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Biogeosciences Discussions

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Interactive Comment

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.

Y. Huot et al.

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We thank Dr. Dall'Olmo for his thorough review and comments which have contributed to improving the paper.

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).
We agree that the difference in the processing methods are significant. However, some of these differences originate from the different geometries of the instru-

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ment (χ factor), others are possible on only one dataset instrument due to its greater spectral resolution. Others yet, cannot be modified after the cruise. We thus opted to use the data as they were processed in their original presentation.

Despite these differences, the derived bbp values appear to be in good agreement (within 4%, according to Twardowski et al., 2007).
The comparison made in Twardowski 2007 was at 470 nm, the only spectral band common to both dataset (462 nm for BB3 and 470 for Hydroscat). It was also carried out on the whole datasets (all depths) and thus present an "average" value, the difference between the two datasets varies with waveband (strongest difference near 550 nm) and tends to be greater at the lowest bbp values.

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"?
We cannot identify the reason for the different behavior at low chlorophyll concentration nor is there a clear rationale to believe that one is better than the other. The fact that they generally agree (see Fig 6) in those waters with very low [Chl] and provide reasonable value for the backscattering probability is reassuring to us.

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 effor t to merge the two data sets and consider them as a single one.
We kept the presentation of the datasets separate so that they could be compared easily. However, the proposed relationships to compute the backscattering coefficient merges the two dataset (i.e. Figure 3 and equations 8a and 8b;

note that equation numbers changed from previous version).

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?
Please see response to reviewer 1 we have added a paragraph to the discussion regarding this question. Essentially, our dataseet does not provide an explanation for this observation.

Could the authors discuss it for example in terms of variations in the par ticle 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 par ticles may not covar y with large ones.
Without an exhaustive analysis of more of these distributions, we find it difficult to go further than has already been done in the present document. However, in contrast to that presented in Loisel et al., because most of the particle size distributions measured during the cruise, do not extend to sufficiently small size to make conclusive interpretation in terms of backscattering we did not carry out such an analysis. We note, however, that a steep change in the slope as observed in Loisel et al. does not imply the absence or a low covariation between the smaller and larger size classes.

Minor comments
I read the Stramski et al. 2007 manuscript and found what I think is an inconsistency with their R_{rs} measurements. In the caption of their Fig. 8 they repor ted an intercept for the linear relationship between R_{rs} 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 R_{rs} (0.0021-0.0013=0.0008 1/sr). However, we would expect R_{rs} to be negligible when bb/(a+bb) is zero. Is this inconsistency due to a bias in the reflectance

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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 R_{rs} data could have been biased. This point may be wor th discussing.

Note that this remark actually refers to the paper of Stramski et al. (2007) and not to the present paper (which does not use absorption or in situ remote sensing reflectance data). There is no need for modification in our manuscript; however, Dariusz Stramski was willing to address this remark fully as follows: The reviewer states that "The intercept seems to be quite significant as it seems to be almost as large....as the whole range of variation of the reported R_{rs} ..... However, we would expect R_{rs} to be negligible when $b_b/(a+b_b)$ is zero". This reasoning is inaccurate and we believe that it has led the reviewer to the erroneous impression that our data are biased. In reality, $b_b/(a+b_b)$ and R_{rs} never approach zero. Under natural conditions in extremely clear ocean waters, $b_b/(a+b_b)$ approaches the value for pure seawater and R_{rs} approaches the value for an ocean consisting of pure seawater.

To illustrate this case, we made several simulations of radiative transfer within a hypothetical "pure seawater" ocean in the spectral range from 350 to 600 nm using 10 nm band intervals (consistent with FWHM bandwidth in our optical instrumentation). We used the Hydrolight code (Mobley, 1994) with Raman scattering included, which is especially important for these types of simulations. We tested several boundary conditions with solar zenith angle less than 40 deg, wind speed 5-15 ms^{-1}, and clear skies (plus one extra simulation with overcast skies). Within the green spectral region of interest to this discussion, the variation in R_{rs} in response to changing boundary conditions is very small. The average values from our simulations are:

band 540-550 nm, central wavelength 545 nm, $R_{rs}(545)$ = 0.0012645 sr^{-1};

band 550-560 nm, central wavelength 555 nm, $R_{rs}(555)$ = 0.0010303 sr^{-1};

band 560-570 nm, central wavelength 565 nm, $R_{rs}(565)$ = 0.0008847 sr^{-1}.

We have here shown the bands adjacent to the 555 nm band to illustrate how

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rapidly R_{rs} changes with wavelength in the green spectral region for the pure ocean (20% change between 555 nm band and the adjacent bands). This is an important fact to keep in mind considering that the optical measurements are made with the FWHM bandwidths of 10 nm. We estimated that the actual effective centroid wavelength in our radiometric measurements of reflectance within the green spectral region can be slightly shorter (1 nm) than the nominal center wavelength of the bands.

Having the results from Hydrolight simulations we can now turn to Fig. 9 from Stramski et al. (Biogeosciences, in press). The lowest data points in this figure, from the South Pacific Subtropical Gyre, actually approach the pure water case. At 555 nm, the ratio $b_{bw}/(a_w+b_{bw})$ for pure seawater is about 0.0145. In this estimate we assumed the b_{bw} value from the computations based on the work of Buiteveld et al. with salinity adjustments as described in Stramski et al. (Biogeosciences, in press). In fact, we here use an average value of b_{bw} for the range of surface water temperatures and salinities as measured on the BIOSOPE and ANTXXIII/1 cruises. However, the small variations in b_{bw} induced by variations in temperature and salinity are not important for our present arguments.

