**Biogeosciences
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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.

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RESPONSES TO REVIEWER #2

We thank Dr. Gardner for his review of our manuscript and valuable comments. In his General Overview, he emphasizes that the carbon to chlorophyll ratio varies significantly and thus makes an estimation of POC from chlorophyll inaccurate. In line with this comment and also in response to comments on the POC:Chl ratio by Reviewer #1, we have decided to add examples of satellite-derived data of POC and POC:Chl ratio 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 band-ratio algorithms. We

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think that these additions provide valuable illustration of the application of the proposed POC algorithm to satellite observations, including the capability to monitor the variability in POC:Chl.

(1) The question regarding our limited discussion on the integrated depth from which the satellite signal originates.
Reply:
This study focuses on the development of "in-water" algorithms based on surface field data. The sampling depths of our measurements within the surface ocean are described in the paper. The vertical attenuation coefficients were used solely as part of the procedure for calculating R_{rs} from underwater profiles of E_d and L_u. This procedure is also described in sufficient detail and supported with references. The depth of the water column from which the signal viewed by a satellite originates has little direct relevance to the development of our algorithms from surface field data. We recognize that the remotely-sensed layer is relevant to our study to the extent that our algorithm is intended to be applied to satellite-derived reflectances. However, we do not feel that the remotely-sensed layer, which is variable in marine environments depending on the optical properties and light wavelength, deserves special attention in our paper.

(2) The question about POC methodology.
Reply:
As written in the paper, our POC method was "generally consistent with JGOFS protocols", which means that the basic aspects of the method were the same (such as the collection of particles through filtration on precombusted GF/F) but not necessarily each and every detail. Therefore, we described our POC protocol in detail. We think that the specific questions raised by the reviewer regarding the issues of DOC adsorption, blank determination, and potential errors have been adequately addressed in our manuscript. We state our view that it is not necessarily advantageous to eliminate one possible source of positive bias (DOC adsorption) while being unable to remove other

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sources of systematic errors, especially those leading to negative bias such as incomplete retention of particles on the filters. The implication is that it is possible that the method that corrects for DOC adsorption may yield larger error in the final estimate of POC than the method that does not attempt to correct for DOC adsorption. Presently, it is impossible to quantify the interplay of all the sources of positive and negative biases. Whereas Gardner et al. (2006) show that the positive bias associated with DOC adsorption can be reduced, they do not prove that in the end the procedure involving DOC correction actually yields more accurate estimates of POC. We recognize that this would be difficult to prove because the true POC would have to be known. In our opinion, further investigation of POC methodology (including all potential sources of bias) is required and we are not in favor of possibly premature and uncritical adoption of a DOC correction scheme in the standard POC method at the present time. For further clarification of our reasoning we reworded parts of the text in section 2.2. In particular, we made it clearer that making measurements on replicate samples and large number of blank filters is important to minimize statistical uncertainties.

With regard to the quantification of final error in POC, we think that our text has been adequate in explaining the difficulties in obtaining accurate estimates of POC in very clear waters as well as accurate estimation of errors. However, we improved the relevant sentence in section 2.2 which now reads "..., one can expect that the samples differing from one another by less than about 5 mg m^{-3} cannot be resolved with current measurement uncertainties, which has particular ramifications for hyperoligotrophic waters where POC is less than 20 mg m^{-3}.".

(3) The questions about SPM measurements and sampling of the "dregs".
Reply:
We share the reviewer's opinion that SPM measurements on GF/F filters have limitations and our awareness of these limitations, as well as our steps to minimize artifacts, are reflected in the discussion of the SPM methods and results. We thank him for indicating an error in the stated resolution of 0.1 mg; this has been corrected to 0.01 mg.

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With regard to the use of GF/F filters, it is still the most practical and feasible way when one attempts to estimate SPM in typical oceanic conditions. An additional consideration was a desire to use the same type of filters employed in POC determinations to minimize biases in estimation of the POC:SPM ratio. As discussed in the paper, we also attempted to use polycarbonate membrane filters (0.2 μm Poretics), but these attempts turned out to be extremely difficult in clear oceanic waters where large volumes of water must be filtered to collect enough particulate matter for SPM determinations.

