent the biology.

The results that the marsh has minimal impact on D.O., when it has the highest BOD loading must be justified by collaborating evidence or an explanation why the marsh loads do not impact DO.

A previous commitment to include multiple decay rates in the model for sensitivity analyses was not honored, thus making this issue a sticking point between the stakeholders.

General model

Plachy, Douglas H SAS

From: Kim, Sung-Chan ERDC-EL-MS
Sent: Thursday, August 14, 2003 3:36 PM
To: Plachy, Douglas H SAS; 'Paul Conrads (E-mail)'; 'Jim Greenfield (E-mail)'; Bailey, William G SAS
Subject: Comments

Sensitivity: Confidential

Categories: Savannah Harbor Expansion

Memorandum_8_14_03.doc

Dear all,

The following is my comment on the calibration draft report.

Memorandum

To: Douglas Plachy, CESAS-PM-CN
From: Sung-Chan Kim, ERDC-EL-MS
Date: August 14, 2003

Subject: Comments on "Calibration of a Water Quality Model for the Savannah River"

The followings are my comments on the report:

1. In a nutshell, the modeling period should cover a certain hydrology condition (ideally low to high flows), meteorological conditions (fair weather and storm events), and tidal conditions (spring-neap cycles). The calibration time period (August-September of 1999, Figure 2-4) represents lower freshwater inflow (5297 – 8828 cfs vs. average summer flow of 9070 cfs). Is this sufficiently long to calibrate the model for the future use? Weather conditions (wind, air temperature, precipitation, etc) were not shown except mentioning location map of the weather stations (Figure 2-6: I cannot find this).
2. Checking interface between BFHYDRO and BFWASP was done for the period between 8/8/99 (day 219) and 10/14/99 (day 286). Model spinup was set for 7/22/99 (day 202) to 8/4/99 (day 215) and the simulation follows to 9/10/99 (day 252). It seems two separate runs were made. The reasoning has to be stated.
3. The interface checking (Appendix C) shows BFWASP has higher surface salinity near the entrance (GPA 26 and 02) and lower bottom salinity upstream (GPA 04, 21, 26 an so on). This suggests BFWASP has more mixing than BFHYDRO. It was stated (2.2.8) that both are using the same mixing coefficients. More explanation is needed.
4. There is no modeling of algae and light extinction. Having said that "the primary productivity in the harbor is highly light limited" (page 17 the last sentence), another option other than dropping the algal term is to model algae and light (or sediment). Of course, this is an afterthought.
5. I think dispersion/diffusion of the system is not adequately represented. Figure 5-12 shows seemingly reasonable spatial distribution of the median DO. But 10 percentiles are quite different. This is explained by time series plots (Appendix D), essentially showing that the time variation (mostly intra-tidal) from the model is much smaller compared to the one from the observation (see for example GPA08) where the horizontal gradients are strong. The model seems too diffusive. Also it would be good to have plots for both the surface and the bottom use the same horizontal scale in Figure 5-12. This brings another question: why the front river surface data comparison was stopped at GPA09 when the bottom was extended to GPA17.
6. I think the comments 3 and 5 are related to diffusion. Developing new formula for vertical mixing was reasoned because of "the inability for standard turbulence formulations to capture (stratification-destratification cycle)" (Page 9 second paragraph). I wonder how much real scrutiny was given regarding the failure of

turbulence model. The fact that a stratification-destratification cycle is essentially a neap-spring cycle could provide basis for relating mixing to tidal range. But, I am not confident of excluding hydrology as a factor. It would be helpful if more explanation of the reasoning, the algorithm, and examples (such as time series of the profiles of mixing coefficients also as a function of river miles) regarding the mixing are provided.

Plachy, Douglas H SAS

From: Bailey, William G SAS
Sent: Friday, August 15, 2003 6:54 AM
To: Plachy, Douglas H SAS
Subject: Hydro model

EXPAN Hydro
ıts on Jul 03 WQ

CESAS-PD-E 11 August 2003

MEMORANDUM FOR PM-CM (Plachy)

SUBJECT: Savannah Harbor Expansion Project;
Review of ATM July 03 Calibration Report on Water Quality Model

1. I reviewed the subject report and have the following comments:

A. Section 2.2.7, page 9, 2nd para. The turbulence parameterization needs to further explained to ensure that the approach is valid for analyzing changes in channel depth.