The regression line for all data from Fig. 9 would predict R_{rs}(555) = 0.0012502 sr^{-1} for the pure water value of $b_{bw}/(a_w+b_{bw})$. The R_{rs}(555) estimates obtained with the regression coefficients corresponding to the lower and upper 95% confidence intervals are 0.0010262 sr^{-1} and 0.0014741 sr^{-1}, respectively. We recall that the average R_{rs}(555) from the Hydrolight simulations is 0.0010303 sr^{-1}. It is thus important to note that this Hydrolight value is within the 95% confidence interval of experimental estimates (albeit admittedly close to the lower end of the confidence interval).

If we treat the ANTXXIII/1 and BIOSOPE data separately, the Hydrolight estimate of R_{rs}(555) = 0.0010303 sr^{-1} is well within the 95% confidence interval of experimental data shown in Fig. 9. For the ANTXXIII/1 data set we obtain R_{rs}(555) =

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0.0011448 sr^{-1} for the best fit regression and the 95% CI range of 0.0007704 - 0.0015456. Similarly, for the BIOSOPE data set we obtain R$_{rs}$(555) = 0.0012650 sr^{-1} [95% CI = 0.0009887 - 0.0015456]. We also note that these two data sets were obtained using different radiometer instruments and measurement methodology (free-fall profiling and near-surface float), yet the pure water estimates derived from each data set are statistically indistinguishable.

In conclusion, this analysis demonstrates that the extrapolation of the experimental data presented in Fig. 9 to the case of pure ocean is consistent with theoretical predictions based on radiative transfer simulations. Although it is impossible to totally exclude the possibility for some bias in the data of R$_{rs}$(555), b$_b$(555), and/or a(555), the above analysis indicates that any existing possible bias is undetectable within the 95% confidence interval of the regression analysis.

The use of a power law of the form y = axb 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 par- ticles belonging to different size fractions, I would expect to find a positive intercept in the bbp vs. bp relationship.

With the information available on particles size distributions in the ocean, we do not find any compelling reason to expect or not to expect an intercept in the b$_{bp}$ vs b$_p$ relationship, even if b$_{bp}$ and b$_p$ respond more strongly to particles in different size classes.

Why was this specific form of power law used? Were the intercepts of the relationships presented in the manuscript always negligible?

We used the power law fit as it provided an accurate representation of the data over the whole range of [Chl]. A significant intercept would be seen as a departure from the fit, this is not observed here in either datasets.

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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.
These clusters correspond to two different regions along the transect one closer to the marquises islands and the other near the other side of the Gyre. Michael Twardowski is preparing a paper that will examine in more details these variations observed along the transect.

To help the reader better appreciate the uncer tainty associated with the equations repor ted in table 1, the RMSEs for log-transformed data may be translated into more understandable "typical" relative uncertainties.
We added to table 1 a column for the mean absolute percent error, which shows on average roughly 20 to 25% error in the estimates of bbp and bp for the BIOSOPE cruise.

Pg 4581, 2nd par.: "However, the model provides a reasonable description of the slope and amplitude within its uncertainties." This sentence is not clear. It is stated that the bbp model based on eq. 2a is the one that "apparently best 64257;ts this data set". This was sur prising to me since that model supposedly included data collected in the Nor th Atlantic where coccolithophores might be responsible for the observed larger scattering coefficient per unit of Chl. It might be useful to discuss this point.
We have changed the sentence to "While the fit and models do not coincide, the models provides a reasonable description of the slope and amplitude within their respective uncertainties". We do not find very useful a discussion of the reason why our data are closer to the fit that includes the North-Atlantic. There is just too many unknowns and it rapidly becomes speculative.

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Pg. 4581, 3rd par.: 8220;some changes in Hydroscat data processing have occurred between the different datasets due to improvements with time in the approach.8221; It is not clear how much of the differences repor ted in 64257;g. 2 can be attributed to these changes in data processing.

We have reworked this paragraph and removed this sentence. Indeed it did not further the discussion. We now simply present the data as being the results from different published studies. The origin of the differences be they instrumental, regional, or calibration differences are not discussed as they are unknown.

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

We have added the following sentence to the document : "This relationship when applied to the whole dataset (n=858) provides an average absolute percent error (see table 1 for definition) of 25.2%, an RMSE on the decimal log transformed data of 0.134 and a coefficient of determination of r^2=0.87. " We provide the similar statistics for eq. 9.

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)?

Equation 11 present backscattering efficiencies while eq 8 presented the backscattering coefficient. We now give the average values for the backscattering ratio of all data taken together in Table 2.

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.

We don't know either. See response to reviewer 1.

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

We now provide the following sentences regarding the uncertainties. "For the whole BB3 dataset, including 650 nm, the expression has a mean absolute percent error of 28.4% and an coefficient of determination of r^2=0.05, the latter reflecting the absence of variability with [Chl]."
and
"For the whole Hydroscat dataset, the expression has a mean absolute percent error of 16.5% and a coefficient of determination of r^2=0.28."

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 instrument8211;specific bbp:bp model behave?

We have now dropped this section following the comments of reviewer 1.

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