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Biogeosciences Discussions

Interactive comment on "Using satellite-derived backscattering coefficients in addition to chlorophyll data to constrain a simple marine biogeochemical model" *by* H. Kettle

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This paper presents results of a statistical parameter optimization for a 1D biogeochemical model that is implemented for three different sites and uses surface chlorophyll and backscatter data. This work is relevant within the broader topic of how to formulate biogeochemical models (in terms of functional complexity) and how to constrain them given limited observations. The manuscript should be interesting to the readers of Biogeosciences; it is well structured and well written. Aside from a number of minor comments that should be straightforward to address, I have one major comment.

Major comment: Reporting the *a posteriori* errors of the optimized parameters (as described, for example, in Fennel et al., 2001) would greatly add to the results and would allow the author to discuss the value of adding backscatter data more definitively. Such an error analysis would also help in deciding which parameters can be tuned effectively (see p. 4210). For example, I noticed that of the optimized parameters, many are at the upper or lower bound of their predefined range when using chlorophyll data only, which implies that they are not at the minimum of the cost function (see Table 3). Fewer parameters are at their bounds when both chl and bb data is used. This is consistent with the idea that fewer parameters are constrained by chl alone (postulating that parameters at their bounds are poorly constrained by the data); more parameters are constrained when bb is used in conjunction with chl. Calculating the *a posteriori* errors for the different cases would likely produce quantitative evidence of this and greatly strengthen the main conclusion of the study.

Detailed minor comments:

General question: was the model optimized independently for all stations only, or also for all stations simultaneously. Obviously this is relevant for 3D biogeochemical models that have to characterize a range of biogeographic regions with a single parameter set. The study by Friedrichs et al. (2007) seems quite relevant in this specific context and for the discussion in general.

p. 4202-4203, l. 26, l. 1-2: "...exporting POC to the seabed (a process called export production)" It's a misconception that biological export involves transfer to the seabed. The amount of POC that actually makes it to the seabed is minuscule compared to the amount of biologically exported carbon below the thermocline.

p. 4203, l. 5: modify to "export to the deep ocean" or something similar

p. 4204, l. 3: colour is misspelled; also, I would suggest to iterate the "a" is absorption here

Section 4.2 (first paragraph) and 4.2.2 I would much prefer to see the study done with

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Geider's C:Chl model instead of Cloern's simple empirical relationship. Cloern's model uses daily mean irradiance in the mixed layer, which doesn't seem particularly useful in models' daily cycle in irradiance. Geider's model is widely used in biogeochemical models (including the author's own recent modeling study); hence, it would be much more relevant.

p. 4208, l. 21: It's not obvious how mixed layer temperature and mean daily mixed layer light are used in a vertically resolved model. What happens below the mixed layer? More detail is needed on this.

p. 4211, l. 10: Is a factor 1/J missing from the equation?

p. 4211, l. 12: The values of σ_j should be reported.

p. 4212, l. 22-23: How was convergence tested for?

p. 4213, l. 6-7: "...in all cases here there is only one optimum parameter set for each site" How is this statement supported? Do repeated optimizations yield the same parameter set? Should be clarified.

p. 4213, l. 12-13: "Adding bb data causes increase in chl error." More relevant is whether the additional data helps constrain the model better, i.e. whether it decreases the *a posteriori* errors. The fact that fewer of the optimized variables run into their imposed upper or lower bounds suggests to me that adding bb did indeed add more information.

p. 4213, l. 14-15: "...in some cases bb RMSE errors are smaller when the model is not tuned to bb." If this is the case because calculation of RMSE errors uses mean bb instead of specific bb for the experiment (as stated on l. 17-18), it would be better to calculate the RMSE using the data that were actually used in the optimization rather than the mean.

p. 4213, l. 16: Should be "worse than" not "worst than".

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Section 5.2, first sentence: How well the parameters are constrained can be calculated as noted above. Again, reporting the *a posteriori* errors would be extremely useful for this discussion and greatly add to this study.

p. 4214, l. 27-28; p. 4215, l. 1: Again, *a posteriori* errors would strengthen this argument. Any model with enough degrees of freedom can be tuned to minimize a cost function (i.e fit the data better). However, that does not imply that the model has more predictive skill. I think large uncertainties result in the discrepant results at ESTOC (see next comment).

p. 4215, l. 6: ”At ESTOC data are too noisy to constrain the model.” I don’t think the noise is the problem, but insufficient dynamical range. The same conclusion was reached by Fennel at el. (2001) when exploring the information content of data from BATS versus a higher latitude station with more dynamical range.

p. 4215, l. 14: Why is the model output not compared to other data from the three sites? For example, ESTOC is a time series site with profile data on nutrient, POC and chlorophyll concentrations.

p. 4216, l. 2-6: The discussion of air-sea CO_2 flux are not very convincing, but I also think that the modeling of CO_2 fluxes is outside the scope of this paper. Using vertically constant, global average values for DIC and alkalinity (as stated on p. 4208) is likely not accurate. No comparisons of simulated pCO_2 with observed values are presented. I don’t see why the statement ”the uptake of DIC by phytoplankton are largely constrained by chl not bb at CIS and PAP” would be true. The few sentences on DIC and CO2 fluxes could simply be removed.

p. 4216, l. 10-11: The export flux could be easily calculated and would be a more useful number for comparing with observational estimates of export than “the amount of detritus below 200m on the last day of the simulation”.

p. 4216, l. 14: Remove reference to seabed in connection with export (see comment

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above).

p. 4216, l. 20-22: Again, this statement would be more convincing with *a posteriori* errors reported.

p. 4217, l. 1: Again, remove reference to seabed.

Fig. 3: Which data is shown at the x-axis and which at the y-axis?

References

Fennel, K., M. Losch, J. Schröter and M. Wenzel (2001), Testing a marine ecosystem model: Sensitivity analysis and parameter optimization. Journal of Marine Systems 28/1-2, p.45-63

Friedrichs, M. A. M., et al. (2007), Assessment of skill and portability in regional marine biogeochemical models: Role of multiple planktonic groups, J. Geophys. Res., 112, C08001, doi:10.1029/2006JC003852

Interactive comment on Biogeosciences Discuss., 6, 4201, 2009.