**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Relationships between the surface concentration of particulate organic carbon and optical properties in the eastern South Pacific and eastern Atlantic Oceans" *by* D. Stramski et al.

D. Stramski et al.

Received and published: 18 December 2007

RESPONSES TO REVIEWER #1

We thank Dr. Behrenfeld for his review of our manuscript and valuable comments.

(1) The comments regarding the variability in the composition of particulate assemblages.
Reply:
We agree with the reviewer that use of the bulk particulate properties alone, such as POC, SPM, TChla, and their ratios, may not be sufficient to draw unambiguous con-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

clusions concerning the variability in the composition of particulate matter. We have made corrections and changes in section 3.1 to address the reviewer's comments and to convey the idea about the variability in particulate composition in a clearer fashion. A detailed reply to these comments and the description of action taken in the revision are provided below.

With regard to the POC:Chl ratio, we agree that the wide range observed for BIOSOPE could theoretically be explained entirely by changes in phytoplankton physiology without invoking changes in the relative contributions of phytoplankton carbon and non-phytoplankton carbon to total POC. This hypothetical scenario could be simply supported by analyzing the range of POC and Chl on the BIOSOPE cruise and the range of phytoplankton carbon to Chl ratio in response to physiological variations known from laboratory studies. Although phytoplankton physiology could indeed have been an important factor driving the variability in our POC:Chl data, we believe that the variability in the phytoplankton and non-phytoplankton contributions to POC were also important. Addressing the question of relative importance of these two scenarios (including the relation to phytoplankton growth conditions) is, however, beyond the scope of our paper, and in addition, this task would actually be impossible to do rigorously given the available data. Therefore, we restricted our revisions to indicating the potential role of changes in phytoplankton physiology as suggested by reviewer and we also added several references which support the idea of variable composition of particulate assemblage along the BIOSOPE transect. For example, Grob et al. (2007) provide estimates of contributions of various picoplankton groups, larger phytoplankton, and non-phytoplankton particle classes to the beam attenuation and POC along the BIOSOPE transect. Their analysis shows significant variability in the composition of particulate assemblages, in terms of both changes within the phytoplankton community as well as changes of phytoplankton and non-phytoplankton contributions to the beam attenuation and POC.

We recognize the importance of the reviewer's comments on the POC:Chl and the fac-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

tors driving its variability in the ocean. Therefore, in the revised paper we have decided to add examples of satellite-derived data of POC and POC:Chl ratio (accompanied by histograms) obtained with the POC algorithm presented in this paper. These additions were made in section 3.2 where we discuss and recommend the use of the band-ratio algorithms. These additions provide valuable illustration of the application of the proposed POC algorithm, and emphasize the benefits of this new satellite capability for monitoring not only POC but also POC:Chla within the global ocean.

With regard to the POC:SPM ratio we agree that the uncertainties in these measurements, especially in very clear waters at many BIOSOPE stations, impose limitations on the data interpretation. This is why we do not interpret the variability in POC:SPM during BIOSOPE, but we restrict our conclusion to the statement that these data suggest the dominance of organic particles in the total particle mass concentration. On the basis of the data we feel comfortable with this statement. With regard to ANT-XXIII/1 data, we stand behind our original conclusion that the POC:SPM data suggest significant variation in the relative contributions of inorganic and organic particles to SPM, and in that sense significant variation in particulate composition.

In response to the reviewer's comment that the variability of POC:SPM is far more constrained than the POC:Chl, we note that the POC:SPM is by definition constrained to the range from 0 to about 0.4–0.6 (e.g., 0.5 if we assume that POC/POM = 0.5). The variation of POC:SPM within that seemingly constrained range thus represents the full range of variation from totally inorganic matter (POC:SPM = 0) to totally organic matter (POC:SPM = 0.4-0.6). If this fact is recognized, then it becomes clear that the range of POC:SPM data for ANT-XXIII/1 represents a large variation in the organic vs. inorganic contribution to SPM.

