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Biogeosciences Discussions

Interactive comment on "Inferring phytoplankton carbon and eco-physiological rates from diel cycles of spectral particulate beam-attenuation coefficient" *by* G. Dall'Olmo et al.

Anonymous Referee #2

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Overview

This study proposes an original method to compute the volume specific daytime and nighttime losses, the volume-specific gross growth rate, the cell size distribution, the carbon biomass and the net diurnal productivity of a population of phytoplankton cells responsible for one observed diel cycle in the beam attenuation coefficient. The dataset analyzed consists in multi-spectral beam attenuation measurements. Results are also interpreted using flow cytometry and ADCP measurements.

The paper is of interest to the readership of Biogeosciences. The paper is generally well written. However, I found some notations confusing. Some paragraphs content

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too many shortcuts. This degrades the quality of the way the study is presented.

The contribution presented here is novel in at least three points: (i) the interpretation of the spectral variability in the beam attenuation is interpreted in terms of biogeochemical information which are difficult and/or time-consuming to measure in-situ, (ii) the beam attenuation is partitioned into a background and diel variable component, (iii) the cells population responsible for the diel variable component is identified.

The authors however use (too?) many approximations in order to adapt a previously published model to interpret the observed beam-attenuation diel cycle. Outlined below are a series of general and specific comments that should be addressed to improve the manuscript by providing more observational evidences to discuss these approximations.

General comments

1. Please explain how cp has been computed from the measurements. It is not clear how the particulate beam attenuation coefficient (cp) has been computed. The ACs instrument provides total absorption (a) and total attenuation (c). The contribution of pure water (cw) and colored dissolved organic matter (ag) has to be removed from the total beam attenuation coefficient to compute cp. It is not indicated how the values of cw and ag have been computed. It seems to me that this is a study of cpg, not of cp.

2. Why don't you examine the diel variability in bp? The effects of attenuation and scattering should be separately examined. Currently, the interpretation of the cp diel cycle does not take into account the contribution of dissolved and particulate absorption (apg). This reduces the validity of the assumptions that (i) cp could be modeled using a power law of wavelength, (ii) the spectral variability of cp is related to the slope of the particle size distribution (PSD), and (iii) the diel changes in spectral cp are mostly related to changes in particles size. Accordingly, the following points should be addressed:

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(i) Is it correct to model cp using a spectral model when the contribution of absorption might be significant? Indeed, the error analysis simulations conducted by Boss et al. (2001) concludes that "Absorption effects are important [..] when a larger number of small absorbing particles will be present", which is likely the case here because of the presence of Synechococcus and small eukaryotes in the particle community.

(ii) The spectral distribution of the scattering coefficient is a power function of wavelength if two successive conditions are met (Morel, 1973): 1) The PSD follows a power law function, and 2) Particles are non-absorbing. The limits of these assumptions should be mentioned: 1) The use of a power-law PSD has been challenged by several studies (Jonasz and Fournier, 1996; Reynolds et al., 2010). 2) The second assumption is not valid in the visible range because most marine particles, and in particular phytoplankton, are absorbing. As a result the imaginary part of the refractive index is high enough to impart on the scattering coefficient (Babin et al., 2003, Doxaran et al. 2007).

(iii) Changes in ap are mostly related to changes in pigments concentration and composition, which can occur independently of changes in Particle Organic Carbon (POC), cell size and/or real refractive index.

3. Why don't you provide two additional graphics showing (i) the spectral cpg and bp at different times of the diel cycle (e.g. at sunrise, noon, sunset, midnight, following sunrise), and (ii) the temporal variation of their spectral slope for days 195 to 198? It would support (or not) the assumption of using a power-law model for spectral cpg and bp. If a power-law model is indeed appropriate, then it would be also useful to show also the diel cycle of the spectral slope for the days 195 to 198.

4. The partition of cp between a background and a diel component is one of the good points of the paper. Combining flow cytometry and ACs data, it should also be possible to partition the diel component of cp into the contributions of nano-1, nano-2, syn, euk and bact (e.g. DuRand and Olson, 1996). This would strengthen the conclusions of

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the paper and consolidate the interpretations of the model results. It would also make it possible to discuss one of the major assumptions of your model, i.e. that only one population of cells is responsible for the observed cycle in cp.

5. It would improve the paper to compare the results of your computationally heavy model with more simple models linking particle growth and beam attenuation diel cycle (e.g. Marra 1995, Cullen et al. 1995, Gernez et al. 2011).

Specific comments

3013:17-27 and 3014:1-3 This paragraph is not accurate enough. Please rewrite it in order to take into account the points mentioned in general comments #2(i) and #2(ii).

3014:14-16 What about the variability in the size of heterotrophic picoplankton? From measurements performed during the same cruise, Talarmin et al. (2011) suggest that mixotrophy should be considered in studies of primary and bacterial production.

