Biogeosciences Discuss., 11, C2077–C2081, 2014
www.biogeosciences-discuss.net/11/C2077/2014/

Biogeosciences Discussions
Open Access

Interactive comment on "Disparities between *Phaeocystis* in situ and optically-derived carbon biomass and growth rates: potential effect on remote-sensing primary production estimates" *by* L. Peperzak et al.

Anonymous Referee #2

Received and published: 3 June 2014

General comments

Throughout the review, I use (X:Y) to refer to page X and line Y of the print version of the discussion paper.

This study utilizes small mesocosms with controlled conditions to examine the performance of optical proxies for *Phaeocystis* biomass, physiology, and growth rates through the transition from exponential to N-limited stationary growth. Unsurprisingly, changes in Chl-specifc absorption, the C:Chl ratio, and Chl fluorescence induced by N-limitation lead to uncoupling in the relationships between optical proxies and parameters mea-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

sured on discrete water samples. The authors conclude from these results that such disparities cast doubt on the utility of satellite-derived estimates of these quantities on a global basis.

My first general comment is somewhat pedantic, but I have to question if the term "mesocosm" is an accurate description of this approach? Mesocosms are generally considered to be enclosures of natural (i.e., multiple species) aquatic communities in which environmental conditions can be manipulated to study ecosytem community response. The only natural part of this study system is the filtered seawater used to fill the vessel; a single species of phytoplankton is then inoculated and there are no other phytoplankton species, microbial community, grazers, etc. In my opinion this experimental setup is just a rather large (140L) batch culture, not a true mesocosm.

The authors assertion that such conditions can be used to evaluate current satellite-based approaches on a global scale is to me very much an overstatement.
1. The results are for a single species only, and cannot be considered representative of the response of a complex phytoplankton assemblage of taxa found in the true environment. Except for very rare conditions, the observed results with *Phaeocystis* will nearly always be mitigated by the presence of other species, species succession, and other ecological processes which are not represented in the experimental design.
2. The experiment is a short-term batch culture in which nutrient conditions are constantly changing (drawdown and crash, with an additional nutrient spike at the end). Thus, the experimental design requires physiology to be adapting to constantly changing conditions, with no opportunity for acclimation to occur. Physiological responses during steady-state growth (i.e., continuous culture) are considerably different than observed in batch culture with intermittent nutrient supplies. There is still considerable debate on whether the real ocean more closely resembles a steady-state or a pulsed environment, but one has to question how representative the experiment is of the real environment.

The overarching claims that these experiments can answer such global questions is

BGD
11, C2077–C2081, 2014

thus much too simplistic and needs to be toned down. That said, I do not dismiss the value that such experiments provide in explaining general trends or certain ecological scenarios, for example in interpreting behavior observed during the wax and wane of a *Phaeocystis* bloom in a given locale. In my opinion, the authors need to be more realistic about the usefulness of their study for real world applications.

In that light, I would also remark that there is not a lot of new or novel information presented in this study. The main conclusions seem to be that Chl is not a good measure of phytoplankton carbon biomass as the C/Chl ratio changes with N-limitation, as do variable fluorescence or optical fluorescence yields. There is a long and rich history of studies examining phytoplankton responses to nitrogen limitation, including their effects on cellular elemental and pigment quotas, which are not referenced in the manuscript (e.g., Shuter 1979, Laws and Bannister 1980, Geider 1987 to name just a few of very many). There are also (fewer) studies focusing on the response of phytoplankton optical properties to N-limitation (e.g., Kiefer et al. 1979, Chalup and Laws 1990, Sosik and Mitchell 1991, Reynolds et al. 1997), including specifically studies of other *Phaeocystis* strains (e.g., series of papers by T. Moisan). Numerous papers also describe changes in chlorophyll fluorescence yields under nitrogen or other nutrient limitation (e.g., Kiefer 1973a,b, Green et al 1991, 1992, Babin et al. 1996). The authors do a poor job of placing their results within the context of this extensive literature.

