Biogeosciences Discuss., 4, S2393–S2396, 2008
www.biogeosciences-discuss.net/4/S2393/2008/

Biogeosciences
Discussions

Interactive comment on "Particle optical backscattering along a chlorophyll gradient in the upper layer of the eastern South Pacific Ocean" *by* Y. Huot et al.

G. Dall'Olmo (Referee)

giorgiod@science.oregonstate.edu

Received and published: 16 January 2008

General comments

This study analyzes the relationships between chlorophyll-a concentration (Chl) and the magnitudes and spectral shapes of the particulate backscattering (bbp) and scattering (bp) coefficients of the South Pacific gyre. The important new findings of this paper are:
1) it confirms that, and provides parameterizations for, the relationships between Chl and the magnitudes of both bbp and bp follow the general trend that was previously found in other less oligotrophic waters (i.e., both bbp and bp monotonically decrease

with Chl).

2) Chl can be used (to first order) to predict bbp or cp with similar accuracy.

3) An interesting, although "not validated", relationship based on experimental data is presented that shows that the spectral slope of the bbp:bp may vary with trophic state.

The manuscript is clearly written and I believe all the above findings are important and deserve to be published.

I have only few comments that, I believe, should be addressed:

I found confusing the use of two bbp data sets that were processed in different ways. I have read the papers by Twardowski et al. (2007) and by Stramski et al. (2007) and find that the differences in the processing methods are rather significant (e.g., the different χ factors used, the fitting procedure applied to the Stramski et al. data set). Despite these differences, the derived bbp values appear to be in good agreement (within 4%, according to Twardowski et al., 2007). Nevertheless, Fig. 6 shows that the two data sets provide a rather different view of the bbp:bp vs. Chl relationship. The "Twardowski et al." bbp:bp shows no dependence on Chl, while the "Stramski et al." does. What is the cause of this different behavior? More importantly, which behavior is most consistent with the "real world"? At this point it is not clear.

I think the manuscript would become clearer if the processing methodology of the bbp data was standardized for the two data sets. Alternatively, I would probably make an effort to merge the two data sets and consider them as a single one.

Finally, it may be worth presenting a more detailed discussion on why Chl could predict bbp or bp equally well. What are the implications of this important result? Could the authors discuss it for example in terms of variations in the particle size distributions (FD) that were measured during the cruise? From a recent paper by Loisel et al. (JGR VOL. 111, C09024, doi:10.1029/2005JC003367, 2006) one sees that the FD showed a steep change in the Junge-slope around 1 μm which suggest that the small particles may not covary with large ones.

Minor comments

I read the Stramski et al. 2007 manuscript and found what I think is an inconsistency with their Rrs measurements. In the caption of their Fig. 8 they reported an intercept for the linear relationship between Rrs and bb/(a+bb). The intercept seems to be quite significant as it is almost as large (0.0007 1/sr) as the whole range of variation of the reported Rrs (0.0021-0.0013=0.0008 1/sr). However, we would expect Rrs to be negligible when bb/(a+bb) is zero. Is this inconsistency due to a bias in the reflectance measurements or in the bbp or both? Given the good agreement found by Twardowski et al. (2007) between the BB3– and Hydroscat–derived bbp, it seems that the Rrs data could have been biased. This point may be worth discussing.

The use of a power law of the form $y = ax^b$ implies the absence of an intercept in the relationship between y and x. This intercept, if significant, may provide interesting insights and consistency checks. For example, given that bbp and bp respond to particles belonging to different size fractions, I would expect to find a positive intercept in the bbp vs. bp relationship. Why was this specific form of power law used? Were the intercepts of the relationships presented in the manuscript always negligible?

Fig.1: there seems to be a clustering of the points presented in panel C along two different relationships. A group of points follows closely the M-2b model, while another lays below the red line. Why? It would be nice to see if such difference is related to the depth at which the samples were collected.

To help the reader better appreciate the uncertainty associated with the equations reported in table 1, the RMSEs for log-transformed data may be translated into more understandable “typical” relative uncertainties.

Pg 4581, 2nd par.: “However, the model provides a reasonable description of the slope and amplitude within its uncertainties.” This sentence is not clear.

BGD

4, S2393–S2396, 2008

It is stated that the bbp model based on eq. 2a is the one that "apparently best fits this data set". This was surprising to me since that model supposedly included data collected in the North Atlantic where coccolithophores might be responsible for the observed larger scattering coefficient per unit of Chl. It might be useful to discuss this point.

Pg. 4581, 3rd par.: "some changes in Hydroscat data processing have occurred between the different datasets due to improvements with time in the approach." It is not clear how much of the differences reported in fig. 2 can be attributed to these changes in data processing.

Pg. 4583, 1st par.: it could be useful to provide an estimation of the uncertainty with which the proposed models (equations 8) predict bbp as a function of Chl.

Pg. 4585, eqs. 11: why are these two equations presented when previously an instrument-independent average model for bbp was proposed (eqs. 8a and 8d)?

Pg. 4585, lines 20-21: the relationship between bbp:bp derived from Hydroscat data and Chl shows less scatter than the one derived using the BB3 data. It is not clear if the reduced scatter in the Hydroscat relationship is supposed to better represent the reality than the more dispersed relationship obtained from the BB3.

Moreover, I really think that it may be worth presenting the typical (relative) bbp:bp uncertainties in the derived associated with these Chl-based models.

Pg. 4586, lines 3-6: Why is the "proposed model" now used instead of the two separate equations for BB3 and Hydroscat? How would the two instrument–specific bbp:bp model behave?

Interactive comment on Biogeosciences Discuss., 4, 4571, 2007.