

**Biogeosciences Discuss., 7, C4647–C4654, 2011, “MODIS
observed phytoplankton dynamics in the Taiwan Strait: an
absorption-based analysis”.**

By S. Shang et al.

We deeply appreciate the reviewers' time and effort to help improve the manuscript. We have revised the manuscript accordingly. Below are our replies to the detailed and constructive comments/suggestions.

Reviewers' comments:

Anonymous Referee #1

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 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.

Reply: Thank you for your encouragement and the very detailed and constructive comments and suggestions. Revision throughout the entire manuscript has been done.

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.

Reply: We have revised the introduction according to your suggestion, including a review of absorption relevant studies and the existing algorithms for absorption retrieval. Detailed as below:

Using phytoplankton absorption, instead of Chl, as a superior metric of phytoplankton pigmentation is becoming increasingly accepted (e.g. Cullen, 1982; Marra et al., 2007), especially from the remote sensing point of view (Lee et al., 1996; Hirawake et al., 2011). This is because the direct controller of ocean color is the spectral absorption and scattering properties of the water media (e.g. Gordon et al., 1988) rather than pigment concentrations, although the variations of the latter will change pigment absorption in a non-stable fashion (e.g. Bricaud et al., 1998, Stuart et al., 1998). However, few studies based on *in situ* measurements exist to test whether a_{ph} can be derived from satellite ocean color data with less uncertainty than Chl. Such evidence is vital in order to confirm that a_{ph} can function as the preferable index for characterizing phytoplankton variability in the upper ocean.

For this study, we first derived a_{ph} from remote sensing reflectance (R_{rs} , sr^{-1}) with the quasi-analytical bio-optical inversion algorithm (QAA, Lee et al., 2002; 2009). In addition to QAA, there are several algorithms available for the retrieval of absorption and backscattering coefficients from R_{rs} (IOCCG, 2006). Here we used QAA because of its transparency in the analytical inversion process and simplicity in implementation.

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

Reply: Corrected. We also changed the grey solid lines to grey dashed lines in Fig.1 to better differentiate the boundary for the TWS area, which was used for spatial mean calculation, from the other lines.

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)."

Reply: Thank you. Revision has been done according to your suggestion.

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 aquasi analytical bio-optical inversion algorithm(QAA, Lee et al., 2002; 2009)".

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}^{insitu} 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 pages pace 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.

Reply: Thank you for your kindly help. We have revised the whole manuscript thoroughly, and included a table of symbols (Table 1 in the revised manuscript; also shown below). However, we chose not to derive separate symbols to differentiate between *in situ* and MODIS parameters, since discussions of properties derived from *in situ* R_{rs} and MODIS R_{rs} only occur in Section 3.

Table 1 Symbols, abbreviations and description

Symbol	Description	Unit
ABI	Areal Bloom Index	m^{-1}
a_{ph}	Absorption coefficient of phytoplankton; $a_{ph}(412)$ means a_{ph} at 412 nm; $a_{ph}(443)$ means a_{ph} at 443 nm	m^{-1}
<i>Aph</i>	$a_{ph}(443)$	m^{-1}
a_{t-w}	Total absorption without pure water contribution; $a_{t-w}(443)$ means a_{t-w} at 443 nm	m^{-1}
Chl	Chlorophyll <i>a</i> concentration	mg/m^3
MEI	Multivariate ENSO Index	
QAA	Quasi-analytical Algorithm (Lee, et al. 2002)	
<i>RMSE</i>	Root mean square error	
R_{rs}	Remote sensing reflectance	sr^{-1}
TWS	Taiwan Strait	

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_0 .

Reply: The above three items have been corrected. Thank you.

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.

Reply: We have omitted “regional”. There is no specific meaning. A brief explanation for Level-3 processing has been provided.

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?

