Interactive comment on "Capturing optically important constituents and properties in a marine biogeochemical and ecosystem model"

by S. Dutkiewicz et al.

Interactive comment on Biogeosciences Discuss., 12, 2607-2695.

Response to Reviewer 1 (Emmanuel Boss):

Reviewer's comments are in black, our replies are in blue. In some of the quoted text, some latex symbols are included (apologies as this makes them a bit more difficult to read).

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.

We thank the reviewer for the positive comments, and we do feel that this manuscript will provide an important foundation for 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?

Previous models (ours or others) are not necessarily "worse", but are designed and used to ask different questions. Our intent in adding the complexity was to have a tool to explore questions on links between ecosystems and optics that required these further refinements. We also did

not initially include analysis on this because we felt that many studies, including Fujii et al (2007), already set out the value of adding optics to biogeochemical models (a point that we now make in the introduction of the revised version of the text, see below). However, we appreciate the suggestion that such analysis would be informative in the current paper, and have consequently added a new section just after the model validation (new section 4) to the revised version with a series of experiments, specifically asking the question of how important this new level of complexity is to the results and compares results to an older version of the model without the explicit spectral radiative transfer component. It is instructive to see that non-radiative transfer, non-optical models can get many of the features similar and some not. The results were interesting and we have made much of them in the abstract, discussion and conclusion; and thank the reviewer for the suggestion of including this topic.

We do, however, maintain that the main purpose of the model development was not to create a 'better' model per se, but to address questions of phytoplankton assemblages that required consideration of spectral optical properties as well as a closer connection to satellite products, such as reflectance, than was previously resolved. We make this point clear in both the abstract, introduction and discussion.

Abstract (old pg 2609, replacing line 24-28): "*This new model that captures bio-optical feedbacks will be important for improving our understanding of the role of light and optical constituents on ocean biogeochemistry, especially in a~changing environment. Resolving surface upwelling irradiance will make is easier to connect to satellite derived products in the future.*"

We also add an extra sentence to the abstract specifically acknowledging what the new model capture relative to old model:
"*We find that incorporating the different optically important constituents and spectral irradiance was crucial to capture the regionally varying depth of the subsurface Chl-a maximum.*"

Introduction (pg 2611, after lines 4): "*Fujii et al. (2007) suggested that including explicit optics in an ecosystem model allowed a more accurate subsurface light field and allowed additional constraints on model parameters. Several additional studies have demonstrated the value of adding optics to biogeochemical models (e.g. Babin et al, 1993; Sathyendranath and Platt, 2007; Kettle and Merchant, 2008).*"

Discussion (near old pg 2634, line 26): "*The model developments presented were necessary for capturing the regional variability in depth of the subsurface chlorophyll maximum, in particular, by resolving the deep penetration of blue-green wavelengths in the subtropical gyres. Not including any of the constituents leads to an unrealistically regionally uniform depth of the deep chlorophyll maximum.*"

Conclusions (old text, pg 2636, line 1):

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"Capturing each of the optically important constituents explicitly, and including a spectrum of light was important for obtaining realistic variability in depth of the subsurface chlorophyll maximum, and in resolving the deep penetration of blue-green wavelengths in the subtropical gyres important for phytoplankton community structure."

New section 4 is as follows and includes an additional figure (new Fig 14):
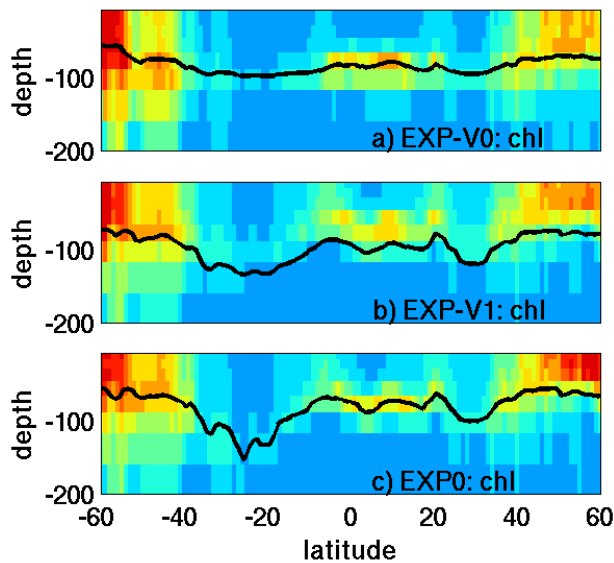
"4. Sensitivity experiments: Value of added model complexity.

