**Biogeosciences
Discussions**

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Interactive comment on "Quantitative observation of cyanobacteria and diatoms from space using PhytoDOAS on SCIMACHY data" *by* A. Bracher et al.

Anonymous Referee #1

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General comments

The differential Optical Absorption (DOAS) method using SCIMACHY data is applied in order to classify phytoplankton types from satellite ocean colour. While the paper is a continuation of the previous work by Vountas et al, 2007, it offers the DOAS method as a successful method to classify cyanobacteria and diatoms in the ocean).

I believe that the scientific question addressed in this paper well fit to the scope of BG. Also the use of the DOAS method for this specific application is novel, especially with combination to new hyperspectral data obtained from SCIMACHY. The results obtained in this paper are interesting, if they are reliable.

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Validation of the methodology is preliminary. Number of in situ data used in the validation is simply not sufficient (5 points), especially when the authors oppose regional approaches (L18-20, pp4562) and their interest is the global scale. In addition, a more sophisticated strategy for validation campaign is required when the satellite data with such a low resolutions are used; I am not impressed with validation result where the authors used match-up data taken in a 12-hour and 180km window.

Presentation of this potentially-good work is not necessarily excellent. Critical information (e.g. a protocol to determine phytoplankton reference spectra, sensitivity of the reference spectra to the DOAS outputs, and reliability of ground truth data used for validation, protocol of validation exercise etc.) is missing, while redundant words/sentences are scattered here and there. Because the main part of this paper is about the DOAS methodology, sufficient (but concise) explanations of the methodology are required so that somebody other than the authors can also replicate the same/similar results presented in this paper. Unfortunately this requirement is not met. I believe that both of the authors and readers benefit from some kind of revision as to the presentation of the paper before final publication.

Reference: See Section 2.2 in Vountas et al. Ocean Sci, 3, 429-440, 2007.

Specific Comments

Introduction

L21-25, 4561: Please clarify that you are talking about the Chla-specific absorption coefficient of phytoplankton (aph*=aph/Chla) rather than the absorption coefficient of phytoplankton (aph), since it affects interpretations of your statement.

L25-27, 4561: Please add reference(s) to support this statement, since classification of phytoplankton by bio-marker pigments do have anomaly.

L6-11, 4562: Please add reference(s) which indeed shows that global satellite Chla algorithm is actually distorted by the packaging effects of phytoplankton absorption. In

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the paper, references cited are not meant to support your statement about the packaging effects as a cause of the algorithm error.

2. Instrument and methods

2.1 Satellite sensor SCIMACHY and principles of retrieval technique DOAS

Almost entire paragraph was copied from the previous publication (Vountas et al, 2007). I am not familiar with publication policy of BG regarding copy right and duplicated publication, but this is definitely not a good exercise, even if you were a co-author in the previous publication. This kind of presentation should be avoided. I will leave a judgement about this to the Editor.

There is no description about radiometric accuracy (e.g. for spectral response) of SCIMACHY over the ocean. How accurate are SCIMACHY data? How do the SCIMACHY data compare to the well-calibrated sensor like SeaWiFS? Since sensitivity test of the DOAS method is not presented in this paper, the authors need to present a justification of the use of SCIMACHY data in oceanic applications in some way.

2.2 The retrieval technique: differential optical absorption spectroscopy (DOAS)

Re-consider terminology *Earthshine radiance*.

This section is meant to explain the DOAS method itself before specific descriptions of the method applied to phytoplankton classification. But it actually does not help reader understand the rest of your paper (especially to understand Section 2.2.1 and 2.2.2; one of the key sections of this paper), because descriptions in Section 2.2.1 and 2.2.2 assume that readers are familiar with Vountas et al., 2007 and this section does not explain Vountas et al 2007 at all. Please provide a brief review of Vountas et al 2007.

Description about atmospheric sensing is irrelevant here. For example, readers of this paper will not care about the achievement of 30km x 60km spatial resolution by SCIMACHY for derivation of atmospheric trace gasses.

It seems that some sentences in this paragraph are copied again from the previous publication by Vountas et al. 2007 (but with a slight modification).

2.2.1 Retrieval of differential absorption by certain phytoplankton groups

L2, pp.4566: You are writing about the methodology for phytoplankton classification in this section, so this sentence is irrelevant. If you still want to include such a sentence, it may fit in Section 2.2 rather than here.

L3-6, pp4566: The objective of this paper is already given in Introduction, so these sentences are redundant.

In this section, you are assuming that readers are familiar with Vountas et al 2007 (indeed, this paper heavily depends on Vountas et al 2007 which is cited throughout this paper). However, since Section 2.2 does not explain Vountas et al 2007 at all, one would not be able to understand all details written in the rest of this section. Please re-write Section 2.2 so that readers can understand the rest of the paper. If readers do not understand this section, they cannot re-produce the same result you present in this paper; if no body else but only you can produce results shown in this paper, your science does not appeal any value to others.

L1-5, pp4567: These lines are redundat since they do not help reader understand the rest of your paper.

2.2.2 Retrieval of cyanobacteria and diatoms chl-a concentrations from SCIMACHY

L7, pp4568: Please re-consider the word, *earthshine radiation spectra*.

Please see the last comment in Section 2.2.1 above.

2.3 In-situ measurements of phytoplankton absorption and composition

L13-16,pp4569: The authors used CHEMTAX with the input matrix by Wright et al. (1996). How sensitive is the choice of the standard input matrix to the phytoplankton classification with your data?

