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Biogeosciences Discussions

Interactive comment on "Direct contribution of phytoplankton-sized particles to optical backscattering in the open ocean" *by* G. Dall'Olmo et al.

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General comments:

This is an interesting study which represents the line of research aiming to advance an understanding of scattering budget in the ocean, the relationships between the particulate beam attenuation coefficient and particulate backscattering coefficient, and the potential use of these scattering properties as proxies for phytoplankton biomass as an alternative to the use of chlorophyll-a concentration.

The problem of scattering budget is particularly difficult to address in a rigorous fashion because no adequate experimental and theoretical tools exist to provide the various

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pieces of required information. For example, no tools exist to measure the complete particle size distribution, quantitatively separate the various particle size fractions, or model scattering by marine particles that are often complex in shape and internal structures. This study suggests that phytoplankton-sized particles have greater contribution to the backscattering coefficient than theoretically predicted from Mie scattering calculations of homogeneous spherical particles. This scenario is possible and not necessarily surprising if one recognizes the fact that Mie scattering calculations (apart from assumptions about particle shape and homogeneity) have never been based on actual measurements of particle size distributions covering the entire size range that is optically significant, as such measurements simply do not exist. Although significant role of phytoplankton-sized particles is possible, the weakness of this result in this study stems from limitations of the fractionation approach.

Several types of problems in particle size fractionation by filtration are recognized in the Discussion section, and the authors offer some considerations to provide circumstantial evidence that such fractionation can be useful to reveal relative contributions of different particles to optical properties of seawater. Whereas it is difficult to argue that this approach may have some usefulness, in my opinion the fractionation with filters is not quantitative and dependable. An example of unreliability is seen in Figures 9 and 10 (for example, higher particle counts were obtained for the <1 um fraction than the <3 um fraction in Fig. 10). These kinds of issues have long been recognized and I think that it is the most important reason why such fractionation experiments have been rarely pursued in the past, and if they were they generally lacked the high level of scientific rigor required to lend confidence in quantitative results of fractionation. In my opinion, in order to generate more confidence in the presented fractionation approach, careful methodological experiments to fractionate samples with the various filters along with good Coulter measurements (i.e. large enough sample volumes to get large enough particle counts over the relevant broad size range) are needed to show how the fractionation actually works. The authors show Figure 10 with particle size distributions for fractionated samples, in which the data are noisy (apparently the sam-

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ple volumes examined were too small) and do not clearly show how the fractionation works (although an obvious problem is seen in terms of the <3 um counts being smaller than the <1 um counts). I therefore think that this study has a significant methodological deficiency because it does not include good Coulter data for fractionated samples. Such data would reveal the extent of possible limitations. For example, typical filter retention characteristics are not sufficiently sharp at the nominal pore size to provide a quantitative size separation and these characteristics depend not only on the filter type but also filtration conditions and most likely even the composition of particulate assemblages. I am afraid that these limitations undermine the results of fractionation experiments in this study. I recognize that the authors have not made adequate Coulter measurements on the cruise but I do not understand why special laboratory tests with fractionation and Coulter measurements were not done and presented in the paper to address the fractionation methodology which is so critical to this study.

This paper also emphasizes a correlation between the particulate beam attenuation and particulate backscattering coefficients. The authors suggest that owing to this correlation the backscattering coefficient derived from satellite data could serve as a proxy for phytoplankton abundance or biomass, similarly to information provided by particulate beam attenuation. It is not clear what is meant by "abundance" or "biomass". Is the actual number concentration of phytoplankton cells or the amount of phytoplankton carbon implicitly implied in these terms? I think it would be highly problematic to claim that these concrete measures of abundance and biomass can be derived from these optical properties. Therefore, some comments would be in order on how the inherent optical property can be interpreted as phytoplankton biomass in the context of specific biological questions.

Although the task of correlating the measurements of beam attenuation and backscattering coefficients is quite straightforward, I have reservations regarding the methodology of using flow-through measurements instead of in situ measurements. It is generally known that marine scientists with strong interest in the analysis of uncontami-

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nated seawater are very skeptical about the use of ship's flow-through systems, even on ships that claim to have clean and well-maintained systems. Special in situ sampling/pumping devices which are deployed in situ (overboard) have been developed and used to obtain clean samples of near-surface water when the ship is underway. I think that it is also highly desirable to use these clean techniques for underway sampling to do optical analyses. Otherwise there will always be a doubt about the possible contaminating effects within the ship's flow-through system. To reduce this uncertainty, a comparative analysis of simultaneous in situ and flow-through measurements would be desirable as often as possible during the cruise. This has not been done in this study.