We conduct two sensitivity experiments to highlight the importance of the extra level of complexities of this new version of the model. In the first experiment (designated EXP-V0) the biogeochemistry and ecosystem are the same as in the default experiment described above (designated EXP0) but there is only a single band of irradiation (400-700nm, summed over the original 25nm, so that total PAR is conserved); attenuation (c_{tot}) of PAR is a function only of absorption by water molecules and Chl-a summed over all phytoplankton types: $c_{tot}=a_{wo}+a_{chlo} Chl_{tot}$, where $a_{wo}=0.04$ m^{-1}, and $a_{chlo}=0.04$ m^2 (mg Chl)$^{-1}$. There is no explicit account taken for optical role of CDOM or detritus (though the value chosen for a_{chlo} implicitly include their role). Similar parameterizations have been used in previous versions of our model (e.g. Dutkiewicz et al., 2014), and are also common in many other biogeochemical models.

The results from EXP-V0 (Fig. 14a) reveals a much more latitudinally uniform penetration of light, and in particular the deep chlorophyll maximum in the subtropical gyre is too shallow relative to the default experiment (EXP0, Fig. 14c) and observations (Fig. 3a).

In experiment EXP-V1 we include all the optical constituents explicitly (as in EXP0), though with only a single band of PAR (as in EXP-V0). We assume the absorption and scattering coefficients for 500nm in this experiment. This experiment (Fig. 14 b) reveals substantial more realistic varying distribution of the deep chlorophyll maximum and penetration of PAR. The addition of spectral light leads to even deeper penetration of light in the subtropical gyres (default experiment, EXP0, Fig. 14c): deepest penetrating light is in the blue/green range and an average absorption across one waveband will not capture these differences.

These sensitivity experiments suggest that explicitly capturing regional changes in all optical constituents is essential for the realistic light penetration variations. Spectral light further enhances the realism of the results. The addition of the radiative transfer code is essential for obtaining upwelling irradiance that can link to satellite products."

"Figure 14: Sensitivity Experiments examining value of increased optical complexity in model. Chl-a (\unit{mg\,C\,m^{-3}}) along the extended AMT-15 transect for (a) {{EXP-V0}} with no radiative transfer, single waveband of PAR (400-700nm), no inclusion of optical effects of CDOM or detritus and no optical differences between phytoplankton. (b) {{EXP-V1}} with radiative transfer, explicit optical properties for CDOM and detritus, but only one waveband (400-700nm) and no optical differences between phytoplankton. (c) {{EXP0}}, the default experiment. Model 1% irradiance depth is shown as a black line."

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.

We now include the model bias in Figure 6 (shown below). We alter the text (pg 2622, line 18-21):
"The model also captures many of the global features in Chl~a (derived from MODIS satellite), primary production (derived using Behrenfeld and Falkowski, 1997) as well as macronutrients (from the World Ocean Atlas, Garcia et~al., 2006), though with notable biases (Fig. 6)."

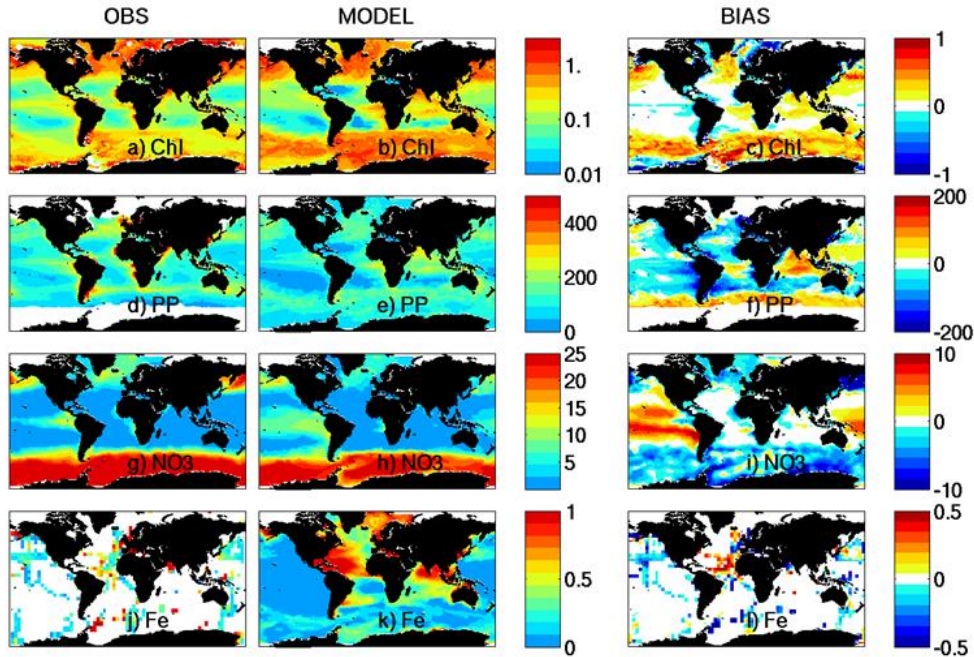The subsequent text in that section sums up the biases.

