

Author responses to Interactive comments on “Carbon-based phytoplankton size classes retrieved via ocean color estimates of the particle size distribution” by T. S. Kostadinov et al.

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 is a very long paper (39 pages plus 11 figures, not counting references) that loses the reader in a complicated description of results based on relatively simple algorithms and a mountain of assumptions.

The text has now been significantly shortened, and material that is more tangential to the main story line and important points has been placed in a Supplement. This includes several sections of text (8 pages, including 3 of the equations), two tables, and 9 figures. The main paper now contains 10 figures. Regarding the large number of assumptions, they are necessary given the current state of the art and the main overarching goal of our work – to decrease the empiricism in today's bio-optical models and base our model more on first principle of scattering, in this case. Assumptions are necessary because the seawater system is too complex to describe with only a few parameters available with multispectral remote sensing data. What we present here is a proof-of-concept approach which is very promising and can be improved in many ways, for example with the advent of hyperspectral sensors. Our algorithm also has other advantages compared to the simple empirical alternatives – we discuss those in the text.

P 575 L8 – this statement shows old references; is this still the present view – or does this need to be qualified strongly with a specific timeframe and types of processes?

The prevailing view is still that the biological pump plays a major role in C sequestration – we added several new relevant references - Siegel et al., (2014); deVries et al., (2014); Marinov et al (2008). This is a general statement and does not refer to specific time scales or processes.

L23 – it is not just climate that can affect these patterns – they change at much shorter time scales as well, and also spatially for a number of reasons

The reviewer is of course correct that the PFTs change on much shorter scales as well. A sentence was added to clarify that.

P 576 L 6 – define green models/reference

These sentences were rephrased and more information was added to clarify what is meant by dynamic green ocean models. The term comes from Le Quere et al (2005) which was originally cited and is cited in the revised text as well.

P 578 L25 – is relaxing the best way to describe this, or “departing from”? “improving on”?

"Relaxing" is the correct term to use when we mean that we are no longer making that assumption, i.e. we have relaxed it. It is used in the literature previously, an example from the bio-optics field is Siegel et al. (2000) below dealing with the relaxation of the black pixel assumption in atmospheric correction.

David A. Siegel, Menghua Wang, Stéphane Maritorena, and Wayne Robinson, "Atmospheric correction of satellite ocean color imagery: the black pixel assumption," Appl. Opt. **39**, 3582-3591 (2000)

P578-579 and P 586 L 4 – how are uncertainties estimated if the time-element is not really included, specifically, the authors use monthly SeaWiFS data to compute the size-fractionated organic carbon content. Is this a linear quantity, i.e. do you get the same result if this is computed ‘daily’ and average to monthly fields?

The C products are not linear in the PSD slope ξ . The fractional PSCs are not dependent on N_o , and the absolute C concentrations are linear in it. See Eq. S1 in the Supplement of the revised version. The PSD products themselves are not linear in the input backscattering coefficients or remote-sensing reflectances. Therefore it is not equivalent to compute the products from daily imagery and then average them to produce a monthly image vs. computing the products from a monthly image of reflectance. This work uses monthly images as input, but in the near future we plan to use daily input products to address this issue.

We have added the following paragraph in Section S3 (in Supplement) to acknowledge and explain this issue:

“The C products are not linear in the PSD slope (Eq. S1) or the underlying backscattering coefficients and $R_{rs}(\lambda)$ values. The absolute C concentrations are linear in N_o , but not in the logarithm of N_o , and N_o is averaged temporally in log-space. Since various steps of the algorithm are not linear, calculating the C products from a composite image of $R_{rs}(\lambda)$ or the PSD parameters and calculating them from individual imagery and then averaging are not equivalent. This work uses monthly SeaWiFS maps, but in the future processing is planned from daily imagery in order to address this issue. ”

In addition, we have clarified and rephrased the paragraph around Eq. S3 to make it clearer that the compositing is done temporally, starting from monthly maps.

P 580 L 19-20: This is confusing. Eq (3) provides cellular carbon content. Yet they then multiply by 1/3 to obtain “living phytoplankton” C. Aren’t ‘cells’ living? I would not call them living if they were detritus. . .?? It is also unclear why the authors first claim that the method they use is better than previous methods because it is not tied to a constant CHL:C ratio. Yet they introduce another constant, of 1/3, as in: “The carbon biomass of living phytoplankton only (C, [mgm-3]) can then be estimated by multiplication by 1/3”. It seems that the authors could have reached similar results simply applying an estimated carbon per cell estimate to the estimates of concentration of cells they published earlier (Kostadinov et al 2010)?

