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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.

Anonymous Referee #2

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The paper describes an empirical statistical relationship between light attenuation due to absorption by colored detrital matter and chlorophyll and evaluates its performance in a typical Earth System Model. Light attenuation by chlorophyll alone is well observed based on in situ measurements where empirical relationships led to parameterizations used in ESMs. The present study extends one typical of those parameterizations (Manizza et al, 2005) to encompass colored detrital material (CDM) which is the sum of the colored detrital organic matter (CDOM) and non-algal detrital particles. The topic of the paper is relevant to BG. The authors propose the inclusion of an important process,

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light attenuation by CDM, in Earth System Models, and provide a possible parameterization of it. The paper therefore is of interest to a large part of the BG readership. The work is well researched and written up and I recommend its publication in BG after all the major & minor comments below are addressed.

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

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.

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 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.

4) The new parameterization, Equation (5), is obtained after all the data from NOMAD with concurrent values of kd, 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 coefficeints in (5)? In this manner, they can assess how important each region is for obtaining the parameters in Equation (5).

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.

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

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

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.

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

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

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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.

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.

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.

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

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.

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.

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

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)?

18) Section 4, Conclusions, line 27: Please replace the expression “movement of bio-

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logical productivity higher up the water column” with the more appropriate “increase of biological productivity in the upper water column and decrease below” or similar.

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

Interactive comment on Biogeosciences Discuss., 12, 3905, 2015.