Reply: We have provided a brief description to explain how the wind stress and its alongshore component were calculated. The details of the drag coefficient calculation can be found in Yelland and Taylor, 1996 and Yelland et al., 1998. The whole thing is lengthy, shown as following:

U_{10} was the wind speed at 10 m height above sea surface from QuikSCAT.

When $3m/s \leq U_{10} < 6m/s$, the u and v component of daily wind stress (u_stress and v_stress) were calculated as:

$$u_stress = 1.225 \times \left(0.29 + \frac{3.1}{U_{10}} + \frac{7.7}{U_{10}^2} \right) \times U_{10} \times \overline{U_{10u}} \times 0.001$$

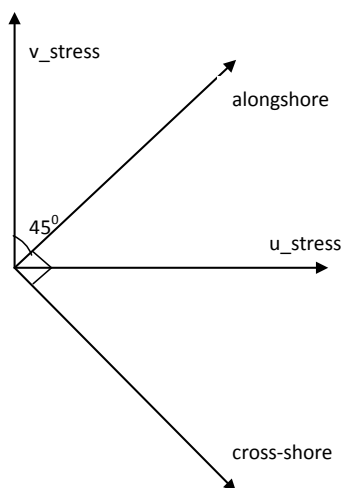
$$v_stress = 1.225 \times \left(0.29 + \frac{3.1}{U_{10}} + \frac{7.7}{U_{10}^2} \right) \times U_{10} \times \overline{U_{10v}} \times 0.001$$

When $U_{10} \geq 6m/s$, u_stress and v_stress were calculated as:

$$u_stress = 1.225 \times (0.6 + 0.07 \times U_{10}) \times U_{10} \times \overline{U_{10u}} \times 0.001$$

$$v_stress = 1.225 \times (0.6 + 0.07 \times U_{10}) \times U_{10} \times \overline{U_{10v}} \times 0.001$$

Where $\overline{U_{10u}}$ and $\overline{U_{10v}}$ were the u and v component of U_{10} , respectively.



Daily wind stresses in a month were averaged to generate monthly mean wind stress vectors. They were then decomposed into alongshore and cross-shore components by doing a simple vector manipulation, as the chart shown.

We have also provided details on the calculation of monthly mean and the anomaly in Section 2.

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.

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.

Reply: The above six items have been corrected. Thank you.

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.

Reply: A brief explanation has been provided: "These samples were rich in highly scattered non-pigmented particles. The standard T-method will thus cause an overestimate of sample absorption (Tassan and Ferrari, 1995)."

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.

Reply: A brief description has been provided: "Detrital absorption (a_d , m^{-1}) was therefore obtained by repeating the modified T-R measurements on samples after pigment extraction by methanol (Kishino, 1985). a_{ph} was then calculated by subtracting a_d from a_p , and the combination of a_p and a_g yields an estimation of a_t ."

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

Reply: We keep the symbol " a_{t-w} " since total absorption " a_t " is well defined, it would be confusing if using " a_t " to represent total absorption without water.

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

Reply: Changed.

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.

Reply: Thank you for the correction. We have checked the tense 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.

Reply: We have moved this to the “Introduction” section and explain why the QAA was chosen. Details are in the above answer to “p. 7797, Introduction”.

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.

Reply: We admit that Fig.2 does not add much to this study. It has been omitted.

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

Reply: Corrected. We have also changed “S” to “r”, representing retrieved data; and “M” to “f”, representing field data.

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 plot (Figs:3ab), 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.

Reply: We have recalculated *RMSE* in linear space and provided the results in Table 1 (current Table 2 in the revised manuscript). But we still choose to report RMSE in log space in the text. The reasons are 1) for such kind of datasets, the value of RMSE in linear space is highly affected by the range of the data so that it is not good for judging differences in mismatch between derived and known data among different properties we concern; 2) it facilitates direct comparison with results ever reported. We also omitted bias since they were barely referred to.

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

Reply: Corrected.

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.

