

Interactive comment on “A new parameterization for surface ocean light attenuation in Earth System Models: assessing the impact of light absorption by colored detrital material” by G. E. Kim et al.

G. E. Kim et al.

gpark16@jhu.edu

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Response to Anonymous Referee 2

We are grateful for Referee #2's comments.

We provide the following responses to the reviewer's comments.

1) The Abstract needs to be rearranged to convey the most important findings C2583

of the paper and the assumptions/limitations used. Important findings are the decoupling of surface nutrients and surface biomass as well as the fact that light limitation affects differently the surface and the total productivity. Important assumption is that of a unique relationship (equation 5) imposed on all of the biomes. Important limitations are the small amount of data the empirical relationship is based on and the fact that the adg used in the model is fixed in time.

We agree that the abstract should highlight the important findings and assumptions as the reviewer mentioned. We suggest the following revised abstract to replace the previous one:

Light attenuation by colored detrital material (CDM) was included in a fully coupled Earth System Model. A modified parameterization for shortwave attenuation is presented in this study, which is an empirical relationship between 244 concurrent measurements of the diffuse attenuation coefficient for downwelling irradiance, chlorophyll concentration and light absorption by CDM. Two ESM model runs using this parameterization were conducted, with and without light absorption by CDM. The light absorption coefficient for CDM was prescribed as the average of annual composite satellite data from MODIS Aqua from 2002 to 2013. Comparing results from the two model runs show changes in light limitation associated with the inclusion of CDM decoupled trends between surface biomass and nutrients. Increases in surface biomass were expected to accompany greater nutrient uptake and therefore diminish surface nutrients. Instead, surface chlorophyll, biomass and nutrients increased together. These changes can be attributed to the different impact of light limitation on surface productivity versus total productivity. Chlorophyll and biomass increased near the surface but decreased at greater depths when CDM was included. The net effect over the euphotic zone was less total biomass leading to higher nutrient concentrations. Similar results were found in a regional analysis of the oceans by biome investigating the spatial variability of response to changes in light limitation using a

single parameterization for the surface ocean. In coastal regions, surface chlorophyll increased by 35% while total integrated phytoplankton biomass diminished by 18%. The largest relative increases in modeled surface chlorophyll and biomass in the open ocean were found in the equatorial biomes, while largest decreases in depth-integrated biomass and chlorophyll were found in the subpolar and polar biomes. This mismatch of surface and subsurface trends and their regional dependence was analyzed by comparing the competing factors of diminished light availability and increased nutrient availability on phytoplankton growth in the upper 200m. Understanding changes in biological productivity requires both surface and depth-resolved information. Surface trends may be minimal or of the opposite sign to depth-integrated amounts, depending on the vertical structure of phytoplankton abundance.

2) The Introduction would benefit by an extra paragraph that describes the results by Siegel et al 2005 with regards to the distribution of CDOM in open vs coastal waters, equatorial vs high latitudes. Siegel et al 2005 show that most of the signal from CDOM is in coastal waters. The implicit reason for discussing the regional dependence is to set the stage for qualifying the parameterization described in the next section.

By "qualifying the parameterization," we understand the reviewer to be referring to the statement made in comment 1, i.e. the assumption that one optical parameterization is imposed on all biomes. We propose to simply state that region-specific optical relationships are not captured by this single global parameterization. We agree the results from Siegel et al 2005 are relevant to this paper and will be included in our discussion of the bio-optical assumption at the end of the Introduction section.

3) In Introduction, the paragraph starting in line 25 explains how CDM abundance is not a local property of the seawater (as maybe chl is) because it is determined to a large degree by riverine outflow or continental runoff which in turn is

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determined by conditions on land and has large seasonal cycle, particularly at mid and high latitudes. Annual means, as being used in this study, are therefore not well representing the actual change of CDM in these regions.

We struggled with the choice to use an annual average instead of a monthly climatology, for the reasons the reviewer mentioned. The downside of using a monthly climatology is the reduced spatial coverage in higher latitudes. We thought it was most important to reduce the total area where satellite data was missing. We agree that our use of an annual average does not capture the seasonal variability of CDM in the ocean and state this explicitly in the last paragraph of section 2.

4) The new parameterization, Equation (5), is obtained after all the data from NOMAD with concurrent values of k_d , chl and adg are plotted in a single plot and fitted by a least-squares regression. However, inspection of Figure 2 shows that most of the data points are in very specific areas, not representative of the global ocean. For example, coastal upwelling regions, Arctic Ocean as well as the open ocean are underrepresented. Of course, this is inevitable, given the few data points where concurrent measurements exist, hence not a criticism here. However, I suggest the authors discuss the validity of (5) doing some quick sensitivity analysis. For example, if they removed from their regression fitting a group of values at a time, eg the Southern Ocean, or the Amazon outflow, would they get very different coefficients in (5)? In this manner, they can assess how important each region is for obtaining the parameters in Equation (5).