Eq. 2 is applied to the outputs of the KSM09 algorithm, which outputs the PSD parameters for the entire particle assemblage responsible for the backscattering. It therefore gives the volume of all particles, not just the living fraction. A sentence was added in the methods to clarify that Eq. 2 refers to all backscattering in-water constituents. Eq. 3 is from Menden-Deuer and Lessard (2000) and applies specifically to living cells measured in the laboratory. We indeed convert the Kostadinov et al. (2010) volume concentrations to carbon via Eq. 3, which then becomes an estimate of total POC if no large influence of inorganic particles or bubbles in the backscattering is assumed. Since previous work suggests that living C is about 1/3 of POC on average, we use

this constant to estimate just the living C fraction. A series of assumptions are necessarily made in this conversion, and they are explained in detail in Sect. 3.8.2 of the revised manuscript. Ideally the sources of backscattering should be distinguished before applying the allometric conversion. This is a difficult and important problem in bio-optics and future work is planned to address it. Finally, the advantages of the presented model are not only that it is more independent of Chl and less empirical in nature, but also that it partitions biomass in any desired size class. These advantages are explained in the text.

P 586 – why were data downsampled to 1 degree, if the original images are 9 km pixels? I didn't understand the need for this – clearly this eliminates substantial pixel noise the authors may have had.

The downsampling is done only to the Chl climatological image in order to make smoother contours of Chl to delineate the subtropical gyres. These are for visualization purposes only and are not used quantitatively in the analysis. In addition, downsampling was performed for the global spatially integrated biomass stock calculation in order to fill gaps. We used monthly and overall composite imagery and downsampled as a gap filling technique -- since this calculation is a summation over pixels, missing data can affect it. So the downsampling is primarily a gap-filling technique, not done to avoid noise. The data will be provided to end users in the original 9 km resolution, and the maps and rest of the analysis of the C products were done in the original 9 km resolution.

P 589 L 15 – aren't all these empirical algorithms, including the CHL algorithm, designed to match field observations? I don't understand why this limits the applicability of a dataset to be compared with the author's new results. It would seem that there is value in comparing the estimates of POC or phytoplankton C to these field observations as a validation step

We do not imply anywhere that the fact that the other algorithms are empirical limits the applicability and value of comparison with their data sets. In fact, because in-situ validation with partitioned carbon concentrations is not feasible, the qualitative comparison with the B05 and S08 methods and the CMIP5 models (Fig. 1) is more valuable as a form of indirect (not using in-situ matched up values) validation. We have also now suggested an empirical correction to the No parameter that leads to more realistic absolute total carbon concentrations, which we now do validate with in-situ POC measurements from the SeaBASS dataset.

L17-20 – how well are the size classes themselves validated globally, if you can't validate something simpler like POC? The patterns derived from the satellite data are described as truth and justified as conforming to common sense oceanography – but not validated against ground measurements.

We now have included a validation of empirically corrected absolute total carbon concentrations (Sect 3.9, Fig. 10) with in-situ POC measurements from the SeaBASS dataset. The validation is quite satisfactory and compares well to the B05 and S08 methods. Our algorithm is novel in that it partitions these carbon values into the PSCs and it takes into account the effect of the PSD on backscattering. It is based more on first principles of bio-optics, which necessarily requires more steps and some assumptions along the way. Regarding validation of the size classes, it is not feasible because currently even total carbon data is barely available (Graff et al 2012; 2015). In addition, the volume-based size classes were validated in Kostadinov et al. (2010) against HPLC measurements. The change from volume to carbon leads to different values, but since the

change is not extreme in value and is systematic at a given PSD slope (Fig. 6 in revised manuscript), we feel that not much new information will be added if we repeat the HPLC validation with the same match-ups. In addition, HPLC-based validation has its own issues since it is not equivalent to carbon fractions. In the future, such a validation is nonetheless planned with an extended merged satellite data set and a larger compilation of HPLC in-situ global observations. Finally, we are careful to state what is validated and what is not, and we do not describe our results as the truth.