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Figure 6. Model and satellite derived products and climatologies of in~situ measurements for annual mean and biases: \textbf{(a)}~satellite derived (MODIS) Chl~a (\unit{mg\,Chl\,m^{-2}}); \textbf{(b)}~modelled Chl~a (mean 0--50\,\unit{m}, \unit{mg\,Chl\,m^{-2}}); \textbf{(c)} bias of Chl~a (model-observations); \textbf{(d)}~satellite derived primary production (\unit{g\,C\,m^{-2}\,yr^{-1}}) (Behrenfeld and Falkowski, 1997); \textbf{(e)}~modelled primary production (column integrated, \unit{g\,C\,m^{-2}\,yr^{-1}}); \textbf{(f)} bias of primary production; \textbf{(g)}~World Ocean Atlas nitrate (mean 0--50\,\unit{m}, \unit{mmol\,m^{-3}}) (Garcia et~al., 2006); \textbf{(h)}~modelled nitrate (mean 0--50\,\unit{m}, \unit{mmol\,m^{-3}}); \textbf{(i)} bias of nitrate; \textbf{(j)}~compiled iron observations (composite 0--50\,\unit{m}, nM) (Tagliubue et~al., 2012); \textbf{(k)}~modelled iron (mean 0--50\,\unit{m}, nM); \textbf{(l)} bias in iron.

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 photopro-
tective 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.

We had mentioned several of these limitations in the older version (see for instance paragraph in the Discussion, pg 2634, lines 9-14). Given the reviewer's suggestion we now add a sub section in the Model description (new section 2.6) that includes those we already mention as well as additional points that the reviewer brings up. We note however that: CDOM and NAP vary in concentration in time and space in the model such that their effects do vary, though agree that the spectral shape does not vary. We had already made mention that NAP spectral qualities do not vary as they should (especially with size), but again make more of this in the new subsection. The OASIM model does take into account of the effect of sea surface on light entering the ocean. We make this clearer in the revised version (pg2612, line 25):
"OASIM includes the impact of clouds, water vapour and aerosols in the atmosphere and surface roughness and reflectance at the ocean-atmosphere interface."

Our model provides upwelling light just below the surface – thus we do not take into account the effect of sea surface on this model output. We add this limitation (as well as others already mentioned) in the new section as well.

New Section 2.6 reads:
"The inclusion of radiative transfer, spectral light and capturing several important optical constituents has been a significant development of the model. However, this version of the model is not without limitations. One major, though currently necessary simplification, is to assume constant absorption and scattering spectra (Fig. 1) for each constituent. For instance, absorption spectra for phytoplankton types do in reality change based on shifts in Chl:C ratios (MacIntyre et al., 2002; Morel et al., 1993; 1995) as well as changes in ratios of photoprotective to photosynthesis pigments as a result of light, temperature and nutrient stress (e.g. Stramski et al., 2002). However, these changes are likely to be small compared to the differences already captured by the representative spectra and photoacclimation component and there is not, as yet, enough systematic observations of these alterations to constrain model parameterisations. Additionally the CDOM absorption spectra has been observed to alter regionally (e.g. Kitidis et al., 2006; Twardowski et al, 2004; Bricuad et al 2010), though as yet we feel it is premature to attempt to capture this variability in the model parameterizations.

Scattering, particularly by detrital particles, remains the least well developed aspect of the model. In particular, we neglect variations in detrital particle size distributions which is likely to be important (Stramski et al, 2001). Additionally the spectra for b^{part}_{det} that we use (Stramksi et al., 2001, Fig. 1b) makes the assumption of homogeneous spheres. However it is likely that differences in shapes and internal structure of the particles will be important for altering the spectral shape (Stramski et al., 2004). We also do not take into account inelastic scattering which may be important for blue and green light in oligotrophic regions (e.g. Ge et al., 1993).

We additionally currently neglect other potentially important optical constituents such as minerals (e.g. Stramski et~al., 2001), particulate inorganic carbon (e.g. Balch and Itgoff, 2009), colloids and bubbles (e.g. Stramski et al., 2004), non-photosynthetic organism including

zooplankton, bacteria (e.g. Morel and Ahn, 1991), and viruses (e.g. Stramski et al., 2001). We felt that these are, as yet, not well enough constrained to include explicitly in the model.

The limitations list above should however not detract from the major enhancement to the model and are similar to those of other models (e.g. Fujii et al., 2007; Gregg and Casey, 2007). This new model provides a unique platform to examine global implication of optical properties to the phytoplankton ecosystem, feedbacks to the biogeochemistry, and links to satellite data that are not possible with limited observational data. Here we first validate the model in a standard "default" configuration. We then provide a series of studies exploring the significance of each of the optical constituents and our parameterization. Several studies in progress build on for these results."

