Interactive Comment on "Global variability of phytoplankton functional types from space: assessment via the particle size distribution" *by* T.S. Kostadinov et al.

T.S. Kostadinov et al.

We thank Anonymous Referee #3 for providing useful comments to our manuscript. Below are our responses (in black) to his/her comments (*italics*).

Responses to General Comments

It is unquestionable that such a comparison is important, but they are not true validations. I recommend reducing substantially the entire "validation" exercise, replacing it for a simple comparison (perhaps keep only Figures 4 and 5). The introduction can instead have a small paragraph on differences, fundaments and assumptions of both HPLC and absorption-based approaches to retrieve cell sizes from ocean color (see works by Bricaud, Brewin, Ciotti, Devred, Hirata, Sathyendranath, Yentsch). In my opinion, the temporal analysis and the discussion of the retrieved parameters over the selected sites (having long term in situ observations) are much interesting and new, deserving emphasis.

The goals of our manuscript are to 1) Describe a very important application of the output of the Kostadinov et al. (2009) PSD approach, i.e. retrieval of the PFT's based on size as independently of Chl and absorption or pigments, which previous methods relied upon; 2) validate the results as best as possible given availability of global data and in-situ PFT methodology; 3) Use the available SeaWiFS data set to calculate global climatologies and describe them, focus on a few well known sites and look at the time series of relevant variables, as well as perform basic analysis of decadal trends and relationships with ENSO. In preparing the manuscript, we arrived at the conclusion that a paper just on the validation of the PFT from PSD method is not publishable on its own. But a paper on applying this method to look at PFT's will require some form of validation. Hence the paper we submitted.

We agree with the reviewer that the validation presented is not a comparison with independent ground truth data that is directly measuring the satellite-retrieved parameter. However, we are inclined to keep the term 'validation' because it is indeed a comparison with temporally and spatially matching in-situ observations of pigments, which are relatable to the PFT's via simple diagnostic pigment formulas (Vidussi et al. (2001)). Considering that there is no data set suitable for a global validation that directly measures the PFT's in-situ reliably, this is the best we can do for a validation, different from the Coulter counter validation of our PSD parameters themselves (Kostadinov et al. (2009)). It was not the objective here to validate the PSD itself, but rather the PFT's. We would like to point out that it is also recommendable to keep the standard term 'validation' to contrast this exercise with the comparisons to Uitz et al. (2006) and Alvain et al. (2008), which are definitely not validations, just comparisons with the satellite data of others. We did add the following comment to Sect. 4.2 to address the reviewer's concern: "therefore the diagnostic pigment estimates of the PFT's cannot be considered 'true values' in this validation; thus we note that the term 'validation' is used loosely here."

Regarding restructuring the manuscript, we also agree that the analyses suggested are of greater interest and should be expanded, but the objectives of our proof-of-concept approach are not a detailed comparison with and overview of existing PFT methods, neither are they just a thorough analysis of trends and point time-series, as well as relationship to chl (see below). We rather aimed at introducing our innovative method, describing the decadal averages from global SeaWiFS data, and briefly validating and comparing our method to two prominent HPLC-based existing methods. Some of the objective the reviewer mentions are the subject or work in progress or planned future work.

Another very important contribution made, but unfortunately not explored too much in the results, is how complementary information to chlorophyll concentration can aid on the understanding of global biogeochemical processes. This was discussed in this paper, but a number of statistical analyses can quantify the degree to which (and where and when) particle number concentration and chlorophyll concentration do not co-vary, or how the ratio of both estimates behave in time and space. In addition, as the proposed model retrieves particle size as a continuous, the chosen size ranges can be set to smaller intervals, not being constrained to the only three classes. This may be more relevant for the nanoplankton size range than for the pico and micro, as the nano class is too broad (e.g., some authors suggest including an "ultra" class (2-5 μm) with nanoplankton varying from 5 to 20 μm).

