

Interactive comment on “MODIS observed phytoplankton dynamics in the Taiwan Strait: an absorption-based analysis” by S. Shang et al.

Anonymous Referee #1

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General Comments

You present an absorption-based study of phytoplankton dynamics in the Taiwan Strait that consists of an initial assessment of the performance of the QAA and subsequent use of the derived absorption information in an analysis of local biophysical phenomena. A migration away from the use of chlorophyll *a* as the primary metric of phytoplankton biomass is becoming increasingly accepted, and, in this respect, your study is welcome and timely. However, the paper lacks narrative and sufficient reference to the plots and figures. Technical sections omit much vital information, making comprehension of your analyses very hard to follow in places, and impossible for others to repeat your approach should they wish to. Lack of a rigorous nomenclature in the manuscript also means that much of your discussions are unclear and laboured.

You do present some very interesting results relating metrics of phytoplankton absorption to an

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ENSO index, but this innovative work is underplayed by a lack of narrative. I do believe that this manuscript can quite easily be revised to produce a well-organised and focussed study that can make a valuable contribution to the field. I encourage you to consider the comments and suggestions below, and look forward to seeing a revised version of the paper.

Specific Comments

p. 7797, Introduction: This section needs to set the scene by providing context and motivation for your study. You should include a condensed review of the merits of using phytoplankton absorption as a superior metric of biomass compared to Chl *a*, and cite the relevant studies – e.g. Cullen (1982), Lee et al. (1996), Marra et al. (2007) or similar. Gordon et al. (1988) (or similar) should also be cited to support the assertion that it is the absorption of photons, rather than pigment concentration (which is simply a proxy for absorption), that is the primary controller of ocean colour. This section should also include a brief review of existing algorithms that allow retrieval of a_{ph} from R_{rs} . This section need not be lengthy, and can be presented in a short paragraph or two, but it will provide context for your decision to use an absorption-based approach that is currently lacking.

p. 7797, line 19: Please just refer to fig. 1 in parentheses. I'm not sure what grey lines you are referring to here.

p. 7797, line 20: Suggest rearranging this sentence to something like, “The TWS has complex hydrographic conditions determined by the relative influence of the South China Sea Warm Current (SCSWC) and the Kuroshio Branch Water (KBW), which are warm, saline, and oligotrophic, and the Zhe-Min Coastal Water (ZMCW), which is cold, fresh, and eutrophic, and varies seasonally in response to changes in the monsoonal wind (e.g., Jan et al., 2002).”

p. 7797 and throughout manuscript: The nomenclature used in the manuscript lacks rigour, and I strongly recommend that you revise it thoroughly using the following as a guideline. The first time a new parameter is introduced, it must be stated in full, followed by the symbol you have chosen to represent it with and its units. There are many instances throughout the paper where this fundamental convention is completely ignored. Using the sentence on p. 7798, line 4 as an example, it should read as follows: “For this study, we first derived phytoplankton absorption, $a_{ph}(m^{-1})$, from remote sensing reflectance, $R_{rs}(sr^{-1})$, by using a quasi-analytical bio-optical inversion algorithm (QAA, Lee et al., 2002; 2009).”

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You must also derive separate symbols to differentiate between in situ and MODIS parameters. If we take in situ and MODIS-derived a_{ph} as examples, I suggest using something like $a_{ph}^{in\,situ}$ and a_{ph}^{MODIS} respectively. Develop a similar set of nomenclature for R_{rs} and Chl a. A well-defined and unambiguous nomenclature will save you lots of page space and allow you to more easily discuss your results. You may also choose to include a table of symbols, definitions and units, which, in my opinion, is an invaluable aid to the reader.

p. 7797, line 10: Provide units for chl-a as per my comment above.

p. 7798, line 12: Units missing for nL_w .

p. 7798, line 15: Units missing for F_o .

p. 7798, line 18: What does Level-3 regional product mean? Please explain briefly what is done to the data in a Level-3 processing.

p. 7798, lines 20-27: This is the methods section, yet some very important detail is missing. How were daily wind stress and monthly mean wind stress vectors calculated? How did you decompose them into along- and cross-shore components?

p. 7799, line 3: R_{rs} can now be used instead of typing it out in full since you'll have defined it earlier in the text. Go through the text and where appropriate, use this convention for all other parameters discussed.

p. 7799, line 5: Units missing for L_u , L_{sky} and L_{plaque} .

p. 7799: Units missing for Δ .

p. 7799, line 15: Please add "Water" in front of "Samples".

p. 7799, line 18: No symbol or units provided for CDOM absorption. You provide a symbol on the next page (with no units), but it needs to appear here.

p. 7799, line 18: No symbol or units provided for particulate absorption.

p. 7799, line 19: You say here that you use a transmittance-reflectance technique for the determination of particulate absorption because some samples were collected near-shore. Why does that necessitate using this technique? Please explain.