B. Section 2.2.9, page 10. The first sentence should be revised to delete reference to draft reports.

C. Section 3.0, page 12. The last sentence refers to Appendix A containing a certain report. That appendix does not contain the entire report. Either the sentence should be revised or the remaining material included in the appendix.

D. Section 5.1, page 26, 3rd para. The 3rd line states that SOD rates were calculated by linear interpolation between the 14 stations. This appears to contradict other sections of the report (Figure 2-1 of Appendix B).

E. Section 5.2.2, page 27. The 4th line refers to a certain report. Since Section 3.0 stated that the report was included as a appendix in this document, the reference should be changed to be Appendix A.

F. Section 5.3.1. The last paragraph implies that additional modifications to the model will be performed to incorporate dynamic offshore boundary conditions. The revised Calibration Report should include this modification.

G. Section 5.3.4, page 30. The end of the 1st paragraph refers to a turning basin. Since there are several turning basins in the harbor, this section should be revised to refer specifically to the Kings Island Turning Basin.

H. Section 5.5.2, page 35. The Expectations Document had also requested a comparison of the model's performance with Dissolved Oxygen Deficit.

I. Section 5.5.2, page 36. The 2nd paragraph states that the average difference in the 10th percentile is 0.48 mg/l. The 3rd paragraph states that the 10th percentile differences in the Back River are 0.56 mg/l. These differences are much larger than desired, particularly since impacts to aquatic species will be determined by using the values the model predicts for the 10th percentile D.O. concentration.

J. Figures 5-3 and 5-4. Footnotes should be added to these figures to clarify that no values are shown for point sources or sediment flux because no organic nitrogen measurements were taken of those sources (as stated on page 27).

K. Figure 5-8b. The model does not appear to capture well the range of data observed in Back River near River Mile 23.

L. Figure 5-9a. The model does not appear to capture well the range of data that was observed.

M. Figure 5-9b. The model does not appear to capture well the range of data observed in Back River near River Mile 14.5.

N. Figure 5-11a. The model does not appear to capture well the range of data observed, particularly at the surface near River Mile 18 and on the bottom near River Miles 14 and 43.

O. Figure 5-12a. The model does not appear to capture well the range of data observed near River Mile 21 or the lows observed between River Miles 14 and 22.

P. Figure 5-12b. The model does not appear to capture well the range of data observed in Back River.

Q. Figure 5-17. The model does not appear to capture well the NH4 and NO3-NO2 loading from the marshes.

R. Section 6.3, page 39. The 2nd paragraph states that all of the sensitivity runs were not included in this document. The additional ones should be included in the revised Calibration Report.

S. Section 6.3, page 39, last para. The apparent beneficial effects on the model's error statistics from an increase in SOD would lead one to question whether higher SOD rates should be used in the model.

T. Section 7.0. The revised Calibration Report should include the model / data comparisons to the 1997 validation data.

U. References, page 43. There appear to be duplicate references included: ATM 1997 Hydrodynamic and Water Quality Monitoring … July-September 1997 …", and ATM 2000, same name.

V. To help make the final report more of a stand-alone document, the following additional reports that contain information from other sources that was used in development of this model should be placed on the CD containing the final report:

- GAEPD, 1985

- Law Engineering, 2000 Long Term BOD
- Law Engineering, 2000 Wastewater Characterization
- USEPA, 1999
- Whitlock, 2002

W. Appendix A, Section 5, lines 3&4. Please include whatever information is appropriate from that report in this document so that readers do not have to search for a separate document.

X. Appendix D. Numerous plots show the model as not capturing well the range of observed D.O. data.

Y. Appendix E is not complete. The revised report should contain all the sections.

2. Based on my above findings, the present calibration of the Water Quality Model is incomplete and therefore not yet acceptable for use in evaluating environmental impacts on the modeled river system.

William Bailey
Physical Scientist