Regarding further expanding the analysis of spatio-temporal correlation of particle numbers with Chl, the intention here is to introduce the subject and present it as future work. This is indeed a very important subject, and was in large part the original motivation for our work. It is work in progress at present, and we mention it to invite other groups to work on the subject with our data or other retrievals. One key aspect that needs further work is the retrieval of actual biovolume and its relationship to carbon biomass. It is the volume of particles, their carbon content and the relationship to Chl that is ultimately of interest. Therefore we believe that the exploration of this subject is best left for a subsequent manuscript that is in the planning stages.

Responses to Specific Comments

- use the real quantitative values instead of "satisfactory, good and poor"

The use of these ambiguous terms was reduced and changed, the R^2 values are now given and the validation of picoplankton and microplankton is called 'satisfactory, though clearly not excellent' and the validation for nanoplankton – 'worse'.

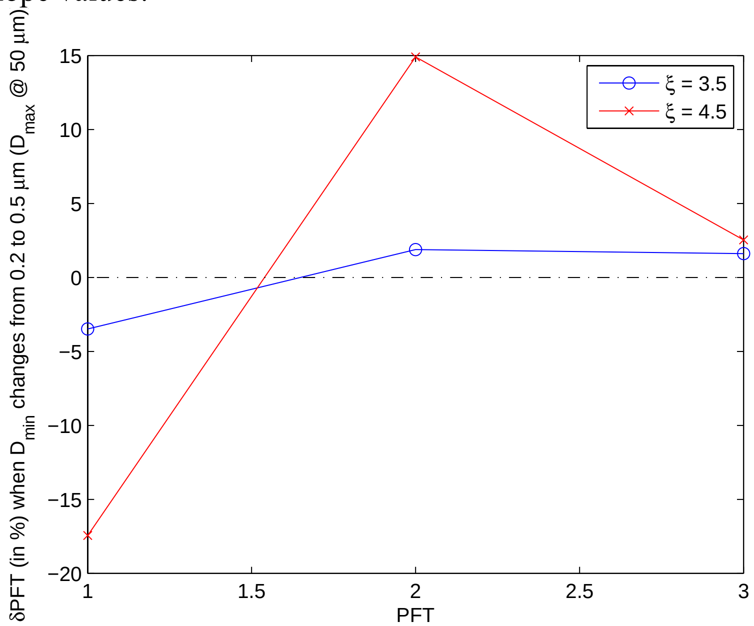
- enough arguments were presented to exclude the data above 60 degrees for all time average analises

Data from these high latitude regions is indeed excluded for the winter months in each respective hemisphere when performing the trends analysis. We have decided to include all available data at high latitudes in the computation of mission mean maps so that the maps are more complete geographically, and we caution the reader that the higher latitudes are biased temporally.

- why was the picoplankton lower size limit set to 0.5 um? Sieburth et al 1978 assigned 0.2 um.

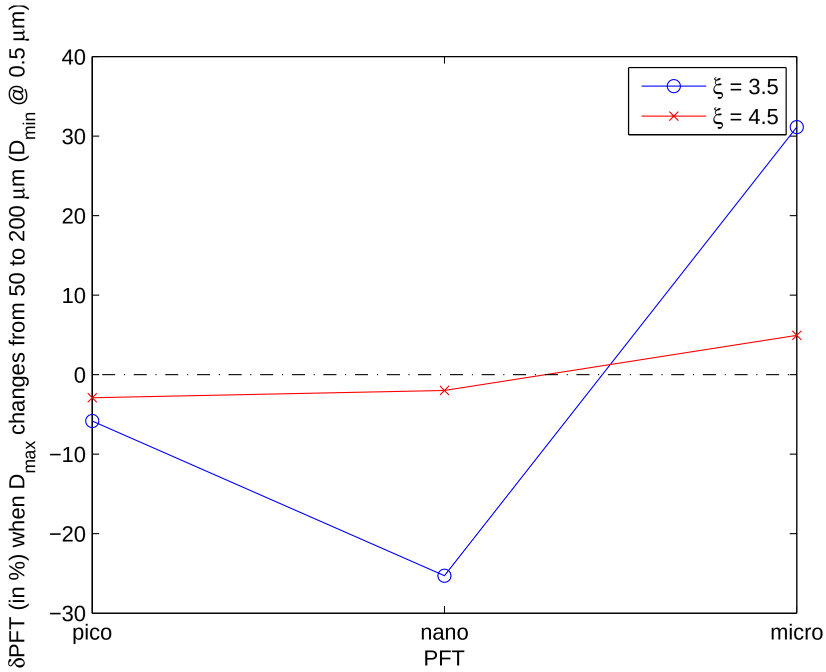
Section 4.1 explains our motivation for the size limits in detail. Since we're working with global data, our size limits present a reasonable balance against biasing the retrievals towards picoplankton or microplankton.

