Ocean Sci. Discuss., 12, C1119–C1121, 2015
www.ocean-sci-discuss.net/12/C1119/2015/


Ocean Science
Discussions

# *Interactive comment on* "Carbon-based phytoplankton size classes retrieved via ocean color estimates of the particle size distribution" *by* T. S. Kostadinov et al.

**Anonymous Referee #2**

Received and published: 9 November 2015

This paper is on the partitioning the carbon biomass between three size classes – pico-, nano- and microplankton which is a novel approach. The authors used a satellite-based model to derive the carbon content and then compered their results to the existing carbon models. They presented a novel and valuable approach to estimating the carbon content in the ocean with the use of satellite data. The model is quite simple and described well in the methodology section. It is based on the model presented by Kostadinov et al. (2009) for deriving the power law size distribution from the backscattering data. It contains a massive description of the obtained results and the uncertainty analysis, which makes the manuscript very long (39 pages!).

The authors used monthly SeaWiFS data to compute the phytoplankton carbon in cho-



sen size classes. Have the authors tried to use satellite data of higher temporal frequency (8-day mean, daily data)? Considering the fact that the SeaWiFS sensor is no longer working, is it possible to apply this model to the operating satellite-borne sensors?

The estimation of the cellular carbon content in living phytoplankton is a little bit confusing. The authors assumed that the C:POC ratio in the entire ocean is constant and equals 1/3 which is in the middle of the observed range (0.14 to 0.49), however they admit that it can be a source of an error (p. 608). How much would the carbon estimate differ if a different value of C:POC ratio was taken?

The method described here has not been fully validated with the use of field measurements (the author emphasized it e.g. on p.589, p. 590, p. 591). The authors admit that they had no sufficient data set to do it. Could the validation of this model be performed on data sets used e.g. by Behrenfeld et al. (2005)? In the further part (p. 596) the authors present the "in-situ closure" which I did not fully understand. What kind of model output was compared to the cruise data? Were they monthly data? Or did the authors use daily values? It does not seem that the cruise data match the model data so well (Fig. 8).

The authors calculated also the total global phytoplankton biomass stock (p. 590 - 594) and compared their result to the published data obtaining relatively low values (0.2 – 0.3 Gt C compared to the published 0.3 – 0.86 Gt C). They explained the differences could result from different integration depths (resulting from i.e. from different criteria for calculating the MLD). Will the change in the integration depth increase the results substantially? Will they be closer or even exceed the results published by Antoine et al. (1996)?

In general, this is a good paper introducing a novel approach to the estimation of carbon content in the ocean. However, the description of the results together with the discussion is very long and sometimes the authors repeat the same information, e.g.

the lack of in situ data to validate the model. I am afraid that the length of this paper makes it somewhat difficult to follow and makes the reader loose the point at the end. Therefore I recommend to shorten the description of the results in order to extract and highlight the most important outcomes of this research. I am sure that shorter and more concise description will make the paper much better.

---

Interactive comment on Ocean Sci. Discuss., 12, 573, 2015.