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Ocean Science
Discussions

Interactive comment on "Improvements to the PhytoDOAS method for identification of major phytoplankton groups using hyper-spectral satellite data" *by* A. Sadeghi et al.

Anonymous Referee #1

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SUMMARY: The paper describes how the original PhytoDOAS method was expanded in an effort to retrieve additional phytoplankton functional types (PFTs). The original (single-target) approach (Bracher et al. 2009) retrieves cyanobacteria and diatoms. The retrieval described in this paper (triple-target) retrieves diatoms, coccolithophores and dinoflagellates. The coccolithophore retrieval is compared to the NASA NOBM model output and the MODIS PIC product. The diatoms are compared to the NASA NOBM model output. There is no comparison with the dinoflagellate retrieval.

GENERAL COMMENTS: While the paper is well written and organized, it lacks scientific rigor. It is highly important that new satellite products be validated with in situ

observations. The authors wave their hands at this aspect stating that there is not enough in situ data for a validation and there are limitations to the colocation of an in situ observation to the size of a SCIAMACHY pixel. However, Bracher et al. (2009) was able to find in situ data for a proper validation as well as many authors of other satellite phytoplankton functional types retrievals published in the literature. The comparison of the new PhytoDOAS PFTs with model output (NASA NOBM) and satellite derived products (MODIS PIC) is insufficient. The model output and derived satellite products have there own associated uncertainties. Validation with in situ observations of all the retrieved PFTs needs to be added to this manuscript before it should be accepted for publication.

There also needs to be a more rigorous statistical treatment as to how the triple-target configuration was determined. It seems that the authors tried various configurations and settled on the one with an appropriate chi-square and lowest residual. This is partially treated in Figure 5. However, this seems incomplete. It seems that a table to more clearly address the quantitative metrics used to make the configuration decision would be a more through treatment.

The use of various absorption spectra should treated uniformly. The mix of culture and natural sample spectra is sloppy. There should also be verification with literature that their own spectra match what has been reported in published literature.

SPECIFIC COMMENTS: It is not clear why cyanobacteria are not considered in the triple-target PhytoDOAS approach. Please add discussion about this topic.

Introduction – it needs to be explicitly called out that your use of the term phytoplankton functional types is in the context of taxonomic groups (dominant species). You clarify this on page 2286, line 2 but this needs to be stated in the introduction.

Section 2.1 – I think the description of DOAS can be substantially compressed with citation of the literature. It is not necessary to walk the reader though the DOAS equations if they are published elsewhere.

Page 2280, line 1 – ".. absorption and scattering of CDOM" – CDOM is dissolved and therefore does not scatter!

Section 2.2 – much of this has already been published by Bracher et al. (2009). This could be much more concise with citation to the literature.

Page 2284, line 28 to page 2285 line 2 - The sentence starting with "The phytoplankton absorption spectra used in this study. . ." is confusing. Do you mean that E. huxleyi was from culture while the dinoflagellates were from a natural sample? Please rewrite to clarify.

Section 2.3 – Why are you mixing cultures for one species with a natural sample for another? Are there absorption spectra for these species in the literature that could be used instead? If you do use a dinoflagellate natural sample, you need to identify what the predominant species is and include discussion about how variable dinoflagellate absorption spectra can be with various species of dinoflagellates.

Page 2285, line 25 – "right panel" should be "lower panel"

Section 2.4 – It is unclear why cyanobacteria were not considered in the multi-target approach. Please explain!

Page 2286, line 9-10: "multi-target fit" was previously defined, thus the definition does not need to be repeated here.

Page 2287 lines 12-18 – You point out how important comparisons with in situ measurements are. However, I don't find your statements about limited availability of in situ data and colocation of in situ observations sufficient justification not to do this comparison. Validation with in situ observations needs to be included!

Page 2288, line 8 – "(as one criterion)" – This leaves the reader wondering what the other criteria are? Please be explicit and add further clarification.

Section 3.1 – You only discuss the retrieval of E. huxleyi and dinoflagellates, but leave

out diatoms. Please add text to justify your reasons for not considering diatoms in your retrieval discussion or add discussion of diatoms . You use March and October as your comparison months. Please add statements as to why these months were selected.

Page 2290, lines 4-8 - The sentence starting with "The PhytoDOAS results,". . . This has previously been stated and does not need to be repeated.

Page 2290, lines 8-12 – I find this type of comparison insufficient for validation. Each of these products (NOBM model output and PIC derived product) have their own associated uncertainty. Where the PhytoDOAS does not match well with either the NOBM model or the PIC product, we are left no knowing if PhytoDOAS is performing poorly or if the model or PIC products are erroneous. This is why comparison to in situ observations is imperative!

Page 2291, lines 1-2 – You state, "However, the precise validity test should be done by converting the PIC concentrations into the concentration of living coccolithophore cells. . .". Then this needs to be done or do not include the comparison with the MODIS PIC products.

Page 2291, line 7 – "other comparisons". . . which are? Please describe.

Page 2292, lines 5-17 – The ideas in this paragraph need to be proven. The current treatment is insufficient.

Page 2292, line 13 – ". . .demanding more investigation. . ." Yes! This investigation should be presented here.

Page 2292, lines 23-24 – ". . .making them bot being sufficient for quantitative comparison." Then why do it? So what does any of section 3.3 tell us? You leave the reader with a lot of doubt.

Page 2295, lines 5-7 – You state, "The global distribution of dinoflagellates retrieved by PhytoDOAS must be compared with an appropriate data set of this taxonomic group." This needs to happen before this manuscript is accepted.

Figure 2 caption – You state, “The first two spectra were obtained from cultures. . .” The first two listed in the previous sentence are E. huxleyi and dinoflagellate. However in the text you said that the dinoflagellate spectra was taken from a natural sample. Which it is?

Figure 4 caption – The caption states “scaled to 0.1”. However the legend indicates “scaled by 0.75”. Which is it?

Figure 5 caption – “(1.07.2005)” Please write out the date. This notation could be confused by some.

Figure 11 – There needs to colorbars associated with the middle and lower panels.

Interactive comment on Ocean Sci. Discuss., 8, 2271, 2011.