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Biogeosciences Discussions

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.

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This manuscript investigates relationships between optical properties in the South Pacific and eastern-most Atlantic Ocean and variability in particulate organic carbon. A primary thrust of the paper is to evaluate approaches for assessing POC from remote sensing variables, including wavelength ratio relationships (including fixed ratios and maximum band ratios), single wavelength relationships, and what is called the "hybrid" approach - i.e., using bbp data from semianalytical models (QAA and GSM).

As I had anticipated given the lead author and list of coauthors, this is a very thorough and well written manuscript that requires few modifications, in my opinion, before publication. My comments/recommendations are as follows:

(1) Of the multiple conclusions made in this manuscript, the only one I really had trouble with was the assertion that the composition of the particle assemblages varied significantly within a given cruise and between cruises. This idea is first presented in section 3.1 where the authors make the follwing statements (in reference to figure 2):

"High values of POC:Tchla within the SPSG suggest an increased contribution of non-phytoplankton carbon to POC....The variability in POC:SPM indicates that there was variation in the composition of the particulate matter in terms of relative contributions of organic and inorganic particles....The fact that both POC:TChla and POC:SPM show a broad range of variability in our data set....implies particle assemblages with variable composition....The POC:TChla data suggest that the total POC pool consisted of either dominant contributions of non-phytoplankton carbon, phytoplankton carbon, or a range of intermediate cases....The highly variable POC:TChla ratio also demonstrates the difficulty in directly estimating POC from Tchla...The POC:SPM data indicate that particule orgnic matter was not always a dominant component of the total particulate matter in terms of mass"

This idea of a variable POC constitution is picked up again later in Section 3.4. While it is highly unlikely that the relative contribution of different particle sizes and types is constant over the oceans, I believe it is important to evaluate the above conclusions more carefully. Let us start with the BIOSOPE data.

My own evaluation of the five left hand panels of figure 2 gives me the following impressions: (1) The BIOSOPE cruise transected a wide dynamic range in physical-ecological regions, causing first-order patterns in POC, SPM, and Tchla to covary significantly, (2) Variability in the POC:SPM ratio (+/- 0.2 g/g or approximately +/- 40%) is FAR more constrained than the POC:TChla ratio (order magnitude variability), and (3) Variability

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in the POC:SPM ratio is spatially random, while variability in the POC:TChla is highly ordered. My conclusions from this evaluation are that (1) Variability in the composition of the particle assemblage based on the POC:SPM ratio is very small relative to changes in the total particle biomass (which was greater than an order of magnitude), (2) The randomly distributed variability in POC:SPM ratio data may simply reflect uncertainties in POC and SPM measurements - i.e., its not clear if this variability is significant or methodological, and (3) The smooth longitudinal variability in POC:Tchla is due to commonality in environmental forcing factors driving changes in biomass and changes in cellular pigmentation - in other words, along the longitudinal transect the availability on nutrients, type of nutrient stress, temperature, and median mixed layer light levels are changing in a relatively smooth manner that results in the smooth trends in both biomass and phytoplankton Chl:C ratios, such that the results in the fifth panel of figure 2 largely reflect changes in phytoplankton physiology and not the relative contribution of phytoplankton to POC. In summary, I do not look at the BIOSOPE data in figure 2 and see significant variability in the composition of the particle assemblage.

If we now turn to the ANT-XXIII data we see very little variability across the latitudinal transect in POC, SPM, or Tchla, thereby yielding little evidence of variability in the particle composition in the POC:SPM and POC:TChla ratios. Taken together with the above considerations regarding BIOSOPE, the only evidence that remains suggesting significant variability in particulate composition is the inter-cruise difference in overall POC:SPM ratios and, as discussed in the text of the manuscript, it is not clear that this difference isn't simply due to methodological differences in the SPM measurements between the two cruises. To go one step further, I would also suggest 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.

(2) In section 3.2, we begin with a discussion about how changes in the ratio of blue-green wavebands can be used to estimate chlorophyll concentration because of the

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spectrally selective absorption properties of phytoplankton pigment. This is then followed in the next paragraph by the statement:

"A similar reasoning can be applied to the estimation of POC from the B-G reflectance ratio by assuming that this ratio is driven largely by absorption associated with all POC-containing particles. The logic behind this reasoning is that spectral absorption by various types of organic particles such as detritus, heterotrophic organisms, and phytoplankton exhibits an increase from the green toward the blue spectral region."

