**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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Review of: "Using satellite-derived backscattering coefficients in addition to chlorophyll data to constrain a simple marine biogeochemical model" by H. Kettle

In this manuscript, the author uses a one dimensional biogeochemical model and a genetic algorithm to (1) assimilate chlorophyll concentrations (chl) alone and (2) assimilate chl as well as the backscattering coefficient (bb, related to particulate organic carbon, POC). Kettle finds that assimilating both chl and bb has a significant effect on detritus. Furthermore, because there are a variety of different algorithms available for estimating backscattering, the analysis demonstrates that the uncertainty in bb does

not significantly affect the assimilation results.

General comments:

I liked the approach of assimilating remotely-sensed data other than chlorophyll. This is, somewhat surprisingly, a relatively novel idea in the field of biogeochemical data assimilation, and is appropriate for publication in Biogeosciences. However, there are a few issues that I would like to see addressed before publication.

1) Some of the algorithms that I am familiar with that estimate POC from Rrs or nLw are developed with data that include zooplankton carbon (< .5cm or so) in their estimate of POC. What type of zooplankton is included in the NPZD model used here? If a small zooplankton component is included, shouldn't it be included in equation 8 for POC? How would this change the results?

2) In order to rigorously test the benefit of assimilating b_bp data as well as remotely sensed chlorophyll data, it would be best to test the simulation results against an independent set of data, or at least a subset of the available data that were not assimilated (see Friedrichs et al. 2006, 2007). By computing RMSE using unassimilated data for the various numerical experiments conducted, the reader would be better able to determine whether the assimilation of b_bp improved the predictive ability of the model. I believe this analysis would strengthen the main messages of the paper.

3) Finally, I think the paper would be improved if the issue of parameter uncertainties were addressed. Are the differences in parameters listed in Table 3 significant? (Clearly not to four significant digits!) Additional discussion of what it means when optimal parameter values are at their upper or lower bounds (i.e. they are not constrained – perhaps correlated to another optimized parameter?) would be very helpful.

Specific comments:

Abstract. It's somewhat confusing to mention that four IOP algorithms exist, yet only three are used. Perhaps remove the 'four exist' part.

p. 4203, line 15: Another issue that should be brought up here is that we actually need not only an estimate of phytoplankton carbon, but the rate of fixation, i.e. productivity, which depends on light, nutrients etc... There are lots of models that estimate productivity from ocean color, but many of these are associated with large errors.

p. 4204, line 2: Typically POC is also made up of small zooplankton as well. Whether or not zooplankton carbon is included in the four algorithms should be discussed.

p. 4205-6: I was under the impression that only $b\_bp(490)$ was being used. Why is the models' ability to estimate $b\_bp$ at other wavelengths mentioned here? Can more details be given for the PML model? (Since the QAA isn't used, more details are probably not necessary there.)

p. 4206: Figure 2 calls the first model 'Loisel', whereas the text calls this 'LP'. It might be nice to be consistent in that regard.

p. 4209: Are the results in the paper sensitive to the $b\_bBG$ parameter? Where does this come from? Is this output from Fujii's model, or did he get this from another study? (If the latter, then it would be better to use the primary reference.)

p. 4210: dimishing should be diminishing

p. 4210: Section 4.2.4 would be easier to follow if we better understood the vertical resolution of the model. What are the depth intervals used? How many are within z90? Does z90 vary in space and time?

p. 4211: Please report observational variances.

p. 4211: The author, and other readers, might be interested to know that a recently 'accepted' paper in the Journal of Marine Systems (Ward, Friedrichs, Anderson and Oschlies, 2009, available upon request) used a one-dimensional ecosystem model as a framework for comparing the variational adjoint method with a genetic algorithm. One of the major conclusions was that, given the level of uncertainty in the data, the two techniques performed equally well in terms of reducing model-data misfits.

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p. 4213: I had to read the third sentence a few times before I understood what was being said here. To better understand (and believe) this statement, I would need to know the uncertainties associated with the fitness values, and with the parameter values. How different do two fitness values (and similarly, two parameter values) have to be in order that they are 'significantly different'?

p. 4213, line 11: How do the results look compared to the pre-assimilation simulations? Can the a priori results be plotted on Figure 4 and 5 as well?

p. 4213, line 17: 'is' should be 'has'

p. 4215, line 19: 'mixed layer depth' should be simply 'mixed layer'

p. 4216, line 12: 'Note this not' should be 'Note that this is not'

p. 4215, line 21: Is this statement referring to surface concentrations only I assume? This must be stated. How, quantitatively, do we know that the nutrient, phytoplankton and detritus are constrained? It does not seem that this could possibly be the case below the mixed layer.

Table 2: Why is RMSE used here, and not the cost that was minimized? How about including fewer significant figures here? Why does the number of data points in parentheses depend on which IOP model is used?

Table 3: Too many significant figures

Figure 3: Axes are not labeled.

Figure 4/5: A different color scheme might make these lines easier to distinguish. I don't think the line connecting the circles (observations) is necessary. I would also like to see the a priori result (no assimilation.)

Figure 6: Lines need to be defined in caption since there is no legend on the plots.

Figure 8: It would be easier to see the differences if all three plots had the same y-axis

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range.

References:

Friedrichs, M.A.M., J. Dusenberry, L. Anderson, R. Armstrong, F. Chai, J. Christian, S.C. Doney, J. Dunne, M. Fujii, R. Hood, D. McGillicuddy, K. Moore, M. Schartau, Y. H. Spitz, J. Wiggert, 2007. Assessment of skill and portability in regional marine biogeochemical models: role of multiple phytoplankton groups. Journal of Geophysical Research, 112, CO8001, doi: 10.1029/2006JC003852.

Friedrichs, M.A.M., R.R. Hood, and J.D. Wiggert, 2006: Ecosystem model complexity versus physical forcing: Quantification of their relative impact with assimilated Arabian Sea data. Deep-Sea Research II, 53, 576-600.

Ward, B., M.A.M. Friedrichs, T.R. Anderson, and A. Oschlies, 2009. Parameter optimization techniques and the problem of underdetermination in marine biogeochemical models. Accepted for publication in the Journal of Marine Systems, May 2009. (Available upon request)

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