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.

The three stream model is indeed an irradiance model, not a radiance model: there is no angular dependence. In order to compare our results to hydrolight, we would need to make assumptions about the angular dependences of each of our optical constituents. This would lead to additional uncertainties and we believe that we would not gain much insight. We believe that it is more appropriate to compare to real data. We have in particular tried to compare against a detailed optical dataset to validate this model. In particular Fig 5 shows that we capture the spectral distribution of the depth of light penetration as found along the AMT-15. Adding a full radiance model would be a significant computational expense. The simplification of Mobley et al (2009) provide a justification for the simplifications we have undertaken. But we also note that these simplifications were also used by Ackelson et al (1994) and Gregg et al. (2007, 2009)

To provide the reader with sufficient background to understand that we have made specific assumptions on RT component we add in the revised version at the end of section 2.1:
"We note that radiative transfer component is a simplification from a full radiance model, and in particular does not resolve the angular distribution of light, nor angular dependence of scattering. These assumptions have been shown to be small in terms of the needs for ecosystem models in Mobley et al (2009). Though not a full radiative transfer model, our three-stream treatment does provides the relevant output for our needs: the light available for photosynthesis and an upwelling component that at the sea surface is similar to that seen by a satellite."

5. You are missing a large historical

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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.

We agree that we did not have enough references to previous studies when discussion the sensitivity studies (though had attempted to mention several in the introduction). We now remedy this as described below. However we disagree that there is "nothing" new in this paper. In particular the feedback from the optics to the productivity to the gyre size is (we believe) new and is a main conclusion of these studies. Also, though much of the different aspects of the constituents are known (and have a large literature behind them), we believe that our approach provides a level of synthetizing of prior knowledge (otherwise quite disparate) in a model that can explore the global impact of feedbacks. Yes, much of the mechanistic understanding is already known (we're not claiming to invent anything new in that regard), but 1) some of these issues may not be transparent to wider biogeochemical (modeling) community, 2) they haven't been brought together in a model such as this before, 3) many of these previous studies are regional observations and models, here we show how mechanistic understanding plays out globally (which in our opinion is novel). However we do now place these results in context of many previous results:

Pg 2626, Section 4 first paragraph (in new version this is Section 5) now reads:
"*Optical constituents play varying roles in their effect on irradiance attenuation (absorption and scattering). These roles have long been a topic of interest, though many studies have had limited observations and been of highly localized in character (e.g. Jerlov, 1953; Chang and Dickey, 1999) though it has also been recognized that they vary regionally (e.g. Barnard et al., 1998; Simeon et al., 2003; Zheng and Stramski, 2013). Targeted cruises have also provided larger scale observations indicating large range of value for each constituent and altering importance in different regions (e.g. BIOSOPE, Bricaud et al 2010) and several attempts have been made to construct algorithms to determine the relative contributions from more easily measured quantities, including those from satellite (e.g. Maritorena et al., 2002; Lee et al., 2002; 2007; Ciotti and Bricaud, 2006; Werdell et al., 2013; Zheng and Stramski, 2013). Our model provides a unique global 3-dimensional perspective. Here our results focus on an (extended) AMT transect (Figs. 15 and 16), however, they are also consistent with observations in other regions (e.g. Bricaud et al. 2010).*

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Absorption by water molecules is most important at longer wavebands (Pope and Fry, 1997), but still has an impact at shorter wavebands (Fig 15a,~b,~i,~j). It is relatively more important in lower productive waters (e.g. South Atlantic gyre) because the concentrations of other constituents are relatively low . Absorption by detrital matter plays a~role, especially near the 1% depth in highly productive regions and at shorter wavebands (Fig 15 c,~d,~i,~j). Absorption by phytoplankton plays a~significant role where Chl~a is highest (e.g. the deep Chl~a maximum as suggested by observations, e.g. Chang and Dickey, 1999) at wavelengths less than $550\,\unit{nm}$, and little role at longer wavelength (Fig 15 g,~h,~i,~j, see also Fig. 1). Absorption by CDOM at short wavebands is important (as seen in observations e.g. Jerlov, 1953) in most regions, particularly where productivity is high where it is the dominant absorber. It also has, relative to other constituents, a~large role at depth (as seen in observations e.g. Simeon et al., 2003; Bricaud et al., 2010). At long wavebands CDOM plays very little role. Scattering by phytoplankton is relatively most important at shallower depths, while scattering by detrital matter is dominant deeper at all wavelengths (Fig. 16).”
(Note Fig 15 and 16 refer to old figures 14 and 15).

