Biogeosciences Discuss., 7, C2154–C2159, 2010
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Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 2 August 2010

General Comments

T. S. Kostadinov et al. discuss the results from applying a novel method for determining the global distribution and dynamics of phytoplankton functional types (PFTs) from satellite remote sensing. The methodology to this approach was previously published in the Journal of Geophysical Research (Kostadinov et al., 2009) and this manuscript focuses on the application on this methodology to 10-years of SeaWiFS data with a focus on marine ecology.

This manuscript addresses a relevant scientific question within the scope of BG and to a broad cross-section of the oceanography community. There is increasing demand

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for synoptic measurements of phytoplankton functional types, particularly in biogeoscience, where such measurements can be used for validation of, or assimilation into, multi-phytoplankton biogeochemical models. Satellite remote sensing appears to be the only method currently available to achieve this goal. While there are certain assumptions to attributing the spectral slope of the particular backscattering coefficient to the phytoplankton functional type, particularly the assumption that the open ocean assemblage is biogenic, I feel the authors make the reader aware of these assumptions and they are acknowledged when interpreting the results. As stated by the authors the approach is novel and appears to offer an alternative to other published pigment-based methods for determining the phytoplankton functional type, which in themselves make certain assumptions. Furthermore, this approach is compared with a few of these models and differences are investigated. The presented results are very interesting and the findings are noteworthy.

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).

Overall, I think that this is a well written paper that should be accepted for publication in Biogeosciences, subject to some moderate corrections detailed below. While I believe this manuscript will make a substantial contribution, I recommend the authors address the following issues:

Specific Comments

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As stated by the authors, the model is based on retrieving the parameters of a power-law 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.

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?

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). Par-

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ticularly 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).

Technical Comments

Lines 12-16, Page 4297: Would perhaps be nice to see some references backing up these statements, there are plenty available.

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

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).

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

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

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

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

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”.

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.

References:

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Irigoien, X, Huisman, J & Harris, R P. 2004. Global biodiversity patterns of marine phytoplankton and zooplankton. Nature, 429: 863-867.

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