As stated in the manuscript, the samples for SPM determinations on Poretics filters included the "dregs" and were collected "by opening the bottom stopcock" of the Niskin bottle (so the bottles were not tipped over and water was not collected through the spigot). We felt that extra cleaning of the outer portion of the Niskin bottles was unnecessary as the bottles were thoroughly "rinsed" in the ocean during the cast and the sampling was done shortly upon completion of the cast. We also note that surface samples were collected at the end of the cast and immediately before bringing the rosette upon deck (minimal time delay between collection and sampling), and we would anticipate the presence of large, fast-sinking particles in the surface waters of highly oligotrophic stations to be minimal. The SPM results obtained at several stations on Poretics filters (that include the "dregs") are shown along with the routine SPM determinations on GF/F filters in Fig. 2. As pointed out by the reviewer, these SPM data are used only to show the general range of variability in particle load during our cruises. In section 2.4, we added a new sentence which reads "For several stations where SPM samples were collected on both GF/F filters and Poretics filters that included the "dregs", no systematic differences between the two determinations were observed."

Reply to detailed comments:

3456/13 The reference Gardner et al. (1985) was added in the context of relationships

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between the beam attenuation and SPM.

3458/2 The study by Chung et al. (1986) does not include POC:Chl data from direct measurements of POC, so we decided not to add this reference in the context of our POC:Chl data.

3459/10 Our intent was to write a concise sentence to indicate that alternative approaches that are not based on simultaneous acquisition of all relevant variables during the cruise or field experiment has significant limitations for establishing correlational algorithms. The reviewer indicates that the Gardner et al. (2006) algorithm utilized simultaneous (synchronous) field POC and satellite ocean color data. However, in that study there is a mismatch between the temporal and spatial scales of averaging of the satellite ocean color signal and a local water sampling at sea for POC measurement. Gardner et al. used the 8-day composite satellite data product of water-leaving radiance representing the 9×9 km pixels. This satellite-derived variable was correlated with POC derived locally from water sampling at a single point at sea. In addition, we note that the satellite-data product used in the algorithm development is affected by atmospheric correction which is an active area of research where procedures constantly evolve with a purpose of reducing errors. We believe that the approach presented in Gardner et al. (2006), as indicated in our manuscript, falls into the category of approaches with limitations similar to those in a few other references cited in that context (Mishonov et al.; Loisel et al.). In our opinion it is critical to pay attention to temporal and spatial mismatches when establishing correlational algorithms. Decades of research in remote sensing of chlorophyll have established that the primary role for matching satellite-derived data with in situ data is in the area of the algorithm validation, and not in the area of the algorithm development. We note that in practicality even on cruises the technical/logistical issues make it difficult to collect truly simultaneous data for all relevant variables involved in the algorithm development. This is why we have devoted a special section 2.5 to the issue of temporal correspondence and some relatively small

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temporal mismatches in water sampling/measurements on our cruises.

We made brief editorial changes in the context of citing alternative approaches (i.e. Gardner et al. 2006 and a few other references) to clarify that the shortcomings are related to temporal and spatial mismatch between the correlated variables.

3457/7 We think that the text gives sufficiently detailed information about the SPM vs. POC regressions, and adding a new graph is not necessary. These data are not critical to our study. We also note that the ratio POC:SPM is shown in Fig. 2.

3475/29 The missing Honjo et al. reference was added.

3476/9 Correction done.

3477/9 The ratio of the maximum to minimum reflectance at 555 nm for the examples shown in Fig. 3a is about 1.5, so no correction is necessary.

3495/20 This statement was dropped in response to Reviewer #1.

Fig. 6. Figures 5, 6, and 7 (6, 7, and 8 in the revised paper) show different pairs of variables so that the number of data points is not necessarily the same on these figures. The number of data points presented for any given pair of variables is determined by simultaneous (or nearly simultaneous) measurements of these variables, which passed the quality control procedures. To answer the reviewer's question, however, each of these three figures in fact do contain the same number of upwelling points (N = 5); the point at the end of the dotted line in Fig. 6 (Fig. 7 in the revised paper) is a non-upwelling station.

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