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p. 7799, line 25: How was a_{ph} calculated? It's not sufficient to simply say that the samples were extracted in methanol. a_{ph} is the difference between particulate and extracted absorption, and you need to state this.

p. 7800, line 2: May I suggest that you replace the symbol for total absorption excluding water with something like a_{t-w} ? a_{t-w} is a little unwieldy. Also (again), please provide units.

p. 7800, line 3: Please omit "(Fig. 1)" – it's not required here.

p. 7800, line 4 and throughout: Please pay careful attention to the tense of your prose. It is standard practice to discuss your methods and results in the past tense. So here, I would suggest changing this sentence to, "This in situ data set covered a wide range. . ." Please check the tense of your writing throughout the entire manuscript.

p. 7800, line 11-13: This discussion of the different semi-analytical algorithms belongs in the introduction section (see my comment regarding this above). It should also be briefly stated here that the QAA allows retrieval of a_t , a_g , (which is absorption of detritus + CDOM) and a_{ph} . You should also explain why you have chosen to use the QAA instead of other existing algorithms. Adoption of the nomenclature suggested above will make this section much clearer and easier to write.

p. 7800, line 4: Fig. 2 is barely referred to. Do you really need it? However, see my comments later that suggest you might make better use of it in one of your analyses.

p. 7800, eqns. 2-4: The parentheses around 'S' and 'M' are unnecessary. Please omit them.

p. 7800, eqns. 3-4: While I understand that the relationship between measured and retrieved a_{ph} is often more clearly presented in a log-log plot (Figs. 3a b), I have serious misgivings about presenting your metrics of error in log space as they are not straightforward to interpret. I would strongly recommend that you recalculate RMSE and bias in linear space and produce a much more informative table 1 that can be easily understood. The plots can remain in log-log space.

p. 7801, line 13: Please omit the word 'even'.

p. 7801, line 14: How much better are your results from QAAv5 compared to the IOCCG (2006) results that used an earlier version of the QAA? Please state.

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p. 7801, line 17: No need to cite Lee et al. 2002 again. We know at this point who is the author of the QAA.

p. 7801, line 18: Melin must be written with the acute accent, i.e. Mélin. Please also amend in your bibliography.

p. 7801, line 18: I may be mistaken, but I don't think Mélin et al. reported their RMSE in log space.

p. 7801, line 19: Suddenly, Chl a derived from the OC3 algorithm appears here, yet you made no mention of it in the methods section. You must add a short description of these calculations, including all relevant details, to the methods section. Please also develop a suitable set of symbols to differentiate between in situ and MODIS-derived Chl a.

p. 7801, lines 23-27: Why can you estimate a_{ph} better than Chl a? What are the fundamental reasons for this? This is an important point and underpins your decision to undertake an absorption, rather than a pigment, -based analysis of ocean colour.

p. 7802, 1st par.: This section is 'launched into' rather suddenly. I suggest starting with a linking sentence that explains why you've performed these analyses – something like, "In order to investigate the temporal behaviour of a_{ph} , we calculated monthly mean values of..." Check the rest of the manuscript for instances where the addition of simple introductory sentence could improve the clarity of your prose substantially.

p. 7802, 1st par.: I have read this section numerous times and had a lot of difficulty understanding what you had done. I think I eventually figured it out by reading the figure caption! Suffice it to say, your description is very unclear, lacks important detail and is confused even further by a lack of rigorous nomenclature that would help us discern in situ from MODIS, and measured from derived.