I have to question this logic though.... While it is true that the different components of the particle assemblage all have increasing absorption with decreasing wavelength in the visible, the spectral slope of absorption for each particle category is different. Thus, if in the previous paragraph the authors argue that there is significant variability in the composition of the particle assemblage than it follows that the logic they apply in section 3.2 is faulty - ie., variability in the particle assemblage will degrade any correspondence between POC and a given B-G ratio. While I recognize that the authors state that this spectral dependence is "qualitative", I think this is not sufficient. Might I instead suggest (again) that the reason the B-G ratio works for these data sets is that the relative composition of the particle assemblage is highly conserved, such that pigment absorption is correlated with POC (as clearly shown in figure 2) for these data. Going a step further, one might also conclude that the reason a power relationship is observed is because the phytoplankton chlorophyll:C are inversely related with biomass during BIOSOPE - for very clear physiological reasons - such that pigment and POC are not simply linearly proportional. These ideas then raise the question of whether the conclusion of this manuscript regarding the estimation of POC from B-G ratio algorithms is robust at the global scale where the chlorophyll- POC relationship observed during BIOSOPE (i.e., the data set that dominates the dynamic variability observed during the two studies) may not hold in all locations (?).

(3) In section 3.4, it is stated that:

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”some measurements in the Atlantic were likely made on water samples with significant contribution of terrigenous inorganic particles, which is indicated by larger range in POC:SPM including low values of this ratio on the ANT-XXIII cruise”

Is this statement true? Can the overall difference between POC:SPM ratios for the two cruise be unambiguously assigned to true changes in the particle assemblages, rather than methodologies? I’m not yet convinced of this. If we actually look at the intra-cruise variability, the BIOSOPE data (estimating from the graph) gave POC:SPM ratios between approximately 0.23 and 0.68 (difference = 0.45). The ANT-XXIII cruise gave values of approximately 0.04 and 0.48 (difference = 0.44). The BIOSOPE data is rather evenly distributed within its range, but the ANT-XXIII data has two particularly high values that if removed reduce the range to approximately 0.04 to 0.3 (difference = 0.26). So, is there really a larger range for the ANTXXIII cruise? I don’t see it....

(4) Toward the end of section 3.4 it is stated that:

”the variation in PSD has been identified as one possible main causes for the differences between BIOSOPE and ANT-XXIII in Fig. 10, but such supposition cannot be ascertained without more detailed and complete data on particle sizes and composition”.

I think this statement should be dropped. There was no PSD data for the ANT-XXIII cruise. For all we know, it could have been exactly like that observed for BIOSOPE. The earlier discussion of this idea is sufficient and I think this later statement could be misleading and is better omitted.

(5) The backscattering ratio data is interesting. What I see in this figure (fig. 10) is a separation of the two cruise data sets. I don’t think this is necessarily evidence that the relative contribution of different particle groups varied significantly between cruises any more than variability in optical properties of particles within any given group. The other question that arises is whether the wider range of variability observed during BIOSOPE is real. Nearly all the variability in that data set is found at the lowest particle

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concentrations. Could it be that we're simply looking at our technical limit in measuring the backscattering ratio under these conditions?

Minor Comments

(1) Regarding the processing of Lu data - there is no mention (at least I didn't see it) of any correction for instrument self-shading (Gordon and Ding, 1992, L&O, 37(3):491-500). Was this unnecessary or encompassed by the statement "Calibration coefficients and corrections for immersion effects were obtained following standard protocols (Mueller et al., 2003)" ?? The importance of this correction increases with absorption (and thus is wavelength dependent) so it may not be critical in open ocean waters. Also, by using reflectance ratios the problem may be minimized. Just a thought....

(2) In section 3.2.2 it is stated that:

"The relationships of the two-step empirical algorithm utilizing a single wavelength of 555 nm are shown in Fig. 7. In this green spectral region absorption by most particle types is weak. Therefore, one may expect that the variation in Rrs(555) will be often driven largely by changes in the backscattering coefficient bb(555), and to a lesser degree by absorption, especially in clear oceanic waters. This explains the relatively small increase in Rrs(555) with an increase in POC in Fig. 3."

However, figure 7b shows that bbp(555) almost doubles when POC doubles. Do you mean in the above statement that "absolute changes" or "relative changes"?

Review provided by: Mike Behrenfeld

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