3015:9-21 This paragraph is not accurate enough. What is the spectral range measured by the ACs? More importantly, it is not clear how cp has been computed from ACs measurement because it is not clear how the contribution of pure water and of dissolved substances has been computed (see also general comment #1).

3016:18-28 The symbols and subscripts are misleading. (i) The symbol r is not appropriate for a number of classes. I would rather use N. Moreover, as the symbol r is often used to design a growth rate, this is confusing to use it for something else. (ii) I do not like the subscript d for “division” because it is confusing with “diel” or “daytime”. This is all the more confusing as cell division is (incorrectly?) assumed to occur during night-time. (iii) Symbol DI is not appropriate to design a probability, please use another symbol.

3016:26-27 Is it really inconceivable that cell division can also occur during the day?

3017:6-9 What is the size range encompasses by the 64 classes: 1 to 30 um?

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3017:12-13 Is it realistic to assume that the growth rate is uniform among size classes? Chisholm (1992) points out that the growth rate might be size-dependent.

3019:7-10 Why do you assume that the imaginary part of the refractive index is negligible? Moreover, the real part of the refractive index is not constant during the day. Stramski and Reynolds (1993) report a diel variation between 1.036 and 1.055 at 660 nm.

3019:20 Eqs. 7 and 9. Again, the notation is misleading as subscript "d" in cpd is likely to be confused by the "d" of detritus. I would suggest to replace cpd by cp1 and write cp(lambda, t) = cp0(lambda) + cp1(lambda, t) in Eq. 9.

3019:21 How do you reconcile Eq.1 and Eq. 7? Is the spectral slope the same for cp0 and cpd?

3022:16 and Fig.4. What day is Fig. 4? Why don't you show the temporal variability of cp for the other days (i.e. 195 to 198)? How do you explain the dramatic drop in cp at 4:00? For example, cp(550) drops from 0.078 to 0.075 in less than one hour, which is about 30% of the total diel variation. In order to highlight the spectral difference between cp(550 nm), cp(630 nm) and cp(710 nm), it would be useful to show the full spectrum of cp at key times of the diel cycle for day 196, such as for example: sunrise, noon, sunset, midnight, following sunrise.

3022:22-26 I may have missed a point, but I do not understand why the fact that parameters are (in)dependent of the refractive index suggests that the estimation of parameters is (in)correct. The explanation for that comes too late (3025:8-16).

3022:26 How do you determine which pair of refractive index and average size is correct between the three options shown in Table 3?

3023:10 Why do you select n = 1.05 and not n = 1.02 or 1.08?

3025:8-16 I think some parts of this paragraph should be moved in the method section.

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3025:23-24 Why should the average population cell size be independent of losses?

3026:4 (Eq. 12) Why do you scale by fd (i.e. the illuminated fraction of the day)? Doing so the ratio fd / (t2-t1) is equal to 1.

3026:13-14 There is no point comparing your volume specific growth rate g with the diurnal rate of cp variation: they are two different quantities. Applying Eq. 6 of Gernez et al. (2011) to your data of Fig. 4 gives a diurnal rate of variation around 0.19 d-1 at 630 nm, which is consistent with their observations at 660 nm (Gernez et al., last line of table 2).

3027:16-21 How do you explain that, according to the results of the model, Synechococcus does not significantly contribute to beam attenuation diel cycle? Your Fig. 3 displays conspicuous diel variation for the side scatter of Synechococcus.

3028:2-3 Oubelkheir et al. (2005) reports a cp* of 1.78 m2 (gC)-1 for the Mediterranean Sea.

3028:23-26 The partition of cp into the contribution of nano-1, nano-2, euk, syn and bact using flow cytometry data should allow you to discuss the assumption that only population of cells is responsible for the diel cycle in cp (see also DuRand and Olson 1996 and general comment #4).

Table 1 is not self explanatory. Please define all parameters of the table, including T, DI, rb.

Table 2. Why do you use the central 68th percentile rather than standard deviation? Are xrb0 and vrb0 identical?

Fig.4. It would be more appropriate to model bp(lambda, t) rather than cp(lambda, t). It would be useful to show more observations before showing the results of the model. I would suggest to add three figures before the current Fig. 4: (i) temporal variation of bp(t) and/or cp(t) at 440, 550, 630, and 710 nm for days 195 to 198, (ii) spectral variation of bp(lambda) and/or cp(lambda) at various time of the diel cycle (e.g. 4:00,

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8:00, 12:00, 15:00, 19:00, and 22:00) for day 196, and (iii) temporal variation of the spectral slope of bp(lambda) and/or cp(lambda) for days 195 to 198.

Fig.4. How do you explain the dramatic drop in cp(t) at 4:00?

Technical comments

3019:14 (Eq. 6) Is beta_i a function of wavelength?

3022:12 patterns

3022:24 Are vrd0 and vrb0 identical?

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