We conducted the exercise suggested by the reviewer. We removed the following clusters of points to test the sensitivity of the parameterization to the data from each region: (1) north Atlantic; (2) Amazon River outflow and nearby offshore stations; (3) Antarctic peninsula; (4) Southern Ocean; (5) western Pacific; (6) stations across the Pacific ocean and (7) eastern Pacific. These clusters correspond to the color-coded spatial groupings shown in figure 3 in the response to referee #1. The parameters are mostly stable. The only parameter whose change was well outside the fitting variability

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was the exponent to the chlorophyll term, which increased by 0.23 when the eastern Pacific stations were omitted.

5) Section 2.2, lines 9-11. It is unclear to me whether the present model configuration allows for changes in climate (SST) due to changes in chlorophyll distribution which in turn result from differential light absorption. Please clarify. This will affect the discussion of biomes later on.

The same optical model is used for calculating light attenuation for ocean physics and biology in our ESM configuration. Therefore, the same attenuation depth is used in evaluating physical processes (such as the surface shortwave heating flux) and biological productivity (by setting the euphotic depth for phytoplankton). The optical model calculates light attenuation using model-derived chlorophyll concentration. Increases in chlorophyll concentration will reduce the attenuation depth, reducing total light available for photosynthesis and the total shortwave heating of the ocean. We will expand on this point to make it clearer to the reader in the manuscript.

6) Equation 10 is not clear. Please explain C. Is it a factor multiplying only $(nlim+llim)^3$?

Yes, C is a constant multiplying only the first term. Reducing equation 9 gives

$$B = P^* \left(\frac{P_0^C}{\lambda_0} \right) \cdot \left(\left(\frac{P_0^C}{\lambda_0} \right)^2 (nlim \cdot llim)^3 + (nlim \cdot llim) \right).$$

C in the manuscript equals $\left(\frac{P_0^C}{\lambda_0} \right)^2$, a constant.

7) Please enlarge the fonts on all figure axes, legends and contour labels as they are hard to read.

We will make this change as suggested.

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8) Section 2.2 line 16: with regards to seasonal variability, please see earlier comment about riverine and coastal runoff which are largely responsible for CDOM distributions there. Annual means will underestimate the effect in light attenuation. Please discuss this point.

See response to comment 3 above. We will include a discussion of how the annual average underestimates CDOM during certain months of the year in areas that are affected by coastal runoff.

9) How do adg/chl values from MODIS compare with the NOMAD values that were used in obtaining Equation (5)?

We did not matchup in situ measurements with satellite-derived data products because we consider this to be outside the scope of our study. We refer readers to publications pertaining to the performance of the GSM a_{dg} (443) product for this type of analysis. We do not utilize the chlorophyll data product from MODIS for our model runs, and thus find a comparison of in situ vs. satellite estimates irrelevant to our study. Chlorophyll concentration is predicted by the biogeochemical model.

10) Section 3.2. It is a very good idea that the authors chose to disentangle chl from CDM in their runs and compare equation (5), i.e. run “chl&CDM”, with equation (5) without CDM “chl only run”. However, it would be informative to see the comparison of equation (5) with results from the model when Equation (4) was used. The reason is that, as the authors state in Section 2.2, lines 19-23 and show in Figure 4, the earlier parameterization produced higher distributions of chl compared to observations, and I wonder whether the new parameterization will further deteriorate the results. Of course, the improvement of Equation (5) is that it includes a missing process, but we still want to know what the authors think are the major sources of model error are then.

This was originally not included because it is tangential to the core focus of our study.

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Nonetheless we understand there is interest in understanding the contribution of missing CDM in model error. In response to this comment and #14 we propose to swap sections 3.34 and 3.4 and add the differences in the model runs using the old and new parameterization. The new section 3.4 will be titled "Coastal Regions and Model Error". We will add global trends in nutrients, biomass and chlorophyll, as well as figure 5 from the response to referee #1.

11) Section 3.2, line 23: "Biological productivity moves up the column. . ." This is not an accurate expression. BP does not "move up" the column, rather it increases near the surface and decreases below. Please correct this expression here and elsewhere it appears.

We will make this change in the manuscript.

12) Section 3.2, line 27: "particulate matter is consumed in the water column" do the authors mean "particulate matter is remineralized"? It seems to me that would be the most appropriate notion here.

Yes, that would be a better way to word this. We will make this change as recommended.

13) Why is Figure 10 mentioned before Figure 9. Please order Figures as they appear in the text.

We will make this change in the manuscript.

14) Section 3.3, lines 10-13: here the authors do compare the run including Equation (5) with the run including Equation (4), but only for coastal ocean. Would it be possible to see the same comparison for the global trends? This is also my point (10) above.

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We will include this in the manuscript in a revised section 3.4 as mentioned in the response to comment 10.

15) Section 3.4, paragraph starting with line 3. I am not clear, as to what causes the changes in biomes. The authors state that the biomes are computed based on winter mixed layer depth, vertical velocities and ice extent, following Sarmiento et al (2004). All these are physical model changes, which imply that SST changes when chl changes. If that is the case, I would like to see a model validation of SST in the "chl&cdm"run and the "chl only" run.

