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

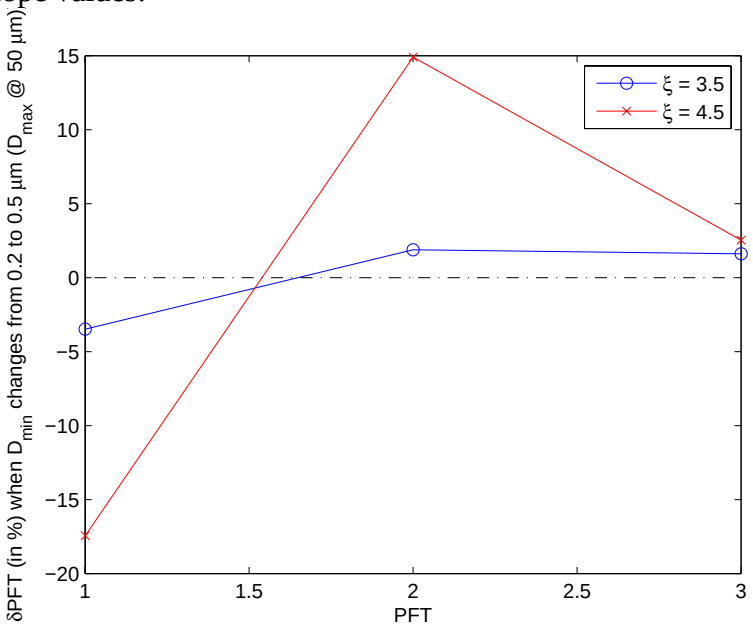
T.S. Kostadinov et al.

We thank Anonymous Referee #2 for providing useful comments to our manuscript. Below are our responses (in black) to his/her comments (*italics*).

Responses to General Comments

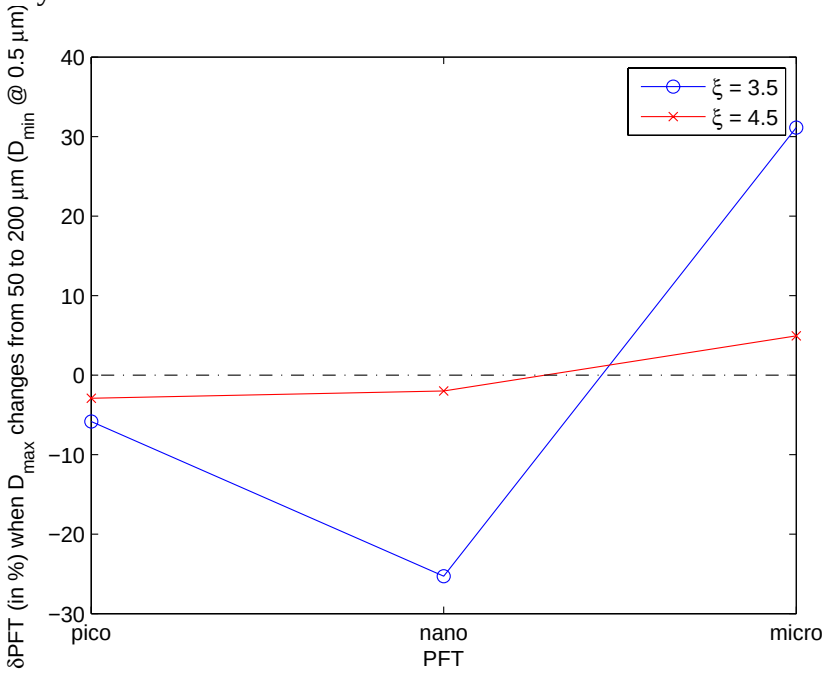
I would perhaps liked to have seen some sort of sensitivity analysis on the limits chosen (0.5-50 μm for total phytoplankton assemblage, 0.5-2 μm for picoplankton and 20-50 μm for microplankton, i.e. microplankton have been known to exceed 200 μm). However, I feel the authors sufficiently address this in section 4.1 of the discussion and I accept that perhaps this is outside the scope of this study, which as stated, is a proof-of-concept study. Furthermore, reading Kostadinov et al. (2009), extensive work was conducted on uncertainty with explanations of both endogenous and exogenous sources, and limitations of the model, in which the reader is referred to (see line 18-19 of page 4300).

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 Specific Comments

As stated by the authors, the model is based on retrieving the parameters of a powerlaw particle size distribution. As this algorithm incorporates nonlinearities I am concerned as to whether such a model should be applied directly to Global monthly Level 3 mapped SeaWiFS images (lines 23, page 4299). Instead, should the algorithm be first applied to daily Level 3 mapped SeaWiFS images, before being averaged to develop monthly composites? It may be that this makes very little difference, or that the models formulation is in fact sufficient for it to be applied directly to monthly images.

However, I recommend the authors investigate this further, possibly by testing, on a particular month, the differences between applying the model directly to a monthly composite, compared with applying it to daily composites from that month and then averaging to create a monthly composite.