The authors do not have microscopic data that give *true* phytoplankton classification, so their analysis is entirely relying on CHEMTAX. Please describe how much confident you are, as to your phytoplankton classification at ground data level.

Have you compared your classification to that by the diagnostic pigment analysis (Viddusi et al 2002 and Uitz et al 2006)? How similar/different will your result be, when the diagnostic pigment analysis is used to identify phytoplankton instead of CHEMTAX? Since there is no description about accuracy of CHEMTAX classification, the authors should compare their results with the diagnostic pigment analysis at least.

These are very important points because validation of the DOAS will be useless if phytoplankton classification by ground data is not successful.

L4-12, pp4570: How representative are your reference spectra of diatoms/cyanobacteria? This is an important point, because sensitivity of the DOAS to the reference spectra is not presented in this paper and there is no description for determining the reference spectra in this paper (In Section 2.3 you only showed how the in situ absorption measurements were taken, but did not show how the reference spectra were determined from the measurements). Although Vountas et al. 2007 is also cited, I don not see any analysis to determine the reference spectra in Vountas et al 2007 (hence this citation is useless here). Is there no ambiguity between differential aph* of diatoms/cyanobacteria and other phytoplankton (e.g. dinoflagellates and even nanoflagellates) in the spectral region of your interest ($<$ approx. 495 nm)? You discussed about the packaging effect in Introduction. When aph* for a specific phytoplankton varies due to the effect, does it not raise the potential ambiguity between aph* of two or more different phytoplankton?

References Uitz et al., J. Geophys. Res., 111, C08005.doi:10.1029/2005JC003207

Viddussi et al, J. Geophys. Res., 106, 19939-19956, 2001

3. Results 3.1 Phytoplankton absorption of cyanobacteria and diatoms from SCIA-

MACHY

L4-12, pp 4570: I personally think that these descriptions fit better in Section 2 (Instrumentation and methods).

L13-15, pp4570: This sentence is not easy to read because it is too long and there are too many "of"s. Please change the sentence.

L13 (pp4570) to L9(pp4571): Are these lines absolutely necessary to draw the conclusions shown later? I can understand your later conclusion, without these (I personally think that Figs.6 and 7 shown later are sufficient).

3.2 Biomass of cyanobacteria and diatoms from SCIAMACHY

L15-18, pp4571: There are too many invalid pixels in the cyanobacteria distribution derived from the DOAS, and visual comparison with NOBM is not easy. Please change the figures in some way for easier comparison.

What is the basis to mention that the distribution of cyanobacteria retrieved from SCIAMACHY data agrees well with the calculations made by NOBM? Please add some kind of statistics to support the statement.

L18-L24, pp4571: Only 5 match-up data are used for the validation, and therefore there is no statistical significance in the result shown in Table 1. The authors should recognise it, but there is no discussion about it. Then what is the basis to mention *a reasonable agreement with a moderate underestimation*? How the error of -4% to -70% representative? I am not convinced with the validation result presented here, if the satellite and in situ data were matched up within a 12h-180km window (especially for Diatoms). What is the basis to choose 12h & 180km? What is the protocol for your validation exercise? Please explain.

Table 1: There are many SCIAMACHY [Chla] on the single day (but within 12h time window)? I suppose that some neighbouring pixels, rather than an exact matched-up pixel, are also used. But there is no explanation about the validation protocol as

mentioned above. In any case, readers will have difficulty to understand how you validate your estimation by the DOAS.

L25(pp4571)-L10(pp4572): The same comments above apply here, too.

Table 2: The same comments apply here, too.

It may be much easier for reader to evaluate your validation results, if the results are presented in a form of a usual scatter plot, rather than in two tables.

4 Discussions and conclusions

Description of this section is a mixture of summary, discussions, conclusions, outlook, and something else. It was not easy for me to pick up what was the main conclusion of this paper. Please re-organise this section.

There are lots of irrelevant descriptions. For example, a role of diatoms on oceanic carbon cycling [(L24, pp4572 to L17 (pp4573)] as well as ecology of cyanobacteria (L20-29, pp 4573) have nothing to do with what was presented earlier in this paper (i.e. phytoplankton classification by the DOAS method using SCIAMACHY). These descriptions can easily be removed (Or be moved to Introduction).

L5-29, pp4574: The authors point out that other published methods (for phytoplankton classification) are empirical based on the data taken in the past, and that unexpected changes in nature in the future will cause a bias in the classification by these methods. This is possibly true. However, the authors discuss that the DOAS is not an empirical method but an analytical method, and therefore free from such a risk of the bias. Unfortunately, this does not sound either logical or right to me, because the DOAS presented in this paper needs a spectral fitting to reference spectra empirically determined from measurements. The empirically assumed reference spectra can also have variability due to unexpected change in phytoplankton physiology (e.g. pigment composition) induced by climate variability for example. The DOAS cannot self-correct such a change in the reference spectra, even if a broader spectral region is used in the fitting. Thus

the DOAS can also introduce a bias when the reference spectra determined empirically are not correct. In addition, Empirical Orthogonal Function analysis was used for some optimisation of the DOAS method presented here (if I understand this paper correctly), thus depending on data. In my opinion, the DOAS method presented here may involve more biological/physical mechanisms than other methods do, but it does not necessarily mean that the DOAS method is not empirical and is free from the problem which the other methods have.

L2-L16, pp4575: The whole paragraph is redundant here, because these discussions are not about what was presented in the earlier sections (Sections 2 and 3), although some sentences may fit to Introduction.

Interactive comment on Biogeosciences Discuss., 5, 4559, 2008.

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