Perhaps I missed some key information - I was confused on how the authors compared the satellite derived estimates to ship data, such as AMT. Did they use the monthly fields to compare to a particular AMT observation (a point on a particular date)? The regressions (done in log – log scale; Fig 8) are not very impressive. The eye would al-most say there is no relationship. The simplistic validation done here against data from one cruise is not sufficient to conclude that (P 612: We demonstrate that satisfactory in-situ closure is observed between PSD and POC measurements)

The AMT cruise data is used in an entirely in-situ closure analysis. As such, it is not a validation of the satellite products and we do not call it so. It is clearly stated in the methodology and results that no satellite data is used in this comparison. The text has been further revised to stress and clarify that. It is a proof-of-concept exercise to demonstrate that indeed there is a relationship between POC measured chemically and POC derived via our PSD/allometric method. We also demonstrate sensitivity to the integration limits, which is expected. There is no bio-optical modeling per se involved in this comparison, just conversion from PSD to volume to POC. We respectfully disagree that the regressions are not impressive. They are highly statistically significant ($p < 0.01$). It is correct that a relatively small fraction of variance is explained, leaving lots of noise variance, but this is expected in marine bio-optics. For examples of other publications with bio-optical regressions that have lots of noise variance, see IOCCG(2006) and multiple references therein. We revised the sentence in the conclusion in order to better acknowledge that the data set on which we base our conclusion is quite limited in scope. Note that finding concurrent POC and PSD measurements is quite challenging and we also demonstrate that same conclusion with a separate such data set already published in Kostadinov et al. (2012) and referenced here.

IOCCG (2006). Remote Sensing of Inherent Optical Properties: Fundamentals, Tests of Algorithms, and Applications. Lee, Z.-P. (ed.), Reports of the International Ocean-Colour Coordinating Group, No. 5, IOCCG, Dartmouth, Canada.

The uncertainty analysis conclusions point to problems with assumptions about the index of refraction used in Mie scattering modeling – but this discussion is not really included in the paper.

We discuss the importance of the N_o parameter in driving absolute C concentrations (e.g. new Section S3), and we state that this parameter's uncertainty is driven by the real part of the index of refraction (Sect. 3.8.2). In this Section (3.8.2) we provide a detailed assessment of why we believe that improvements in the treatment of the index of refraction in KSM09 will improve N_o and thus C estimates. In the interest of keeping the paper tight and manageable and length, and since both reviewers ask to shorten the paper, more discussion of this is not warranted. There is more discussion in Kostadinov et al., 2009, which is cited. In addition, we now suggest and

demonstrate an empirical correction to N_o that improves the absolute C estimates, and we include a validation with in-situ POC measurements using the SeaBASS dataset and match-up utility.

In any event, this is a long paper that ends up tiring the reader. Perhaps the authors can extract the essence and submit a more digestible version.

The paper has been revised to shorten it and a lot of material was moved to a Supplement – text (8 pages, including 3 of the equations), two tables, and 9 figures. We feel that the discussion on assumptions and sources of uncertainty is a bit long indeed, but it is important to keep it mostly in the main text, as this informs the user and the scientific community of the current limitations of our approach and drives the science forward by giving ideas of where things need to be improved. Instead of shortening methods, we prefer to move them to a supplement, because reproducibility is important.

Summary of major revisions/changes in manuscript:

- A Supplement was added into which a lot of material was moved. Specifically. Parts of Sect. 2.1.2 were moved to Sect. S1.1. Other methods details were moved to Sect. S1.2 (in-situ POC-PSD closure analysis), Sect. S1.3 (uncertainty propagation), Sect. S1.4 (other analyses and ancillary data).
- The monthly climatological global carbon biomass stock results and discussion were moved to the Supplement.
- The analysis of sensitivity to the PSD parameters and limits of integration was moved to Supplement.
- Sections and figures were renumbered accordingly.
- Two tables were moved to Supplement.
- 8 figures were moved to the Supplement.
- Two major scientific changes were introduced – 1) an empirical correction to the N_o PSD parameter is suggested that addresses the spatial exaggeration of retrieved absolute carbon values. 2) These empirically corrected values are validated against in-situ POC determinations from the SeaBASS data set.
- One section (3.9) and two figures (Figs. 9 and 10) were added in the main text and one figure (Fig. S7) and one section (S1.5) were added in the Supplement to discuss these changes.
- Overall the manuscript has been reduced in size significantly at the suggestion of both reviewers. Lots of material more tangential to the main story line is now in Supplement, because we still feel that it needs to be accessible to the scientific community as part of this paper.