February 5, 2016

In our responses, references to sections of text, tables and figures refer to the revised version of the manuscript unless otherwise noted. The reviewer comments are quoted in italics, and our responses are in plain text.

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 manuscript has been shortened significantly, and material more tangential to the main story line is now placed in a supplement. The supplement now includes several sections of text (8 pages, including 3 of the equations), two tables, and 9 figures.

The authors used monthly SeaWiFS data to compute the phytoplankton carbon in chosen 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?
For this manuscript we indeed used monthly reflectance data as input. This algorithms is non-linear in nature (in terms of the PSD slope ξ, and No is averaged in log space, making the algorithm non-linear in $\log10(N_o)$ as well). For more information see our response to the other reviewer's comment to "*P578-579 and P 586 L 4* " in the original manuscript. Work is already in progress to generate the C products from daily $R_{rs}(\lambda)$ values and address this issue. Applying the LAS2006 algorithm to other sensors is not trivial and leads to added uncertainties, and bias between sensors complicates merging. We are currently addressing these issues and planning to use more modern active sensors, rather than just SeaWiFS by generating the C products from the merged and bias-corrected OC-CCI data set. The results will be used in future work.

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?
A sentence was added in the methods to clarify that Eq. 2 refers to all backscattering in-water constituents. Hence the factor of 1/3 is needed to approximate the living phytoplankton fraction only, assuming biogenic origin of the scattering particles. A sentence was added in Sect 3.8.2 (with the respective references) clarifying that total backscattering can be due partly to inorganic particles such as coccoliths and even bubbles, in addition to inorganic suspended sediment, which are already mentioned.

The equation for carbon concentration in any one size class is linear in the assumed constant of 1/3 (Eq. 4, Eq. S1). Therefore, in any one given pixel if the actual C:POC value is about 0.5

instead, our approach will yield an underestimate of ~32%. If the actual C:POC is close to its low limit of ~0.14, then our approach will yield an overestimate of about ~140%. These are the most extreme values of error expected, i.e. they are an upper bound, given the laboratory measurements summarized in Behrenfeld et al. (2005). Considering that the N_o parameter uncertainties affect the C estimates to a much larger degree (Figs. S6A, S9C), we choose to address those first by introducing an empirical correction to this parameter (Sect. 3.9). This leads to satisfactory validation against total POC measurements (Fig. 10). Addressing the 1/3 fully is a non-trivial task out of the scope of this work. Future work should address this by gathering more field data on the C:POC ratio and by using additional PFT and ecological information to inform the introduction of a dynamic (spatially and/or temporally) conversion factor.

Finally, it is important to emphasize that just like the No factor, the 1/3 factor will cancel in the estimation of the fractional PSCs. As long as it is reasonably constant in a single pixel across the size range considered, its exact value will not influence the values of the fractional carbon-based PSCs.

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).

