

Response to MEG March 7, 2013

James O'Donnell and James Fitzpatrick

Stacey Comments

1. We agreed that 1998-2000 appear to be anomalous and that we should focus on the other years at the outset and return to this period later. It is important to note that we will be modeling 1999-2002 (1998 not included in modeling effort. It was included in the data analysis to illustrate the unusual behavior in C:Chla ratios).
2. The load graphic was used to illustrate the general effect of management on discharges. It was a convenient and well constructed graphic. We did not mean to imply that these numbers were used in the model. We have received loading estimates from Kelly Streich (CTDEEP) and will be updating the model with revised fall-line loading estimates. (Thanks to Kelly for her help)
3. The Dam-O'Donnell report is at

http://sp.uconn.edu/~odonnell/reports/Dam_Final_Report_LI-97127501_8_2010.pdf
4. We agreed that the schedule is compressed and we are hoping to revise it.

Bierman Comments

1. The primary goal of the MEG is not to review the whole model but to advise on the technical aspects of the project. Comments on the broader issues by experts are welcome of course and will be addressed by the program office.
2. The scope of the current project does not include the reassessment of physics.
3. We will focus on the 88-89 and 94-95 periods first and then the 2000-2002 period.
4. The reason to introduce the variable C/N ratios is to improve the DO calibration while retaining realistic mixing. HydroQual has been using the variable stoichiometry algorithm for a number of years and found it to function well and it does not appear to slow computations down.
5. The development of a better light absorption representation is valuable. We will consider it, but it is not part of the present scope of work. Currently, we are only considering the chl-a correction to the observed light attenuation fields, as discussed in the handout.
6. The use of CFD for model-data differences is something we will explore in the sensitivity analysis.
7. We appreciate the reference to the CBP and the Cole (2010) report.

8. The silica limitation only occurred in a simulation with forcing of academic interest. The current model does have provision for a third phytoplankton group, but it is not active for the current SWEM work. We will discuss the approach recommended.

9. The numerical experiments suggested were conducted in earlier assessments and will be repeated. The mass balance assessments will also be undertaken.

Brush Comments

1. We agree with the initial focus on the recalibration for 88-89 and then revisiting the anomalous years.

2. We have assessed the C-CHL data as suggested.

3. We have explored the effects of a variety of C/CHL ratios as suggested.

4. We are in the process of seeking additional phaeophytin data.

5. We have not yet investigated the grazing rate representation. Initially SWEM utilized grazing rates, ranging from 0.03/day (from November-February), 0.10/day (March-May), and 0.06/day (June-October) based on limited data collected by Battelle (Duxbury) as part of the SWEM field effort. Current calibration results are using temperature driven rates of 0.10/ day at 20 deg C for diatoms and 0.05 /day for the summer mixed assemblage. Considering use of a higher rate for the summer group.

6. There is no evidence of anomalous weather conditions in 98-99.

7. Water column model respiration rates can be indirectly compared to observed BOD₅ data. There are very limited measurements of SOD to compare to modeled values. We have some from the LISICOS program and we will compare these to model predictions in the near term. .

8. Comparison to the BZI estimates might reveal inconsistencies. It is also not part of the current scope.

Grear Comments

1. Seeking an explanation of the anomaly in CHL is not a central issue for this step in model development. It is simply noted as a challenge. We are focused on getting the DO right with mixing present.

2. There does not appear to be much evidence of a weather anomaly.

3. The grazing is specified using limited data (see discussion above). We are also looking at a seasonal variation that is temperature dependent. Previous work has suggested that including a lag term (i.e.,

chl-a concentrations from 10 days earlier) in the grazing term doesn't change the computations in any significant way. Might be more effective if lag time increased.

4. The issue of significance in the skill metric is a good point and we will address it.

5. The Issue of statistical significance and model optimization is broad and interesting, however, the step we are addressing is simple. The model was far from optimal. We are moving it closer to realism and we will then address refinements.