If I've understood correctly, you are attempting to demonstrate that, in your study area, retrieval of a_{ph} can be fairly accurately achieved, but that Chl a retrieval is confounded by CDOM and detritus. However, you take a very long time to tell us this, and I'm not sure that you need to undertake this spatial averaging exercise to prove it. Is it possible to examine the relationship between your error metrics and the concentrations of CDOM, e.g. a regression of RMSE for Chl a^{MODIS} against a_g ? This may also make fig. 2 a more useful figure. Once this has been

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established, you could then integrate temporal and spatial effects by performing the averaging and anomaly calculations you describe. However, if you decide that these calculations are useful and informative, they MUST be explicitly described. Specifically, we require descriptions of how you calculated monthly mean $a_{ph}(443)$, and spatial means and anomalies of all parameters. You must also show explicitly what the δ parameter is. As before, appropriate and unambiguous nomenclature should be derived to differentiate between the different types of parameters. I would also suggest that the symbol for monthly mean $a_{ph}(443)$ be changed from A_{ph} to something like $\bar{a}_{ph}(443)$ with a bar over the top, or some other symbol to indicate that it is a mean value.

Please consider these suggestions carefully and decide if your current figures are necessary and pertinent to your objective.

p. 7803, line 2: Fig. 5 isn't really discussed at all. Do you really need it? If you decide to keep fig. 5, please refer to the white line and white box at appropriate points in the text to point the reader to the relevant parts of the image.

p. 7804, line 18: Please change 'at' to 'in'.

p. 7804, line 23: Make sure you tell us somewhere how along shore wind stress anomaly is calculated.

p. 7804, ABI description: This is an interesting section.

p. 7804, line 23: You tell us that ABI and along shore wind stress anomaly are well correlated, but I think we need to see quantitative evidence of this. You may choose to make a plot of ABI vs. along shore wind stress vector, but at the very least, tell us what the descriptive statistics are, i.e. R^2 , p , n .

p. 7805, discussion of fig. 7b: Please make sure you refer to each aspect of fig. 7b as you're discussing it. You should tell us that the grey bars represent the number of valid pixels, and we are never told either here or in figure, what the blue and red curves represent (I assume MEI). Fig. 7b, although informative, is pretty busy. Consider splitting out some of the information.

p. 7805, lines 14-15: What does, "...the relationship between the Asian monsoon and ENSO is mutual but selectively interactive..." mean?

p. 7805, line 28: Please define what a frontal probability is. It's never stated.

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- p. 7805, line 27: Please change 'grater' to 'greater'.
- p. 7811, table 1: Strongly recommend RMSE and bias are calculated in linear space.
- p. 7812, fig. 1: Suggest adding a key that explains that circles are satellite R_{rs} match ups, and crosses are in situ R_{rs} match ups.
- p. 7813: fig. 2: This figure is not really referred to, but may be useful if you perform an analysis relating a_g to metrics of retrieved Chl a error.
- p. 7814, fig 3: Strongly suggest relabeling these plots to be consistent with a revised nomenclature scheme.
- p. 7815, fig. 4: The explanation in the text of how this plot was derived is very poor. If you revise how you do the analysis, this plot may change substantially. However, if you keep it, the key is VERY misleading. Omit the 'vs.' completely – it suggest you've done a regression. If you define δ rigorously, you'll be able to come up with a better key.
- p. 7816, fig. 5: Carefully consider if this plot is strictly necessary.
- p. 7817, fig. 6(a): You must provide units on the colour bar. Also, the latitude and longitude are invisible against the plot colours in some places. Consider moving the labels and ticks to the outside of the plot.
- p. 7817, fig. 6(b): Suggest adding 'Coastline' after 'most variable', and 'deep water' after 'least variable' for clarity.
- p. 7818, fig 7(b): There is no information to tell us what the red and blue curves are.

Suggested Reading

Cullen, J.J. (1982). The deep chlorophyll maximum: Comparing vertical profiles of chlorophyll a. *Can. J. Fish. Aquat. Sci.*, 39, 791-803
 Gordon, H.R., Brown, O.B., Evans, R.H., Brown, J.W., Smith, R.C., Baker, K.S., Clark, D.K. (1988). A semianalytic radiance model of ocean color. *Journal of Geophysical Research*, 93, 10909-10924

Lee, Z.P., Carder, K.L., Marra, J., Steward, R.G., Perry, M.J. (1996). Estimating primary production at depth from remote sensing. *Appl. Opt.*, 35, 463-474
 Marra, J., Trees, C.C., O'Reilly, J.E. (2007). Phytoplankton pigment absorption: A strong predictor of primary productivity in

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the surface ocean. *Deep Sea Research Part I: Oceanographic Research Papers*, 54, 155-163

Interactive comment on *Biogeosciences Discuss.*, 7, 7795, 2010.

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