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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.

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General overview

Scientists have spent over 30 years developing and refining algorithms for estimating chlorophyll concentrations from ocean color measurements from satellites. In this paper several groups have combined data sets and expertise to develop similarly derived algorithms for determining concentrations of particulate organic carbon (POC). This is important because in trying to follow the path of carbon in the ocean, it is important to

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measure POC directly rather than estimate it from chlorophyll. As pointed out in the paper, the carbon to chlorophyll ratio varies significantly and makes such an estimate highly inaccurate. The paper is extremely detailed (perhaps overly so at times) and thorough in describing methodology. At times there is more review of past work than normally occurs in papers in print, but the reviews are generally well done and bring the relevant material together in one place so the reader doesn't have to go search other papers if s/he is not familiar with the issues. The paper is well organized and very well written. The rationale is clear for the development of the algorithms and they provide appropriate caveats about various aspects of the methodology, leaving room for further refinement and research. The conclusions section makes a good summary of the work presented in this paper. It is a very nice piece of work.

Questions/comments

One discussion that is lacking in the paper is the integrated depth from which the satellite signal originates. The authors have discussed optical depths in other papers, but virtually nothing is said about it here. Page 3460 says that samples were collected from about 5 m depths, and on page 3465 there is discussion about the depth at which vertical attenuation coefficients were determined, but I don't remember any other general discussion.

I'm very saddened to read on page 3461/1-14 that the protocols for POC analysis followed those of Knap et al., 1996, because that was an unfortunate aspect of the JGOFS program. The authors of this paper note that they just used dry filter blanks without passing any water through the filters. They later review the issues of the Gardner et al, paper of 2003 in which it was expressly stated that it was important to pass filtered water through the blank filters to correct for adsorption of DOC. On line 3462 they say that the blank determinations were based on a relatively large number of blank filters. It doesn't matter how many blanks you have if the methodology is flawed. From your range of blanks did you make any estimates of how great the error may be for your POC values? The biggest problem is that POC concentrations < 2 micro mo-

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lar (24 mg/m³) are likely to be elevated. This may affect a large percentage of the BIOSOPE POC values between 130 and 90°W, but would have little effect for the ANT-XXIII/1 values. On the positive side, the POC correlations with optical data were very good (Fig. 4), suggesting the POC errors should be low. Of course most of those plots are on log scales, but the fit is still impressive even at the low concentration end of the scale which is the only range where blank errors would have any impact.

I was also taken back that GF/F filters were used to measure SPM concentrations in the open ocean. These filters have always been notoriously unreliable for low concentrations because of the humidity effect for adsorbed water. From your data it appears that pre-washing the filters had a very positive impact on the weight stability. Perhaps this is a larger reason for weight errors in these filters than previously realized. It also helps that near-surface waters (as opposed to > 200 m depths waters) were being filtered as they contain much higher particulate mass, and they were able to filter 6-11 liters in one project and 1.5 - 8.4 liters in the other. Surely, however, on line 3472/14 there must be an error in the stated resolution of 0.1 mg. How accurate are your weight measurements with no better resolution than that?

On line 3472/23 you refer to the "dregs". This issue was first explained in Gardner (1977). It is not clear in this section if all the water was removed from the bottle. Did you tip the bottles over to get all of the water out through the spigot or did you open the bottom of the Niskin bottle? If the latter, what precautions were taken to clean the outer portion of the bottle first? What concentrations did you find in this water and how were these results used in your analysis?

I recognize that the above two points about SPM weights do not change the overall results of your work with POC algorithms. They are only used to argue that you were sampling waters with very different compositions in developing your algorithms.

Detailed comments

3456/13 Other early papers using beam attenuation coefficient for measuring SPM are

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Gardner et al., 1985 and Gardner 1989- see refs at end.

3458/2 Chung et al. 1996 mapped sections of POC:chl across the equatorial Pacific during the JGOFS EqPac program. This would be of use in your discussion on page 3474/23 or 3476/25 also.

3459/10 While it is true that the Mishonov et al, POC, in-situ beam Cp and ocean color data were not collected simultaneously, POC and ocean color data were collected simultaneously for the algorithms developed in Gardner et al. (2006). Only the synchronous data from the JGOFS Southern Ocean and 9 cruises in the Gulf of Mexico were used in the final algorithms.

3475/7 It would be useful to see a plot of these data.

3475/29 The Honjo reference is not listed at the end.

3476/9 . . . suggest A relatively small role

3477/9 It looks like less than a 1.5-fold increase at 555 nm, but you have the numbers and don't have to judge by the chart - please check this in the Fig 3 data.

3495/20 . . . one of THE possible . . .

Fig. 6 There appears to be one more point in figure 6 than in figure 5 or 7 as part of the upwelling points. Is the point at the end of the dotted line in figure 6 (BIOSCOPE) not an upwelling point?

Honjo -cited on page 3475/29, but not listed in references.

Gardner, W. D., 1977. Incomplete extraction of rapidly settling particles from water samplers. Limnol. and Oceanogr. 22:764-768

Gardner, W. D., P. E. Biscaye, J. R. V. Zaneveld and M. J. Richardson, 1985. Calibration and comparison of the LDGO nephelometer and the OSU transmissometer on the Nova Scotian Rise. Mar. Geol. 66:323-344.

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Gardner, W. D., 1989. Periodic resuspension in Baltimore Canyon by focusing of internal waves. Jour. Geophys. Res. 94:18185-18194.

Chung, S.P. W.D. Gardner, M.J. Richardson, I.D. Walsh, and M.R. Landry, 1996. Beam attenuation and microorganisms: Spatial and temporal variations in small particles along 140° W during 1992 JGOFS-EqPac transects. Deep-Sea Res. II 43: 1205-1226.

Review by Wilford Gardner

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