Reply: We have stated in the text. The *RMSE_log* for $a_{ph}(443)$ in the IOCCG report was 0.321 and ours was 0.150.

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.

Reply: The above two have been corrected.

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

Reply: We checked the result reported by Mélin et al., the *RMSE* they used (the symbol was ' Δ ' in the paper) was 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.

Reply: A short description of OC3M has been added in the method section. We are sorry that symbols to differentiate between in situ and MODIS-derived Chl are not developed. If they are generated, they will be only used in a short paragraph.

p. 7801, lines 23-27: Why can you estimate a_{ph} better than Chla? 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.

Reply: We have added an explanation in the revised manuscript.

“This analysis of match-up uncertainties clearly indicated improved performance of R_{rs} -retrieved A_{ph} over Chl in the TWS. One fundamental reason for such results is that R_{rs} is largely determined by the absorption and scattering properties of all the optically active materials in the water, of which phytoplankton is simply one of them (Mobley, 1994; IOCCG, 2006). Higher uncertainty associated with Chl is thus anticipated while trying to retrieve Chl by simple spectral ratio of R_{rs} in marine waters where the contribution of non-phytoplankton components is significant (e.g., TWS).”

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 Chla 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 $Chla^{MODIS}$ against a_g ? This may also make fig. 2 a more useful figure. Once this has been 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 $\overline{a_{ph}(443)}$ 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.

Reply: We hope the following modifications could be more satisfactory. 1) We have separated this section from Section 3. Now it is Section 4 as a report of the spatial pattern differences among MODIS derived properties. The reason to report this is due to the concern that the evaluation results shown in Section 3 was merely a comparison of discrete match-up samples in the TWS and most of the R_{rs} data used in the analysis were *in situ* measurements rather than MODIS measurements. We believe such a comparison of MODIS spatial patterns is useful and informative. 2) Starting from this section, no daily MODIS properties will be used. All discussions are on monthly mean values. We have chosen to mention this clearly at the beginning of this section without introducing a new symbol like $\overline{a_{ph}(443)}$ with a bar over the top. 3) Relevant descriptions of temporal and spatial averaging, and anomaly calculation, etc., have been supplied in the method section.

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.

Reply: Fig.5 is omitted in the revised version.

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

Reply: Corrected.

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

Reply: It has been supplied in the method section.

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

Reply: Thank you.

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.

Reply: We have added the descriptive statistics in the text ($R^2=0.67$, $N=7$).

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.

Reply: We have revised Fig.7b, splitting it into three sub-plots (currently Fig.5b, shown below). We have also paid attention to refer to each aspect of the figure while discussing about it. The text has been revised accordingly.

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

Reply: The relationship between the Asian monsoon and ENSO is actually not clear and under debate. When fluctuations of the monsoon and ENSO are observed, nobody knows who is cause and who is effect. Our understanding of the "...the relationship between the Asian monsoon and ENSO is mutual but selectively interactive..." is that there are tele-connections between them but they are not always interactive. A

fluctuation on one of them may not definitely result in a fluctuation on the other.

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

Reply: It has been provided in the method section.

p. 7805, line 27: Please change 'grater' to 'greater'.

Reply: Corrected.

p. 7811, table 1: Strongly recommend *RMSE* and bias are calculated in linear space.

Reply: *RMSE* in linear space has been calculated.

p. 7812, fig. 1: Suggest adding a key that explains that circles are satellite R_{rs} matchups; and crosses are *in situ* R_{rs} matchups:

Reply: It has been explained in the figure caption.

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 Chla error:

Reply: Fig.2 is removed.

p. 7814, fig 3: Strongly suggest relabeling these plots to be consistent with a revised nomenclature scheme.

Reply: The plots have been relabeled.

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.

Reply: Corrected.

p. 7816, fig. 5: Carefully consider if this plot is strictly necessary.

Reply: It has been omitted.

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.

[Reply: Corrected.](#)

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

[Reply: Thank you very much for your help.](#)