With regard to the question of whether or not the particle composition exhibited significant variation, the reviewer suggests "that an important factor contributing to the very nice relationships between optical properties and POC shown in latter figures is that the composition of the particle assemblages was in fact not highly variable". We

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

believe otherwise, that is that variations in particle composition were significant, but the different opinions may be to some extent an issue of misunderstanding due to semantics or the general point of view. Therefore, we will answer this question in some detail to support our contention. From the optical point of view, even seemingly small changes in particle size distributions and in the relative composition of various particle types and species are optically significant. This is because the optical cross-sections of individual particles are generally very sensitive to particle size, shape, internal structures, etc. In that sense, significant changes in particle composition are typical in the ocean, especially at spatial scales investigated on our cruises, and this is the context within which we refer to changes in particle composition. These significant changes in particle composition do not, however, mean that (all or some) bulk optical properties must always change significantly in response to variations in particle composition. For example, two very different particle assemblages may produce similar bulk optical properties. The most important point is that particle composition does change significantly in the ocean (especially at scales of interest to our study) as has been shown in many studies. One of our intentions in section 3.1 was to convey a message that our data for the algorithm development were also collected under conditions where such changes in particle composition occurred, as opposed to a situation where data might be collected for essentially the same particulate assemblage that exhibits just a variation in concentration. We agree that the data of bulk particle properties in Fig. 2 do not convey this message clearly. Therefore, our revisions of section 3.1 included editing the text and adding references from the BIOSOPE project that illustrate the variability in particle composition, including the composition of plankton community and changes in particle size distribution (Grob et al., 2007; Ras et al., 2007; Loisel et al, 2006; Stemmann et al., 2007).

Recognizing the variation in particle composition is important from the point of view of the development of our algorithms. We agree that the greater the range of variability in particle composition, the greater the chance of deterioration of the statistical relationships of interest to our study. However, our point is that the presented algorithms can

work reasonably well (i.e., within the defined statistical uncertainties) under significant range of variability in particle composition encountered across large scales within open ocean basins, such as the eastern South Pacific and much of the eastern Atlantic.

(2) The comments regarding the reasoning that supports the blue-to-green band ratio algorithms.
Reply:
We stand behind our general reasoning concerning the relationship between the changes in the blue-to-green reflectance ratio and the optical properties/effects of POC-containing particles. The essence of the operation of the blue-to-green ratio algorithm is that it relies on a monotonic decrease in this ratio with an increase in the concentration of water constituent of interest. This principle has been used for estimating chlorophyll and we also rely on this behavior in our POC algorithm. Our reasoning in section 3.2 describes the rationale for why the POC algorithm may actually be less susceptible than chlorophyll algorithms to violating the monotonic decrease of the B-G ratio with an increase in constituent concentration. Imagine two hypothetical water bodies, one that contains only phytoplankton, and the other that contains identical phytoplankton with the same chlorophyll and additionally also contains organic detritus. The POC for the second water body is thus higher than for the first one. These two water bodies will have different B-G ratios, higher in the first case and lower in the second case because of additional blue absorption by detritus. Therefore, as a result, we have the same chlorophyll concentration accompanied by different values of reflectance ratios in these two water bodies which violates the monotonic decrease of the B-G ratio with chlorophyll. However, this behavior is not violated for the B-G ratio vs. POC because we will have the higher B-G ratio at lower POC in the first water body and lower B-G ratio at higher POC in the second water body. This is the essence of our reasoning that is based upon first order effects of constituent concentrations and their absorption properties. We stand behind this reasoning in the revised paper but we have edited the relevant text in section 3.2 to make this point clearer.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

Other aspects brought up by the reviewer definitely play some role in the potential variability of the B-G algorithms (both in terms of mean trends and statistical spread of data), but we do not find it necessary to speculate further on these aspects. We feel that our paper emphasizes clearly enough the potential for the regional/seasonal variability in the empirical algorithms within the global ocean and the need for further investigation in this area.

(3) The comments about the POC:SPM in the Atlantic.
Reply:
We stand behind our interpretation of variability in POC:SPM. Above in our reply to (1), we explain the issues related to the "seemingly" constrained range of POC:SPM and the interpretation of variability in this ratio. The reviewer appears to base his judgment on the differences between the observed maximum and minimum values of POC:SPM, but the values of this difference are not directly meaningful to the quantification of variability in the organic and inorganic composition of SPM. The difference of 0.26 between 0.04 and 0.3 indicated by the reviewer actually corresponds to a very large variation in the relative proportions of organic and inorganic particles in these two assemblages. For POC:SPM = 0.04, the organic particles contribute only $\leq$10% to SPM, whereas for POC:SPM = 0.3, this contribution is $\sim$60–80% (depending on the actual ratio of POC/POM). This is a 6–8 fold difference in the composition of particulate assemblage as measured in terms of organic vs. inorganic fractions of SPM.