We start Section 5.1. (old text pg 2627, line 14, old section 4.1) with:
“Observations have determined that detrital matter does play a role in light attenuation, though with varying regional importance (e.g. Jerlov 1953; Bricaud et al., 2010)”

And in this same section when discussing the change in community structure (old text, pg 2628, line 13) we add reference to Moore et al (1995):
“This favours phytoplankton, at least in the subtropics, which absorb more efficiently in the blue part of the spectrum (i.e. \textit{Prochlorococcus}, Fig 16c) as anticipated from laboratory studies (e.g. Moore et al., 1995)”

We start Section 5.2 (Old text, pg 2630, line 26, old section 4.2) with the following new text:
“CDOM and its contribution to light absorption is observed to vary in different regions of the ocean (e.g. Jerlov 1953, Bricuad 1981, Nelson and Seigel, Morel et al., 2010) and many studies have attempted to empirically link a_{cdom} to other more easily measured quantities such as Chl-a (e.g. Morel, 2009). However these studies are still regional or include only sparse data. We conduct a~series of sensitivity experiments that test assumption and importance of a_{cdom} globally and its feedback to the biogeochemistry.”

Also in this section we modify pg 2631, lines 15-17 to:
“In the parameterizations that either tie χ_{cdom} to Chl~a ({{EXP-C3}}) or to DOM ({{EXP-C4}}), a_{cdom} is almost non-existent below the $1\,\{\%\}$ light level (Fig. 17), at odds with observations (e.g. Simeon et al., 2003; Bricaud et al., 2010).”

And we also include an additional sentence (old text, pg 2631, at line 9):
“The model experiments thus reveal a potentially important role for CDOM in setting phytoplankton community structure via alteration of the visible light spectrum, building on previous studies (e.g. Arrigo and Brown 1996).”

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We already included (Introduction, pg 2610, lines 17-18) several references on the issue of selection of phytoplankton by light environment (Bidigare et~al., 1990a; Huisman and Weissing, 1995; Stomp et~al., 2004; Hickman et~al., 2010). We add here the Moore et al references as well. However, we now also include this list of papers in section 5.3 (old section 4.3, sensitivity studies with a_phy and b_phy):

"Community structure is also altered (Fig. 19c) showing that the photosynthetetic absorption specific to each type is important for the emergent biogeography as has been suggested by previous studies (Bidigare et~al., 1990a; Huisman and Weissing, 1995; Moore et al, 1995; Stomp et~al., 2004; Hickman et~al., 2010)."

We include reference to Fennel and Boss (2003), Wang et al (2009) in the Discussion (old text pg 2634, line 26):

"The subsurface chlorophyll maximum can indeed be captured without including all constituents and spectral light (as seen in EXP-V0, and in other models, e.g. Fennel and Boss, 2003; Wang et al., 2009). However, the model developments presented were necessary for capturing the regional variability in depth of the subsurface chlorophyll maximum, in particular, by resolving the deep penetration of blue-green wavelengths in the subtropical gyres."

We note that Taylor et al (1997), Fennel and Boss (2003), Wang et al (2009) did not resolve spectral nature of light at the DCM. We look at this issue further in a paper in preparation (Hickman et al).

We now cite the following additional papers on the above subjects:
Arrigo, K.R. and Brown, C.W.: Impact of chromophoric dissolved organic matter on UV inhibition of primary productivity in the sea Mar. Ecol. Prog. Series, 140, 207-216, 1996.

Barnard, A.H., Pegua, W.S., and Zaneveld, J.R.V.: Global relationships of the inherent optical properties of the oceans, J. Geophys. Res., 103, 24,955--24,968.

Bricaud, A., Babin, M., Claustre, H., Ras, J., and Tieche, F., Light absorption properties and absorption budget of the Southeast Pacific waters. J. Geophys. Res., 115, C08009, doi:10.1029/2009JC005517, 2010.

Bricaud, A., Morel, A., and Prieur, L: Absorption by dissolved organic matter of the sea (yellow substance) in the UV and visible domains, Limnol. Oceanogr., 26, 43-53, 1981.

Chang, G.C. and Dickey, T.D.: Partitioning in situ total spectral absorption by use of moored spectral absorption-attenuation meters, Applied Optics, 38, 3876--3887.

Ciotti, A. M., and Bricaud, A.: Retrievals of a size parameter for phytoplankton and spectral light absorption by colored detrital matter from water-leaving radiances at SeaWiFS channels in a continental shelf region off Brazil, Limnol. Oceanogr. Methods, 4, 237–253, 2006.

Fennel, K. and Boss, E.: Subsurface maxima of phytoplankton and chlorophyll: Steady-state solutions from a simple model, Limnol. Oceanogr., 48, 1521-1534, 2003.