Specific comments:

(1) The title of the paper

I find "Direct" in the title confusing.

(2) p. 295, line 3

Replace "phytoplankton-like particles" with "low refractive index homogeneous spherical particles"

(3) p. 297, line 13 and everywhere else in the manuscript where relevant

"Palmer" should be "Parmer"

(4) p. 297, line 20

Are you suggesting that by making a measurement of the wavelength corresponding to the emission maximum of the light source, your determination of "effective" wavelength of beam attenuation measurement is actually correct? This "effective" wavelength is a function of both the light source and the spectral response function of the detector. The same question applies to your determinations of "effective" wavelengths for the ECO-BB3 instrument.

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(5) p. 298, line 10

Wouldn't it be desirable to have the internal surfaces black mat rather than black glossy to minimize reflection?

(6) p. 300, lines 10-16

For the Coulter measurements it is critical to provide sample volumes that were examined and address possible limitations in particle counts if sample volumes were too small. The statement that triplicate measurements were taken is insufficient. If each replicate measurement was taken on a very small sample volume (which is normally the case with the Coulter technique), then the particle counts are usually noisy over a large range of particle sizes, perhaps with the exception of small sizes where particle abundance is high enough. Therefore, for typical oceanic particle concentrations, it is necessary to make many more than three replicate runs and accumulate the data from these runs to achieve a high enough total volume examined and high enough particle counts over a broad size range examined. This is a highly demanding but necessary procedure to get good Coulter data on open ocean samples. It appears that this procedure has not been followed in this study as the Coulter data presented in Figure 10 are very noisy across the entire size range examined, including even the small-sized particles.

(7) p. 302, line 12-13

I think that if the calibration method for scattering instruments is based on standard beads, at least 2 or 3 different particle sizes should be used. Generally the calibration is not a trivial task. The nearly-monodisperse populations of standard beads exhibit a complex shape of volume scattering function with multiple maxima and minima at different scattering angles. The presence of these peaks and their amplitude and angular width can affect the calibration of the scattering detector that has some angular response over a finite range of scattering angles. This is why the use of one particle size does not appear to me like a robust approach for calibration.

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(8) p. 306-307, Section on IOP validation using radiometric data

I suggest removing this section. This type of closure exercise has several sources of difference, in particular not all the inputs needed to run radiative transfer simulations were measured, so some assumptions had to be invoked. I do not see how this exercise can serve as a robust quantitative tool for validating your IOP measurements. All it shows is that the modeling results and measurements agree to within 10-20

(9) p. 307-308, Section on Bulk measurements

What is the correlation coefficient for the plots shown in Figure 5? The relationships involving chlorophyll-a in Figure 6 are poor. What is the correlation coefficient? Clearly, your data support the notion that correlating the scattering properties with chlorophyll-a does not lead to good results. The statement on p. 308 (lines 5-6) about the relatively constant average values for the bbp:cp and bbp:Chl-a ratios presented in Fig. 4c can be misleading. To me, the bottom line is that these ratios are highly variable. When one looks at selected portions of these plots, they show differences in the "average" values. So the statement about the relative constancy of average values is just based on arbitrary choice of the portion of data used in averaging.

(10) p. 308, Section on Along track size-fractionated IOPs

As explained in my general comments I have little confidence in quantitative data derived from fractionation experiments.

(11) p. 309, line 11

The lower limit of your Coulter measurements is 1.4 um. Is it reasonable to suggest the presence of peak centered at 1.5 um, which is so close to the lower limit of detection? The prudent practice of experimentalists is to ignore data from several bins near the lower limit of Coulter detection.

(12) p. 310, Section on the 3rd fractionation experiment

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The data presented in Figure 10 suggest that the protocol to collect the particle size data was inadequate to obtain large enough particle counts and to minimize noise due to counting statistics. Significantly larger volumes (i.e., more replicate runs) should have been accumulated to obtain reasonably good PSDs. In addition, as the authors point out and as discussed in my general comments, the Coulter data clearly show that the approach to fractionate samples with filters is not quantitative and can be unreliable (for example, higher particle counts were here obtained for the <1 um fraction than the <3 um fraction).

(13) p. 313, lines 13-14

Support the statement about tight correlation in Figure 5 with the values of the correlation coefficient.

(14) p. 313, discussion of diel effects

Previous studies of diel optical variability in phytoplankton cultures showed that changes in refractive index (in addition to cell size) can be an important source of variations in optical cross-sections on time scales of a few hours. It seems possible that the diel variability in cellular refractive index may induce a quantitatively different effect in bbp and cp.

Interactive comment on Biogeosciences Discuss., 6, 291, 2009.