As described in the response to comment 5, the physical and biogeochemical models are coupled. Changes in chlorophyll concentration can change SST according to our model configuration. This is why the biome areas are different between the two model runs. The following SST contour plot shows modeled (chl&CDM) minus observed using NOAA_OI_SST_V2 data provided by the NOAA/OAR/ESRL PSD, Boulder, Colorado, USA, from their Web site at <http://www.esrl.noaa.gov/psd/> (Reynolds, 2002). The RMS error between annually averaged modeled and observed SST is 1.5°C. Additional validation details for the physical ocean model can be found in Galbraith et al 2011. Chl-only minus observed is not shown because the differences are qualitatively similar to those shown in figure 1 below. The differences in SST between the two models runs are small. See figure 2.

16) Figures 13 and 14 are a very nice representation of the changes in the 2dimensional limitations space.

Thank you.

17) In the discussion of Figure 14 (Section 3.4, lines 8-20), please clarify whether the decreases and increases discussed and the vector lengths shown in Figure 14 are absolute differences or normalized differences (eg. Percentage change)?

The values and vector lengths shown are absolute differences. These values are all

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less than 1. You can think of them as scaling factors that scale down the optimal productivity based on nutrient and light limitation.

18) Section 4, Conclusions, line 27: Please replace the expression “movement of biological productivity higher up the water column” with the more appropriate “increase of biological productivity in the upper water column and decrease below” or similar.

We will make this change as suggested.

19) Overall, very few typing errors exist, which a word processing software should easily capture.

Indeed.

References

Galbraith, E. D., Kwon, E. Y., Gnanadesikan, A., Rodgers, K. B., Griffies, S. M., Bianchi, D., Sarmiento, J. L., Dunne, J. P., Simeon, J., Slater, R. D., Wittenberg, A. T., and Held, I. M.: Climate Variability and Radiocarbon in the CM2Mc Earth System Model., *J. Climate*, 24, 4230–4254, 2011. 6, 8

Reynolds, R.W., N.A. Rayner, T.M. Smith, D.C. Stokes, and W. Wang, 2002: An improved in situ and satellite SST analysis for climate. *J. Climate*, 15, 1609-1625.

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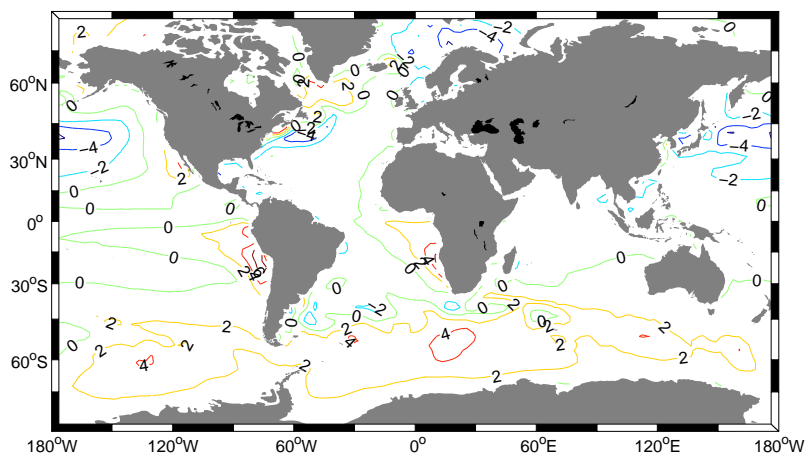


Fig. 1. Difference in annual average SST in degrees C, chl&CDM minus observed using the NOAA_OI_SST_V2 dataset (Reynolds, 2002).

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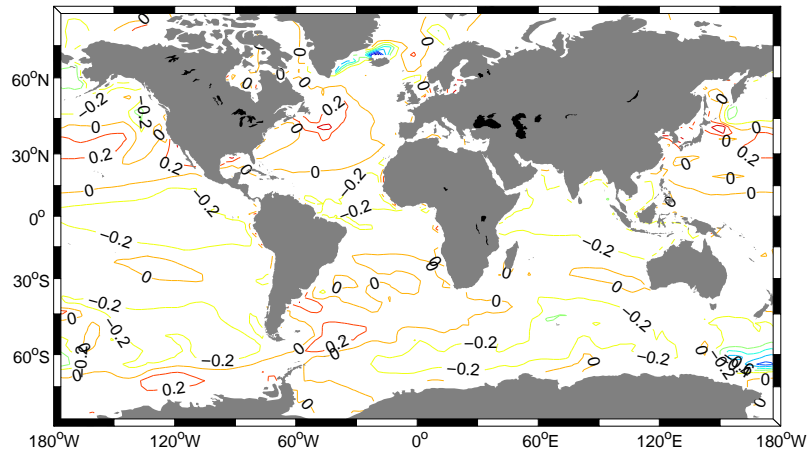


Fig. 2. Difference in annual average SST in degrees C, chl&CDM minus chl-only

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