A final general comment pertains to a lack of information provided on cell size and morphological characteristics observed during the experiments. Particle size is critical to understanding variations in particle optical properties, yet there is no mention of it in the paper. Although I am unfamiliar with this particular isolate, many species of *Phaeocystis* are known to form large colonies (up to 3 cm diameter) which will have a dramatic effect on the interpretation of changes in the bulk optical properties of the culture. The authors performed (presumably microscopic) cell counts, and it would be useful to note if individual cell size or colony formation was changing throughout the

BGD
11, C2077–C2081, 2014

course of their experiments.

Specific comments

(6121:25) - Fv/Fm and φ_F are NOT equivalent. Fv/Fm is based on a functional definition of light absorption energy delivered to PSII. φ_F is based on total absorption by all pigments, including ones that do not transfer energy to the photosystems.

(6122:19) I understand that for many details related to the experimental setup the reader is referred to other papers, but at least some of the very basic information needed to interpret the results of this study must be included here. What are the dimensions and volume of these microcosms, and in particular the depth? Are the enclosure sides clear or opaque? Such information is needed to interpret radiometric measurements, light propagation, and the light environment for growth.

(6124: 14) The above inquiry regarding mesocosm depth and clarity of the enclosure sides relates to the calculation of "absorption" by phytoplankton. Is the culture optically thin at all times? If Chl concentration is 30 μg/L and the pathlength is long, then there are problems with interpreting the loss of light as absorption. It is not described here or in the Appendix how one goes from attenuation to *Phaeocystis* absorption. Is CDOM absorption accounted for (I note that this species is known for producing large amounts of mucous material)? For that matter, I do not see where this information is used in the manuscript, all results appear to use absorption calculated from the Reflectance measurements and model.

(6124: 18) The calculation of phytoplankton quantum efficiency, φ_F, here and in the appendix needs further elaboration. What is the wavelength range over which the *Phaeocystis* "absorption" signal integrated? Is this total cell absorption, or specific to chlorophyll a only? For that matter, how can the fluorescence quantum efficiency be greater than 1 (as indicated in Fig. 5)?

BGD
11, C2077–C2081, 2014

I would question whether such calculations critical to the manuscript (absorption, fluorescence, quantum efficiencies) should be relegated to an Appendix. Why interrupt the narrative and force the reader to stop and hunt for them at the end of the paper?

(6125: 6) All estimates of POC and PON used in growth rate calculations assume that no bacteria or other microorganisms exist in the cultures. Was this verified?

(6126: 3) I have a difficult time keeping track of which measurements are used in different calculations and why. "Absorption" coefficients are determined 3 ways that I can see; measured with water samples (ICAM), derived from light attenuation through the mesocosm, or computed from Reflectance measurements. Each of these is interpreted in a slightly different way (total pigment absorption vs. chlorophyll a or c absorption), some are spectrally-resolved and some represent unspecified integrals. How do these various absorption coefficients intercompare, and why bother with an uncertain modeled Reflectance-based estimate when you have ICAM + HPLC pigment estimates? In the authors previous paper they report such algorithms have about 75-80% accuracy, but acknowledge that the performance changes with physiological state of the cultures.

(6127: 21) I do not understand the utility of normalizing POC to Chl a+c. As far as I am aware, most existing algorithms for computing primary production generally employ some means to first estimate Chl a, which is then converted to phytoplankton Carbon assuming a C:Chla ratio. The need to interpret the C:Chla+c seems to me an additional and unneccessary complication.

(6145: Fig. 3) Please specify the wavelength range over which these aborption measurements correspond.

(6147: Fig. 4). How can the fluorescence emission F be in irradiance units? To be consistent with the described derivation and Table 1, the units should be sr^{-1}.

Interactive comment on Biogeosciences Discuss., 11, 6119, 2014.