Jerlov, N.G.: Influence of suspended and dissolved matter on the transparency of sea water, Tellus, 5, 59--65, 1953.

Maritorena, S., Siegel, D.A., and Peterson, A.R.: Optimization of a semianalytical ocean color model for global-scale applications, Applied Optics, 41, 2705--2714, 2002.

Moore,~L.~R., Goericke,~R., and Chisholm,~S.~W.: Comparative physiology of^M \textit{Synechococcus} and \textit{Prochlorococcus}: influence of light and^M temperature on growth, pigments, fluorescence and absorptive properties, Mar.^M Ecol.-Prog. Ser., 116, 259--275, 1995

Morel,~A., and Ahn,~Y.-H.: Optics of heterotrophic nanoflagellates and ciliates: A tentative assessment of their scattering role in oceanic waters compared to those of bacterial and algal cells, J. Mar. Res., 49, 177--202, 1991

Simeon, J., Roesler, C., Pegau, W.S., and Dupouy, C.: Sources of spatial variability in light absorbing components along an equatorial transect from 165°E to 150°W, J. Geophys. Res., 108, doi:10.1029/2002JC001613, 2003.

Wang, X.J., Behrenfeld, M., Le Borgne., R., Murtugudde, R., and Boss, E.: Regulation of phytoplankton carbon to chlorophyll ratio by light, nutrients and temperature in the Equatorial Pacific Ocean: a basin-scale model. Biogeosciences, 6, 391-404, 2009.

Werdell, P.J., Franz, B.A., Bailey, S.W., Feldman, G.C., Boss, E., Brando, V.E., Dowell, M., Hirata, T., Lavender, S.J., Lee, Z., Loisel, H., Maritorena, S., Mélin, F., Moore, T.S., Smyth, T.J., Antoine, D., Devred, E., d'Andon, O.H.F. and Mangin, A.: Generalized ocean color inversion model for retrieving marine inherent optical properties, Applied Optics, 52, 2019--2037.

Zheng, G., and Stramski, D.: A model based on stacked-constraints approach for partitioning the light absorption coefficient of seawater into phytoplankton and non-phytoplankton components. J. Geophys. Res, 118, 2155–2174, doi:10.1002/jgrc.20115, 2013

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.

The AMT-15 had a particularly diverse and relevant set of optical measurements. In particular the light penetration data used in Fig 5 has been very useful in model development. We

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decided to stick to just one cruise to avoid interannual variability and the differences in cruise tracks which we believe would have distracted from the main points that we want to make. We believe that adding additional cruise data would not have provided sufficiently in the validation to justify sacrificing clarity of the manuscript. We acknowledge that Martinez-Vicente et al (2013) do have additional scattering data (e.g. total backscattering), but that cruise lacked other datasets (e.g. phytoplankton light absorption measurements). We now reference that there are other AMT cruises but explain why we stick to just this one cruise. We add this just after old text pg 2620 line 25

"Though there are other AMT cruises that include some similar and/or different combinations of optical data (e.g. AMT-19, Dall'Olmo et al., 2012, Martinez-Vicente et al., 2013), we chose to look at only a single transect for clarity. In particular, the combination of data on spectral irradiance penetration, a_{CDOM}, and light absorption by phytoplankton were of particular use in model validation."

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.

