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Discussions

# *Interactive comment on* "Capturing optically important constituents and properties in a marine biogeochemical and ecosystem model" *by* S. Dutkiewicz et al.

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Review of "Capturing optically important constituents and properties in a marine biogeochemical and ecosystem model" by Dutkiewicz et al.

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This paper focus on the modification of the MIT-gcm model to explicitly include optics. The authors show the output of global simulation showing the ability of the model to provide qualitatively realistic results. They then do a series of sensitivity runs where specific optically important components are varied and observe their impact on the global fields.



The paper is well written and concise. I am in favor of publishing this paper as it describes an important modification of the model which will open a variety of avenues for research with this model in future studies.

I have some significant comments that I feel, if addressed, could improve this paper. Significant comments: 1. The global runs with the explicit model were not compared to run when optics was not explicit? Why not? The community needs to know if adding optics is important in general (e.g. to obtain the appropriate biogeography, nutrient fields etc') or not? Is it worth the increased computation costs? Does it help to better constrain the model's parameters by having more data to compare to (e.g. Fujii et al).? W/o that I don't see the use of the initial run. Until now you have published papers on BGC and species distribution where the optical model used was even simpler. Were their results (distribution, timing etc') systematically wrong in ways that the optics has now fixed? 2. Qualitative comparison should be performed (e.g. mean % or absolute deviations etc') , and not just computation of correlation coefficient. The later is strongly affected by dynamic range. 3. The limitations of the current model need to be spelled out in a dedicated paragraph in the method section. E.g.: neglecting PIC and minerals, neglecting the group specific changes of absorption coefficient with light and nutrients (you model the changes in chl/C but not the ensuing modulation of the absorption spectrum). Fixed parameters for CDOM and NAP rather than varying them. You ignore inelastic scattering (e.g. Raman, Chlorophyll and CDOM). Raman has been found to be important for chl<1mg mˆ-3, particularly in oligotrophic environments, where it would increase the availability of blue and green light. You assume a fixed ratio of photoprotective to photosynthetic pigments (which, in nature, varies with light and nutrients). You are ignoring non-phtosynthetic bacteria as having optical properties. You neglect effects of sea surface on light entering/leaving the ocean. 4. The differences between using a 3stream model compared to using a full RT model need to be quantified or cited from other studies. The 3 stream model is an approximation and one would like to know the likely biases associated with using it (ignoring the full RT calculations). The full RT is the constituent equation in optics and models to solve it exist (e.g. Hydrolight).

While you will always have to assume thing (e.g. sky model), what you neglect by doing approximations needs to and can be quantified. 5. You are missing a large historical body of literature that should be cited, as it specifically addresses the role and nature of the constituents you are focusing on. E.g. the works of Jerlov, Kale, and Bricaud and Stramski 1981 for CDOM and its parameterization. Many works comparing the relative absorption of different constituents have been published. I can think of works by Chang, Arnone, Barnard and Roesler among other. Arrigo has published on the effects of CDOM on phytoplankton (again, among others). Morel, 1988, has looked at the effect of H2O on PAR. There are many studies that have been conducted showing that phytoplankton either photo-acclimate or are selected for the light field they experience (e.g. Moore and Chilsolm). Models capturing the chlorophyll max dynamics have also been published (e.g. Taylor et al., Fennel, Wang). I can't think of anything new that I learned from your paper about the role of optical constituents in the ocean, how they are affected by light or how they modulate the light field and reflectance. 6. There exist more comprehensive optical data from AMT that has already been published (e.g. by Dall'Olmo, Martinez-Vicente). Why not use it? If I understand correctly that you are modeling an 'average' year (not a specific year), you could aggregate all the data. 7. It will be very interesting if you could show the species succession in the spring in key locations (e.g NABE) and whether light and/or nutrients are the culprits (and whether the more explicit model is needed compared to the previous one). I am not aware that this question has been ever studied in a model framework.

Minor comments: 1. Title: I think that 'Modeling' rather than 'Capturing' will better describe the content of the paper. 2. Abstract: Qualify what you mean by 'important' in your abstract. It seems it is related to domination of the absorption coefficient. 3. Abstract: Line 23: Eu/Ed is referred to as the 'irradiance reflectance' not the reflectance of the irradiance. 4. What is the time step of the model? 5. 2.3.2.: Rather than detritus or detrital matter, the ocean optics community now uses the term non-algal particles which is a much better terms (does not assume anything about these particles). Notice that given our methods, cell wall materials and cytoplasm are counted as NAP.



Bacteria and viruses are also NAP. 6. 2.3.2.: It is not clear why you have to define a 'detrital material' particle. You can refer to it as a pool of carbon with specific absorption and scattering w/o having to define such 'idealized' particle. 7. 2.3.3 A CDOM spectral slope of 0.02nmˆ-1 is rather high. 0.0145nmˆ-1 is more representative (studies by Babin, Roesler, Bricaud, and Carder among other). Specific values are also method dependent, e.g. what spectral range and what fit method is used (e.g. Twardowski et al., 2004). The specific value you use (0.02061nmˆ-1) contains at least 2 insignificant digits 8. Equ. 20 is not clear to me (unitwise). A. Maximum quantum yield of absorption is 0.4 (I assume unitless) – what is this representing? If units of aˆchl_ps,j are mˆ2/mg Chl and Eo mol quanta per nm the units of \Lambda_E,j, integrated over wavelength, will be quanta mˆ2 per mg chl. 9. Nowhere do explain the use of mmolP (I assume phosphate is the maine currency of your model). – e.g. Table 1. Why not keep everything to mmolC (as you assumed Redfield).? 10. Sec. 3.1/3.2 . Is the realism observed different from when you did not used a sophisticated optical model? 11. 3.3 numerical domination by picoplankton is well known. Do they dominate a_ph (they usually do not)? 12. 12 p.2625 l. 4. Could you use HPLC to estimate the larger phyto? Could you use other AMT cruises where such data is available? 13. Variability in Chl/CDOM has been reported in Bricaud and Morel 1981. 14. Fujji is Fujii (several instances throughout). 15. Discussion: your treatment of light, while more comprehensive in species, is less comprehensive in RT (e.g. compared to Hydro or EcoLight). Question is always: are the advantages of being comprehensive important and worth the computational cost. I don't think you answered this important question in this version of your manuscript. 16. Note that while Stramski's data base include measured optical data, certain optical parameters are based on simulations with Mie theory (homogeneous spheres). It is known that shape and internal structure will increase backscattering compared to spheres (e.g. Stramski's 2004 review on backscattering).

Dear authors, I am often wrong. If you feel I have misunderstood the paper and that comments are off base or not clear, feel free to contact me directly. –Best, Emmanuel

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C1435/2015/bgd-12-C1435-2015-supplement.pdf

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