**Biogeosciences
Discussions**

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

Anonymous Referee #2

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The manuscript describes an attempt to use satellite data to constrain a simple marine ecosystem model. This is a scientifically interesting problem that is of wide interest to readers of Biogeosciences.

The ecosystem model is applied at three sites in the North Atlantic, and a number (11) of the model parameters are optimized using a genetic algorithm. The main result is that satellite chlorophyll and backscatter data are not sufficient to fully constrain the model. Given a number of previous studies using more and different data, this is not surprising. The author further claims that the data are sufficient to constrain the simulated nutrient, phytoplankton and detritus fields (at two sites). I doubt whether this

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is correct and I cannot see that this is supported by the results shown. First, only a few of the model parameters are optimized, and from the material shown it cannot be ruled out that other model parameters do not effectively control the simulated fields. Second, the cost function seems to be either essentially insensitive to many of the parameters optimized (flat curves in Figure 6), or the optimal parameters are consistently at the upper/lower bounds of the available parameter space. From looking at Figure 6 I can hardly find a single parameter that has a well-defined cost-function minimum well within the allowed parameter range!

I don't think the discussion about carbon uptake and export is really needed, nor is it very helpful. It might be skipped altogether.

A major deficiency of the paper is the complete lack of any discussion about error estimates and uncertainties of the "optimized" parameters. Figure 6 contains a lot of information, which could be exploited more carefully. Though I cannot comment much on the satellite algorithms, which is completely outside my expertise, I found the description relatively difficult to follow: There were many missing units and definitions, sometimes it was not clear why so much detail (many wavelengths) was needed (see specific comments below).

I think that for publication in Biogeosciences, the paper needs a substantial revision, in particular a thorough analysis of the parameter errors after "optimization". Also, more details about the model spin-up (physics and biogeochemistry) are required. From figures 4 and 7 it is obvious that the model does not produce a periodic seasonal cycle. This might to some extent be explained by aperiodic real forcing, but I doubt that this can explain the large trends simulated at ESTOC.

specific comments:

p.4202, l.16: why are only 3 out of 4 algorithms used? Either explain or remove this somewhat confusing statement from the abstract.

p.4202, l.26: a wide range of much shorter time scales also exist (daily, seasonal, annual, decadal) for large water volumes. Hundreds of years is near the upper end of the spectrum.

p.4303, l.1/2: not correct: export production is not normally defined as carbon "removed from the system" (from what system? certainly not from the earth system, perhaps OK for the ocean system), or export to the sea-bed (this would depend to a large extent on water depth).

p.4204, l.3: what is "IOP a" ? Only IOP seems to be defined much later in the manuscript.

p.4204,l.15: mention that you use real data in contrast to the (simpler) case of simulated data employed by some other studies.

p.4205,l.6: "normalized" to what?

p.4206,l.1: why is b_bw independent of lambda?

p.4206,l.7 which Chl units are used?

p.4206,l9-12: why do you need b_bp at so many wavelengths? Further above you say that you only use $b\_bp(490nm)$.

p.4207,l.11: The Cloern et al. formulation is not "physically-based" - it is only an empirical fit (using physical AND biogeochemical properties).

p.4207, l.19: The units given in Table 1 for gamma are inconsistent with the remineralization rate.

p.4209,l.3 "growth saturation parameter" is usually referred to as "half saturation parameter".

p.4209ff, eq.9,10,11, p.4210,l.1: Units?

p.4210,l.15 : why is the analysis restricted to the range $0-z\_90$?

p.4210,l.20: Scott et al. (2008) is a submitted paper not accessible yet. I therefore cannot review whether the choice is adequate.

p.4211,l.10ff: Presumably, the optimization is done simultaneously at the three sites? Should this be reflected by an additional sum in the cost function? Is sigma the variance over the entire time period over all sites? Please specify!

p.4213,l.4: How do you know that the GA has achieved "optimum" fitness?

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