We thank the reviewer for this very useful and constructive comment. We conducted a test on August 2007 daily and monthly SeaWiFS 9 km mapped data (Reprocessing 2009). Daily PSD and PFT products were generated and averaged to obtain monthly images. Those were then compared to the monthly product images generated using the monthly remote-sensing reflectance as input. For the PSD slope ξ, absolute differences between daily averaged and monthly collocated pixels has a mean of 0.099, a median of .041 and a standard deviation of 0.155 (N = 3,514,453). These differences are minimal compared to the typical PSD slope values and are comparable to reported endogenous uncertainties (Kostadinov et al. (2009). Likewise, for the N_o parameter, differences (in log10 space, N_o unit is m^{-4}) had a mean of 0.14, a median of 0.05 m^{-4}, and a standard deviation of 0.23. These differences are much smaller than the reported endogenous uncertainties. The same test was performed for the PFT products, namely % pico, nano and microplankton. In percent, the difference mean, median and standard deviations were as follows, for % pico-, nano- and microplankton contributions, respectively: (3.60, 1.53, 5.55; 2.55; 1.00; 3.87; 1.77; 0.89; 2.73). These are very low values and fall within the maximum reported endogenous uncertainty or the microplankton PFT (~ 7%, typical for the high productivity regions). Therefore, the effect of using monthly imagery directly instead of daily imagery and then averaging it is generally negligible.

Regarding the validation of the algorithm against in situ pigment measurements, I generally agree with the authors that given the discrepancies in using pigments to derive size classes (only a proxy of size, differences in temporal and spatial scales of satellite and in-situ measurements, and various other arguments highlighted in section 4.2) that the validation appears satisfactory. Nonetheless, I recommend the authors suggest additional validation methods that may be conducted in the future. For instance, the possible advantage of having coupled HPLC pigment and PSD in-situ measurements, together with concurrent satellite reflectance measurements, may help improve the validation of both this algorithm, but also the pigment based classification of Vidussi et al. (2001) and other satellite algorithms based on pigment proxies. This is touched on in lines 4-6 of page 4314, but I feel this could be expanded upon further. Nanoplankton perform poorly in the validation. Could this also be linked with evidence that nanoplankton have the highest diversity (Irigoien et al., 2004; Liu et al., 2009), which may increase the variability of their optical properties and hence make them difficult to detect from satellite?

We agree with the reviewer that the validation presented in our manuscript is preliminary and more extensive validation efforts are required for our PFT algorithm, as well as others. Comments were added to the end of Sect. 4.1 and the beginning of Sect. 4.2 to address this issue and suggest what additional data sets are needed in more detail.

Regarding nanoplankton validation, as we point out in the manuscript, we believe that the poor performance of nanoplankton in the validation is due to the nature of our algorithm. Note in Fig. 1 that for most values of the PSD slope typical of oceanic regimes (as

retrieved by the PSD model and used in the PFT algorithm), nanoplankton percentage is about 50%, with little sensitivity to a change in the PSD slope. This does not necessarily correspond to a real feature of oceanic ecosystems and is likely an artifact of the model formulation, which we state in the manuscript. We retrieve the PFT's using a single parameter (the PSD slope) and inevitably there are limitations to the approach that are properly acknowledged.

There are recently published attempts to improve the pigment classification of Vidussi et al. (2001) and Uitz et al. (2006) (see Hirata et al., 2008; Brewin et al., 2010). Particularly accounting for picoeukaroytes in low chlorophyll-a environments. This may explain discrepancies between the higher picoplankton percentages found using the model presented here when compared with Uitz et al. (2006) in oligotrophic environments (highlighted in lines 12-14 of page 4316 and Figure 11). Furthermore, using the Vidussi et al. (2001) and Uitz et al. (2006) pigment classification, Hirata et al. (2008) found a non-negligible proportion of Fucoxanthin within the oligotrophic gyres of the subtropical Atlantic. Fucoxanthin is also a precursor pigment of 19'-Hexanoyloxyfucoxanthin and maybe found in some prymnesiophytes (Vidussi et al., 2001; Uitz et al., 2006). Higher values of microplankton % in the oligotrophic gyres using the approach of Uitz et al. (2006) maybe due to errors in the HPLC based pigment to size-class classification (Figure 11).
We thank the reviewer for these comments. We do recognize many of the uncertainties in the HPLC methods for assessing PFT's and address this issue in Paragraph 2, Sect. 4.2.

Responses to Technical Comments
Lines 12-16, Page 4297: Would perhaps be nice to see some references backing up these statements, there are plenty available.
Citations were added as requested.

Lines 25-27, Page 4297: Would perhaps be nice to see some references backing up these statements, there are plenty available.
Citations were added as requested.

Line 22-23, Page 4298: The Alvain et al. (2008) approach was actually developed in Alvain et al. (2005), it was extended, validated and applied to the SeaWiFS 10-year dataset in Alvain et al. (2008).
The original Alvain et al. (2005) citation is now included when referring to the method.

Lines 14, Page 4304: "picpolankton" is spelt wrong it should be "picoplankton".
Fixed.

Lines 15, Page 4304: "nanoplantkon" is spelt wrong it should be "nanoplankton".
Fixed.

Lines 21, Page 4309: The word "Others" is capitalised after a comma, should it not be "others"?
Fixed.

Lines 11, Page 4310: Should "3C" and "8C" be "3C" and "8C"?
Fixed.

Lines 5, Page 4312: Add "a" in front of "smaller" and after "and" so the sentence would read ", with a smaller particle abundance and a larger contribution by picoplankton".
Fixed.

Lines 29, Page 4313: An additional bracket is needed after (Kostadinov et al., 2009) (i.e. "(Kostadinov et al., 2009))" to close the bracket in the line above.
Fixed.