

Interactive comment on “Photophysiological state of natural phytoplankton communities in the South China Sea and Sulu Sea” by W. Cheah et al.

Anonymous Referee #2

Received and published: 8 September 2013

The manuscript presents Chl-a, phytoplankton pigment, light absorption and photophysiological parameter data along a transect in the South China Sea and Sulu Sea. These properties are compared to environmental gradients (temperature, salinity, macronutrients, light and wind). The paper is generally well written and the data are clearly presented. However, the analysis and interpretation of the data is rather weak. I didn't feel that anything new was learnt beyond reporting the data. Although the data are certainly of interest, I would question whether the manuscript is insightful enough for publication in Biogeosciences in its current form. I recommend either a more rigorous interrogation of the data and/or better use of the literature to provide stronger evidence of what is learnt in the current study. I have included some general and specific comments that I hope the authors will find useful in a revision.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

General comments:

The description of the data seems quite labored and some figures seem unnecessary. For example: - The dissolved oxygen data does not seem to contribute to the main focus of the manuscript (which is photophysiology). If it is not needed for the story, I suggest removing it. - Are both the absolute (Fig 6) and chl-a normalized pigment concentrations (Fig 7a,b,c,d) really needed? I also didn't see the need for both Fig 10A-F and Fig10G-L, which illustrate the same data? - Perhaps it would help to have a clearer objective or hypothesis, so that it is more obvious what the data are being used for.

Interpretation of the data was mainly via a correlation analysis, but unfortunately I did not feel that much was learned by this approach. Co-variability in photophysiological properties and environmental variables, such as T, S, nutrients, could be seen quite clearly in the figures. Whether or not the correlations are significant seems, to me, rather arbitrary. Also: - The main drivers of photophysiological properties were light and nutrients (P12135 L1-6) but light was not part of the statistical analysis. - Be careful not to assume causation from correlation. For example, in Section 3.8: "These results indicate that an increase in Si supplied by river outflows (stations close to the coasts at Kuching, Kota Kinabalu and northeast of Sabah) contributed to higher concentrations of diatoms and cyanobacteria (Fig. 4c), in which affecting the abundance of Prochlorococcus at the surface." Why would Si availability be important to cyanobacteria or Prochlorococcus? - Some of the interpretation of the stats seems contradictory, e.g.: P12134 L11: "significant correlations between Si and Fuco . . . , Si and Zea . . . , and a negative correlation between Si and DVChl a were observed at the surface" P12134 L 19: "there is no significant correlation between Si and major phytoplankton pigments".

Specific comments:

P12119 L4: Were there any contamination issues when collecting nutrient samples

BGD

10, C4872–C4876, 2013

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



from a moonpool? (Can you provide some resistance in the text?). Also, please state the limit of detection for nutrient measurements.

P12119 L19: Please include some information about calibration of the CTD sensors (incl. oxygen).

P12121 L1-25: I found this sections hard to understand. Can it be simplified? - I suggest removing “a total of 10 PAR profiles were measured at 9 stations. During the cruise, Station 1 and 3 were very shallow to determine zeu which was confirmed by both zeu and zeuPAR. For Station 5, PAR profiles were measured at two daytime casts while TChl a measurements were only available on a third cast. Hence, the comparisons between zeuPAR and zeu for Station 5 were carried out based on the average of the PAR profiles (68.05 m; the two cast were only 1 m apart). So finally” So that the sentence reads: “The zeu values were then validated with the collocated zeuPAR values. Out of 14 CTD stations, seven stations with collocated zeu and zeuPAR could be compared in order to verify the zeu values...”

P12122 L17-21: What were the implications of only collecting FRRFf blanks in the surface mixed layer and chlorophyll maximum? Please provide some assurance that this simplification is adequate? i.e. how big were the blanks, how much did they vary compared to the unfiltered samples and were they representative of the assumed layers?

P12122 L26: 13cm and 25cm Whatman GF/F filters seems rather large for HPLC and, particularly, phytoplankton light absorption measurements (where analysis is normally conducted on in tact filters)? Are these sizes correct?

P12123 L7: The phytoplankton community structure information is entirely dependent on a CHEMTAX model. Were the modeled phytoplankton community structures ground trothed in any way? If not, please provide additional reassurance that the CHEMTAX starting points from Zhai et al. 2011, which were for winter conditions, were reliable for this application.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

P12126 L10-L28: Please be specific when using the terms “TChla” and “biomass” when describing the HPLC –derived phytoplankton community structure. TChla is initially defined as “total chlorophyll-a” but is this really what you mean when talking about the Chl-a contributions of the different phytoplankton? Also, please avoid using the term biomass when you really mean Chl-a. It would be helpful to state the property in the unit (i.e. mg Chl-a m⁻³ rather than mg m⁻³) to avoid confusion. In particular, please specify whether you are talking about chlorophyll (mg Chl-a m⁻³) or carbon biomass (mg C m⁻³) in Fig 5.

Section 3.4 and 3.3. I suggest swapping these sections around (and also swap Fig 5 and 6). It makes more sense to describe pigment concentrations first because those are actual measurements, while community structure is a derived quantity.

P12127 L7-L16: I suggest moving the sentence “A linear regression of $r^2 = 0.89$ ($p < 0.05$, $n = 75$) was observed between surface HPLC-derived TChl a and Fm-derived TChl a. It shows that Fm-derived TChl a is applicable in this study region. Subsurface TChl a maxima (SCM) were observed between 30–80 m, mostly below the mixed layer and above the euphotic depth” to the methods section.

P12128 L27: change “shows” to “suggests”, because there is no direct evidence of different strains.

P12129 L 9: Please define VAZ on first use.

P12129 L5-30: This section could do with a bit of work. To what extent did the changes in pigment concentrations reflect different community structure versus photo acclimation? Better use of the literature (e.g. Griffith and Vennell 2010 Nature Proceedings; Alderkamp 2011 DSRI) would help make the evidence for xanthophyll cycling much more convincing.

P12130 L14-15: “For deeper offshore stations, aph were usually lower at the surface and increase with depths (Fig. 8c–e). This occurs when cells are acclimated to low

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

irradiances at deeper depth, leading to subsequent increase in pigmentation and less efficient absorption per mass of pigment Falkowski et al. (1985)”. The second sentence contradicts the first here. The data show an increase in a^*_{ph} with depth while Falkowski refers to a decrease in a^*_{ph} with depth as a result of the package effect. I am intrigued by the observed increase in a^*_{ph} with depth (in Fig 8), which is unusual and does not seem to fit with the other data presented (in stratified water columns with subsurface chlorophyll maxima containing larger cells, a^*_{ph} would be expected to decrease with depth). Is the data correct? If so, what is the explanation?

Section 3.7: Better use of literature on mixing vs. acclimation timescales (of which there is a lot, e.g. by Cullen, Marra, Falkowski, Geider, Moore and many others) would help make this a more robust discussion.

P12133 L26: What do you mean by “disappearance ratio”?

Figures: - Is a map needed in Fig 2 and 4? Can the information be combined into Fig 1? - Is it necessary to quote both the CTD number and the station number? I suggest using one or the other (or, better still, something more meaningful like latitude). Fig 7: please include units, g:g? mol:mol?

Interactive comment on Biogeosciences Discuss., 10, 12115, 2013.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)