Biogeosci Review MS BG-2013-336
Title: Photophysiological state of natural phytoplankton communities in the South China Sea and Sulu Sea
Author(s): W. Cheah et al.

This MS examines phytoplankton photophysiological variability along an environmental gradient in the South China and Sulu Seas. Overall, the idea behind the MS is sound – this is not a well-studied region and currently efforts to model primary productivity for this region are confounded by a lack of understanding of the underlying photobiology. Despite having a great data set, the MS suffers from a lack of direction and it's impossible to know what the readers are supposed to take away. Primarily, the MS lacks clear (overall as well as specific) aims/objectives/hypotheses; therefore, it is difficult to follow what the authors are really "looking for" through their data analysis, which in itself suffers form a lack of depth (and is presented/examined in a very 1-dimensional manner). I was frustrated and disappointed as I read on through the MS since this is a very nice data set that could really be used to explore something more novel and elegant. There is definitely a paper in here somewhere but it needs a major re-think. At present, I cannot recommend that the MS is published but should be resubmitted as a new MS for re-review. Most of my comments at this stage reflect what the authors perhaps need to (re) consider in getting much more out of the data they have and build on recent papers that have moved forward our understanding as to how the environment regulates phytoplankton communities and (photo)physiology.

General Comments
1. It is not clear just what the aim of the MS is. It reads as a data paper, which is fine given this is not a well studied region (but is it enough for the impact factor of Biogeosciences?) but it really comes across as confusing in what the authors are attempting to do by describing the patterns. Are the trends they observe expected and consistent with what one would expect for the environmental gradients encountered? – similar type studies have been published from other seas/oceans, including complex environmental gradients, that should perhaps be used to better inform the study up front, e.g. hypothesis formulation. The authors appear to have an interesting (unique?) oceanographic environmental setting and should draw more on this. As such, the introduction needs to be more detailed and well-thought through to inform specific aims aims.

2. The MS is undermined by two major problems associated with the Figures/data analysis (i) The quality of the figures makes them very difficult to examine critically and pull out the key trends within and across the environmental grandient(s); (ii) the data is analysed in a very 1-dimensional manner such that it's impossible to see the trends and patterns. I'm really surprised that the authors don't try to elaborate on some of the more elegant approaches used previously e.g. Moore et al. 2005 DSR that do a really nice job of linking the environment and the photophysiology across environmental gradients. As presented, this does not feel as though the MS (ands the unique data set for this region) is really moving the field forward. On this note, the data analysis really needs a re-think. Much of the analysis is univariate and the only attempt to quantify patterns is restricted to a series of correlations (Table 3) that provides no insight as to how the complex multivariate environment is operating to regulate the taxonomy over the physiology. The authors really need to take a more

multidimensional space type approach; either relatively ‘simplistically’ such as that used by Moore et al. (2005) (whereby values for parameters are visualised in multidimensional space – I appreciate the authors are trying to do this for the cast data via the contour plots but these do not really convey the complexity and so more treatment of the data (binning) is really needed to extract the key points) – or a more complete multivariate statistical analysis. I’m really surprised the data analysis has not been taken to this level to help streamline the complexity of the issues at play. The key trends/messages are lost but again I feel this reflects that the lack of clear messages upfront confounds just how the data needs to be treated.

Specific Comments
PG12116LN5. The abstract states that “This study investigates thephotophysiological state of natural phytoplankton communities” but it needs to clearly describe why this is important to do.

PG12118LN3. The authors begin to distil to the main issue in the introduction by stating that little “photoadaptation information” exists with which to inform models but this train of logic falls short since it is not clear (i) what is this information specifically and (ii) how the study here has been designed (i.e. the set of measurements used) to address this limitation.

PG12119LN3. I was intrigued by the idea of sampling from a moon pool. What is the residence time of water here and is there any delay between where the ship is sampling and the water that is comprising the moon pool. Later in this paragrah I did not understand why samples for only pigments and absorption were taken from these shallow sites. In fact, it’s entirely unclear the (mis)match between when the ‘standard’ oceanographic and FRRf measurements wre made and when absorption/HPLC samples were taken – this needs to be better explained and when (and when not) there are matched samples available.

PG12121LN1. I appreciate that the detail associated with the relationship between TChla and Fm and dealt with in section 2.6 but it would be good to see the resulting trends (or at least report the regressions/correlation details and how well they hold across sites once NPQ is accounted for); on this note also, why use Fm and not F0 or even Fv? There are various arguments behind how well each of these parameters can/should relate to biomass and a well reported issue and the authors should perhaps explore this in a little more detail to identify the most robust approach based on their fluorescence data.

PG12122LN1. There’s some issues with the FRRf approach that need to be clarified: (i) what was the processing software used to derive the fluorescence parameters (if the default Chelsea software then this is known to be erroneous – see papers by Sam Laney – and in which case the error here again will need to be sensitised); (ii) The approach to yield qP by Suggett et al. 2006 is actually the ratio of the PSII yield from the light and dark chambers – this is the most robust approach to yield qP from FRRf deployments in situ since F0’ cannot be strictly determined. The authors need to be more clear as to what they have done here; (iii) the prime for the fluorescnec parameters determined under actinic light should follow the fluorescence parameter and not be embedded halfway through e.g. sPSII’ not s’PSII (or Fm’ not F’m) as they currently read; (iv) blanks seem logical but need a reference perhaps to support the in situ blanking approach (perhaps Smythe et al 2004?) – even so, it would be good to know what typical % of the signal was represented by the blank (i.e. how significant it was).

I did not follow sections 3.3. and 3..4.1 (and to some extent 3.4.2) whereby there are separate sections on phytoplankton community (obtained from diagnostic pigments) and then on the changes of the major pigments. Seems like the same issue is really being considered at least twice and there could be much better integration here to state how the major pigments changes and how this relates to (i) changing community versus (ii) changing physiology. Section 3.4.2. in terms of "acclimation" makes sense but is almost impossible to tease out the influence of taxonomy issue – it may make more sense to simply define what are the photosynthetic versus non-photosynthetic pigments and how these rations are changing? Again, a more transparent analysis of these features in relationship to the environment would really help. At this stage, why not use this pattern of logic to better link the taxonomy to the physiology. Suggett et al use a neat approach in their 2009 paper (and sure the authors attempt to build on this through their Fig. 10 but it is currently ineffective since depth may not be the key variable required to interpret the variability – perhaps look at diagnostic pigments of taxa versus acclimation here instead?). The authors can bin this data in all sorts of ways depending on the main questions being asked of the data and so much more could be done here.

On a similar note, section 3.6. currently only superficially considers whether qP or sPSII (i.e. NPQ) are driving how the cells are primarily responding to environmental change but again so much more could be done to examine whether the two co-vary (but normalised in some way) or whether one is much more 'plastic' than the other; similarly, how these actually vary relative to the changing light environment across environments. The authors also have the data on the xanthophyll pigments so why not analyse these in relation to the fluorescence parameters rather than speculating that they may be involved?! This is yet another example that I feel the authors still have a lot to do with this data set to not only do it (and their hard work in collecting the data) real justice but also highlight more transparently what are the novel messages here. Later sections (e.g. photoacclimation) are just too superficial without the authors really getting their teeth into the detail earlier.