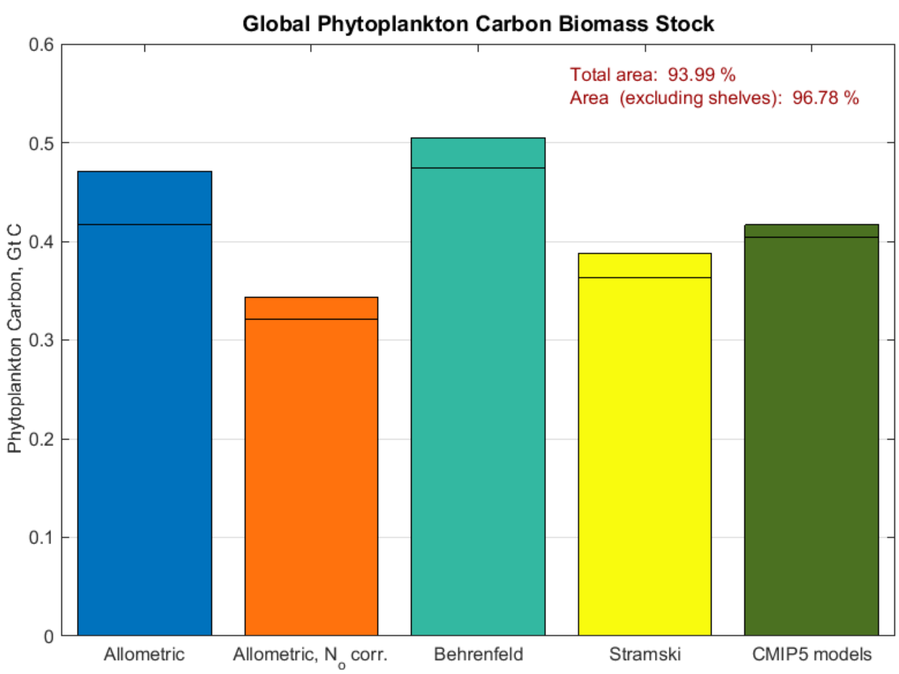
Regarding the in-situ POC-PSD closure with AMT cruise data, see our response to reviewer #1, their comment starting with "*Perhaps I missed some key information…*". With regards to the level of significance of the regressions for this in-situ closure (now Fig. 5), we also present an explanation in our response to that comment. Fig. 5 includes no satellite data. While it is true that the regression statistics leave something to be desired, they do demonstrate that to first order this is a viable approach. We emphasize that this is a broad, first-order approach upon which ourselves and the community should improve in the future, that is the goal. We also do not have enough samples for a really robust analysis of the closure, and some assumptions, such as the range over which the PSD slope is calculated, introduces uncertainties and differences with the satellite PSD.

Regarding validation, please see our comments to reviewer #1, their comment for page 589 "*L17-20"*. We emphasize that we have now introduced an empirical correction to address the spatial exaggeration of absolute carbon values, and in addition we have added a validation using in-situ POC measurements from the SeaBASS data set, matched up to the SeaWiFS observations. The validation is satisfactory (Fig. 10). We are qualitatively comparing the algorithm output to the Behrenfeld et al. (2005) algorithm in Fig. 1 and the quantitatively comparing the global sum in Fig. 2.

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)?
The key point is that one and the same MLD global field was used to compute global phytoplankton carbon biomass stock with all three satellite methods compared here, and with the CMIP5 model output. Thus, these estimates are fully comparable to each other, keeping the MLD input exactly the same among them (Fig. 2), which is the goal here.

Each pixel's satellite estimate is multiplied by the MLD to get to total carbon concentration within that pixel. The global estimates are thus a linear function of MLD. If MLD is changed by a given percentage uniformly everywhere, the C estimate will change by the same percentage. The value of the MLD is of particular importance in pixels with high biomass of course. Usage of another MLD climatology does indeed change the results substantially, as illustrated by the figure below – it's the same as Fig. S7 (compare also to Fig. 2), but instead of using the Hadley MLD estimates, the NCEP-based climatological values are used. Compared to Fig. S7 and Fig. 2, values are almost doubled. This illustrates that MLD uncertainty is a large factor in our confidence of estimating the total phytoplankton carbon biomass stock. Sentences were added in Sect. 3.2 to address this. The figure below also illustrates that while the Antoine et al. (1996) estimate is still significantly higher probably due to other methodology differences, our estimates can be considerably closer to theirs just by using a different MLD climatology.



We further note that we have introduced an empirical correction to the PSD parameter N_o, which leads to a reduction of the estimate according to the PSD/allometric method (Fig. S7 and figure above) and importantly a reduction of the percentage of biomass stock in the shallow shelf regions.

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.
We have shortened the manuscript and placed a considerable amount of material (8 pages, including 3 of the equations, two tables, and 9 figures) in a supplement. We deleted some repeating statements.

For a summary of major revisions/changes in the manuscript, see our response to reviewer #1.