We agree that this would be a very interesting study, and in fact we are working on two manuscripts that explore the relative role of nutrients and light on controlling growth. One focusing on the spring bloom at high latitudes, and another considering these controls both at the surface and the DCM. However this is beyond the scope of this paper. We do provide a figure here on the species succession for the reviewer's benefit. We decided however that we have too many figures in the text as it is, and so do not include it in the paper.
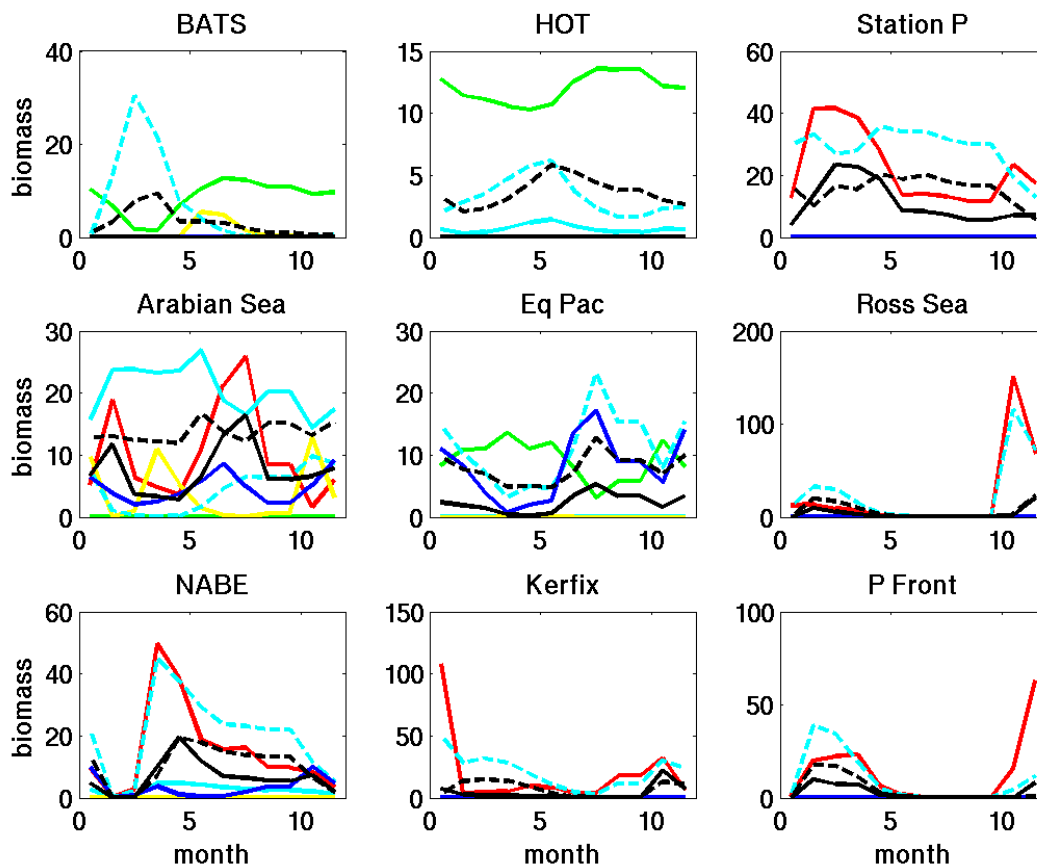
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Figure A: Biomass (mgC/m3) of plankton types at the JGOFS stations (locations shown in Figure 1 of the BGD paper). Colours indicate different plankton types: red(diatoms), dark blue (cocco), yellow (diaz), dashed light blue (pico euks), solid light blue (Syn), green (Pro), dashed black (small zooplankton), solid black (large zooplankton). Total Chl is compared to satellite and in situ in Fig 8 of the BGD paper.

Minor comments:

1. Title: I think that 'Modeling' rather than 'Capturing' will better describe the content of the paper.

The title already includes the word "model" – we believe that the goal of the paper is to "capture" the optical properties in a model. As such we believe the current title is more relevant.

2. Abstract: Qualify what you mean by 'important' in your abstract. It seems it is related to domination of the absorption coefficient.

Good point. We now include this in the abstract (in place of old sentence pg 2609 lines 15 onward):

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"CDOM has proportionally more importance at attenuating light at short wavelengths and in more productive waters, phytoplankton absorption is especially important at attenuation at the deep chlorophyll~a (Chl~a) maximum, and absorption by water molecules is relatively most important in the highly oligotrophic gyres."

3. Abstract: Line 23: Eu/Ed is referred to as the 'irradiance reflectance' not the reflectance of the irradiance.

In the abstract we used the words "sea surface reflectance", but did find other occasions where indeed we said "reflectance of irradiance" (e.g. pg 2611, line23). We endeavor throughout the manuscript to now use the term "irradiance reflectance" or simply "reflectance" where this is obvious.

4. What is the time step of the model?

3 hour. We tested this against a 1 hour timestep. We add this detail to the revised version of the paper in the "Simulation design" subsection (old 2.6, now 2.7):
"The model timestep is 3 hours. We tested this against smaller timesteps with almost identical results."

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.

This is a tricky issue. The numerical modeling community use "detritus". We also suggest that since we are not capturing bacteria or viruses in the model that it might be incorrect to use NAP. We also note that Stramski used "detritus" in his 2001 paper (where we get the spectra from for this constituent). In the revised version we elected to keep the word "detritus" as this links directly to our POM pool (which would include cell walls and cytoplasm of dead organic matter). However we make the distinction and the link to the optics community terminology in the revised text. Appended below (old) pg 2615, line 22 we add:
"We note that in the optical community the term "non-algal particles" or NAP is frequently used for any non phytoplankton particles. In this paper we specifically use the term "detritus" instead, as we link to the non-living organic matter pool and do not include other non-algal particles such as viruses and heterotrophic bacteria"

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.