Sensitivity analysis was performed to address specifically the changes in the PFT with changing minimum and maximum diameter of integration. Namely, the change in the PFT's as the minimal diameter was varied from 0.2 to 0.5 mm is plotted below for two different PSD slope values:



It is evident that for low PSD slopes, sensitivity for all three PFT's is minimal (in fact similar to endogenous uncertainty values), whereas it becomes larger for picoplankton and nanoplankton (about 15 – 18 %) at higher PSD slopes. Considering the arguments in 4.1 as also outlined by the reviewer, and the fact that only one parameter is used for this unconstrained retrieval (the PSD slope ξ), this is an encouraging result.

Regarding sensitivity to the upper limit of integration, varying it from 50 μm to 200 μm similar analysis yields:



Here, as expected, at high PSD slopes sensitivity is minimal, whereas at lowser PSD slopes sensitivity of nanoplankton and microplankton increases to up to about 30% difference. Picoplankton are still robustly differentiated from the bigger group of nano + microplankton, which is often of most interest. Note that the maximal values seen here are a near worst case scenario and will only affect certain oceanic regions. As stated, the chosen size limits are a global compromise and appropriate for this proof-of-concept study. The following was added to the main text of 4.1 to address the results of this sensitivity analysis:

"A sensitivity analysis to these limits shows picoplankton contributions typically decrease by about 17% and nanoplankton increase by about 15% as the lower limit of integration is changed from 0.2 to 0.5 μm for a range of PSD slopes. For low values of the PSD slope, microplankton increase by about 30% and nanoplankton decrease accordingly when the maximum limit of integration is changed from 50 to 200 μm. Conversely for low PSD slopes, sensitivity to the lower limit is very small (<5%); while for high PSD slopes, sensitivity to the upper limit is small (<5%). "

Responses to Technical Comments

A review of nomenclature throughout the paper is needed. I acknowledge that these terms are widely used, but that does not make them correct. Here are some suggestions: - Particle size ranges (PSRs) instead of PFTs - Size ranges are only one aspect of PFT categories - inter-comparison instead of validation - Validation denotes some of the compared variables are "right" - Residuals instead of Anomalies - refers to a time series only 10 years long - Decadal average instead of climatology - same as above

We believe that the term PFT should be used to emphasize the plausible biogeochemical interpretation of our size distribution data. We clearly state that the PFT retrieval is size-based, with the assumption of biogenic origin of the particles. We further believe that

PFT is the appropriate term to use in terms of the goals of the manuscript – mainly, to further understanding and modeling of oceanic ecosystems and innovative use of satellite data products in modeling and analysis. Furthermore, phytoplankton functional type or group is the standard term used in the relevant literature (Le Quéré et al, 2005; Hood et al., 2006).

Regarding the 'validation term' – see Responses to General Comments above.

Anomalies is the standard term used in relevant literature (e.g. Gregg et al. 2005), where they only have 6 years of data).

'Climatology' was replaced with 'decadal average' everywhere in the manuscript.

- *Please, review for over-citation of Kostadinov et al 2009*
Some of the references to Kostadinov et al. (2009) were removed.

- *chlorophyll concentration and productivity are used as synonymous in some parts of the text*
It is not an emphasis or a goal of our manuscript to differentiate between chlorophyll and primary production, and given that chl is currently the main proxy for assessment of the trophic level of a given marine ecosystem, we use 'high productivity areas' and high chl areas' as loosely synonymous for our purposes. We acknowledge that future work that needs to analyze the relationship of Chl, productivity, biomass estimates and the PFT's in detail needs a stricter usage of these terms.

References:
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