(4) The comment about the PSD.
Reply:
As suggested by reviewer, we dropped this statement in the revised manuscript.

(5) The comment about the backscattering ratio.
Reply:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

We do not think that revisions in response to this comment are necessary. The reviewer is correct in recognizing the limitation in the backscattering determinations in very clear waters and the interpretation of the backscattering ratio in terms of variations in detailed composition of particulate matter. We feel that these limitations are adequately presented in the paper. One clarification that we wish to emphasize here is that when we discuss the particle composition we do not consider the particle assemblage as consisting of just two broadly defined particle categories, phytoplankton and non-phytoplankton components. If one thinks solely in terms of such oversimplified partitioning of particle assemblage, then this may be a reason for some misunderstanding of our concepts and interpretation whenever we refer to the variability in particle composition. Our way of thinking about the variability in particle composition and their effects on optics refers always to detailed particle composition that includes a large number of various particle types/species that have different optical properties at the level of individual particles.

Minor comments:

(1) The comment about the instrument self-shading.
Reply:
The reviewer is correct: the measurement of radiances and irradiances themselves introduce modifications of the radiance field, potentially leading to error. Most notable is the "shading" effect of the instrument which decreases the radiance field in the shadow zone below an instrument, particularly when it aims at nadir. The magnitude of this effect to first order depends on the ratio of the diameter of the instrument (r) to the absorption coefficient of the wavelength of interest (a); the effect is most pronounced for red wavelengths in most ocean and coastal waters at low solar zenith angles (Gordon and Ding, 1992; Leathers et al., 2004). Not only the diameter, but also the height of the instrument and any supporting structure also affects the in-water radiance distribution (e.g., Piskozub et al. 2000).

To first order, the approach of Gordon and Ding (1992) appears to be satisfactory at least for clear to moderately turbid waters (Mueller et al. 2003). Here, the correction term, ϵ, for a radiance sensor of radius r is given as $\sim 1 - e^{(-\kappa' ar)}$, which simplifies to $\kappa' ar$ for small values of the non-dimensional product of the instrument radius and the attenuation coefficient. The term κ' varies inversely as a function of solar zenith angle (actually as a function of the refracted angle in water). To fix an order of magnitude of the error, consider the HyperPro radiance sensor with a diameter 6 cm and the SPMR that is 10 cm. For the clearest of ocean waters, and with the sun near zenith (20 degrees), the error in measuring upwelling nadir radiance with the smaller instrument is less than 1% over the waveband 350–580 nm, but climbs to $\sim$7% at 700 nm. Errors in measurement of upwelling irradiance are somewhat less. An instrument of 10 cm diameter would have errors less than 2% for wavelengths shorter than 555 nm. Therefore, in our study of clear open ocean waters with Chla typically less than 0.5 mg m^{-3}, no correction for shading effect was deemed necessary for the blue-green bands involved in the algorithm development. To indicate this, we added a sentence in the revised manuscript in section 2.3.1. We note that in turbid waters, for example for bloom situations (Chla $\sim$30 mg m^{-3}), the error would become higher for both instruments, > 3% over all wavelengths (but less than 10%).

Gordon, H.R. and Ding, K. (1992). Self-shading of in-water optical instruments. *Limnol. Oceanogr.* 37: 491–500.

Leathers, R.A., Downes, T.V., and Mobley, C. (2004). Self-shading correction for oceanographic upwelling radiometers. *Opt. Exp.* 12: 4709–4718.

Mueller, J.L., Fargion, G.S., and McClain, C.R. (eds) (2003). *Ocean Optics Protocols For Satellite Ocean Color Sensor Validation*, Revision 4, Volume IV. NASA, Goddard Space Flight Center, Greenbelt, MD.

Piskozub, J., Weeks, A.R., Schwarz, J.N., and Robinson, I.S. (2000). Self-shading of upwelling irradiance for an instrument with sensors on a sidearm. *Appl. Opt.*, 39:

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

18728212;1878.

(2) The comment about the two-step empirical algorithm.
Reply:

Because R_{rs} is to first order proportional to b_b and inversely proportional to a, we think that our statement about the relative roles of b_b and a at 555 nm is clear. We have, however, edited the text to clarify that it is the interplay of absorption and backscattering that results in relatively smaller change in R_{rs} in the green spectral region compared to changes at blue wavelengths.

Interactive comment on Biogeosciences Discuss., 4, 3453, 2007.

EGU