The paper presents a comparison of optical and what the authors refer to as "standard oceanographic techniques" to estimate growth of phytoplankton. The work is carried out using previously described mesocosms equipped with radiance and irradiance sensors.

General comments.

Problems with the casting of the paper. By reading the introduction it is not clear to me the exact question the authors wish to address. The authors mention that phytoplankton carbon cannot be measured directly from space, acknowledging Behrenfeld et al. (2005) paper but not a series of paper coming from the same group since. They also mention the variability of the C:chl ratio as a limit to the chlorophyll-based approach to remote sensing of ocean carbon concentration. A known limitation that has been acknowledge extensively in the literature as a limit to the estimate of photosynthetic parameters to include in primary production models.

They also mention the quantum yield of fluorescence (φ) as a proxy used for estimates of growth rate of phytoplankton. I do not know of any proposed application of φ as a measure of growth rate nor of anyone using it as such (the authors do not cite appropriate literature for their statement). Some have proposed it as a measure of primary production (the authors do not cite this literature either). In any case, limitation of this method have been pointed out a long time ago (e.g. Kolber and Falkowski 1993). Finally, the statement that φ is the equivalent of Fv/Fm is simply not true.

Given these problems with the introduction, it makes it difficult to understand exactly what the authors wish to address in the rest of the paper. Is it some key remaining unanswered question in the literature or is it that they perceived previous results as incorrect and they wish to "set the record straight". In short, it reads like a dataset in search of a question, not a question addressed with a dataset. The authors seem to have decided to avoid vast section of the literature, that not pertaining to Phaeocystis, to make their point (authors such as Geider [e.g. 1996, 1997, 1998], Cullen [e.g. 1988], Falkowski [many before ~1997], Behrenfeld and many others do not get a fair treatment). I feel that a lot more care needs to be taken to correctly cast this paper and show the relevance of the study in the context of past and present literature.

Methods are extremely succinct and refer extensively to a previous paper. Presently the paper must be read with the paper of Peperzak (2011). I think the methods should be developed such that enough is present in the paper to avoid to referring extensively to the previous paper. For examples, geometry of the measurements should be repeated here.

Furthermore, I could not understand for example how the Fv/Fm measurement was carried out. Was it done under ambient light or not? If not, how long, if any, was the dark acclimation period?

The fluorescence algorithm does not seem to address that there is reabsorption of fluorescence within the volume of the water before it reaches the detector nor is it clear what is the "total" absorption used for these calculation. Was it only phytoplankton or does it include water (in Fig5 caption it reassuringly mentions "total phytoplankton absorption" but he methods do not state that)?

All this needs to be made very clear.

The phytoplankton dynamics are not well explained and some statements cannot be made. In section 3.1 the authors skip over the observation that after day 8, both mesocosms (not only

mesocosm 1 where nutrients were added) showed an increase in POC while every other measurement decreased in mesocosm 2 (and Fv/Fm was very low, limiting the possibility of photosynthetic carbon fixation). Since the authors use POC as a measure of phytoplankton carbon and their reference measure of growth rate, such surge in the growth rate in mesocosm 2 in nutrient starved cells must be explained. Also the statement that the number of cells is greater in mesocosm 2 and in mesocosm 1 due to the different light levels cannot be made as there could be a range of other reasons why this happened beyond light.

Specific comments

1) The symbol φ changes to φ_{ph} in section 3.4. Sometimes both are used in the same sentence e.g., P. 6131, line 24.
2) To me Figure 5 is probably the most interesting result in this paper showing a relatively constant quantum yield even under starvation (nutrient enrichment not having a strong effect). Very high levels of starvation bringing values that are similar to nutrient replete (days 2 and 3). However, this result is not discussed in the context of relevant literature (e.g. Cleveland, J. S. and Perry, M. J. 1987, Laney, S. R., Letelier, R. M. and Abbott, M. R. 2005) and many others which have inferred nutrient stress from *in situ* measurements (e.g. Cullen and Letelier groups).
3) P.6131, line 11. It is not clear at all what the loss of correlation correspond to? There is to my eye a non-linear and fairly nice relationship between φ and growth rate. I note, however, that the quantum efficiency values presented in Figure 6 do not match the values presented on Figure 5.

References

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Laney, S. R., Letelier, R. M. and Abbott, M. R.: Parameterizing the natural fluorescence kinetics of Thalassiosira weissflogii, Limnol. Oceanogr., 50, 1499-1510, 2005