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We define a detrital material particle size spectrum (not a single particle) so as to convert from our POM pool (in terms of concentration) to particles so as to use the spectrum from Stramski et al 2001. We are clearer on this point in the revised version (added to pg 2615, line 12-14): *"These spectra were deduced by assuming an assemblage of particles with size distribution described by a power function with slope of -4, and the values are given in terms of absorption or scattering per particle. Thus we introduce the coefficient p_part to convert the model particulate organic carbon (POC) to number of particles, making the crude assumption that the size distribution of particles is uniform everywhere."*

7. 2.3.3 A CDOM spectral slope of $0.02nm^{-1}$ is rather high. $0.0145nm^{-1}$ 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 ($0.02061nm^{-1}$) contains at least 2 insignificant digits

We specifically used this value as it was taken from measurement of Kitidis et al (2006) as an average value over the AMT. This did seem appropriate given that we were using AMT data for model validation. We now acknowledge that S_ACDOM does vary regionally in the new section 2.6 referencing Bricaud et al (2010) and Kitdis et al (2006) (see above).

We remove the "insignificant digits" from the revise text and table (though note that this is the value we actually use in the model and given by Kitidis et al 2006).

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 $a^{chl}_{ps,j}$ are m^2/mg Chl and Eo mol quanta per nm the units ofn $Lambda_{E,j}$, integrated over wavelength, will be quanta m^2 per mg chl.

Quantum yield has units of mmol C/mol photon (see Table 3). Thus units of Eq 2 are mmolC/mgChl /d. This is now included in the revised text.

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 every-thing to mmolC (as you assumed Redfield).?

Yes, phosphorus is the main currency of the model (we do mention this in the appendix, pg 2637, line 11-12). But we agree it would be easier to keep as much as possible in carbon for the main text and the tables and have done so in the revised version. However several things (e.g. matrix of P:all other elements) makes more sense to keep as it is – will be much easier for anyone using this paper as documentation for the model.

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10. Sec. 3.1/3.2 . Is the realism observed different from when you did not used a sophisticated optical model?

See our new section 4. (quoted above in response to major comment 1)

11. 3.3 numerical domination by picoplankton is well known. Do they dominate a_ph (they usually do not)?

It is likely that picoplankton dominate a_ph because a) the aphy* for picos is higher than for larger cells (Ciotti et al. 2002) and b) picos dominate the chl-a along the AMT transect (Poulton et al 2006). Consequently, since aphy = aphy* x chla, it is very likely that picos dominate the aphy as well. An interesting question would be where do different types dominate the absorption, but not the biomass. But we believe this is an issue beyond the scope of the current paper.

Ciotti A.M., Lewis M.R. & Cullen J.J. 2002. Assessment of the relationships between dominant cell size in natural phytoplankton communities and the spectral shape of the absorption coefficient. *Limnology And Oceanography* 404-417.

Poulton A.J., Holligan P.M., Hickman A., Kim Y.N., Adey T.R., Stinchcombe M.C., Holeton C., Root S. & Woodward E.M.S. 2006. Phytoplankton carbon fixation, chlorophyll-biomass and diagnostic pigments in the Atlantic Ocean. *Deep-Sea Research Part II* 53: 1593-1610.

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?

We use HPLC to compare to pigments directly in a paper we are just about to submit (Hickman et al). Since there are many uncertainties when trying to relate HPLC pigments to specific groups however, we decide not to include these here.

Again, we could use different AMT cruise data, but instead choose to refer to these data in the new text (We believe using different cruise data would get messy because there are measured along different transects).

We alter old text pg 2625. Lines 3-4 to:
"The model distribution of large phytoplankton biomass (e.g. diatoms, Coccolithophores) compared well to observations made along other AMT cruises (Tarran et al. 2006; Cermeño et al. 2008)"

13. Variability in Chl/CDOM has been reported in Bricaud and Morel 1981.

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We do acknowledge that there is variability between CDOM and Chl – in fact this is a significant reason to include explicit CDOM-like tracer (mentioned several times in the text). The experiment EXP-C3 was designed precisely to look at how important it is that CDOM and Chl vary. We do already reference Kitidis et al 2006 and Morel et al 2010 on this issue (see old text, pg 2630, line 15). We now also add the Bricaud et al (1981) (note, not Bricaud and Morel) paper as well in this location.

14. Fujji is Fujii (several instances throughout).

Apologies, these have been corrected.

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.

Our interest here is about interaction between ecosystem and light field. In this case complexity in species is needed but we believe RT requires less emphasis. See our comments and additional text mentioned above (Major Point 4). Additionally, the MITgcm is open source code, and thus we also feel it is important to include an open source radiative transfer component.

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).

We include this limitation (and the Stramski et al, 2004 reference) in the new section 2.6 (see reply to major point 3).

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

Thank you for your comments. Your suggestions have definitely improved the paper. Where we do not include your comments, we believe that the issues are beyond the scope of the paper – though several are directions we are currently working on.

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