

Interactive comment on “On the impacts of phytoplankton-derived organic matter on the properties of the primary marine aerosol – Part 1: Source fluxes” by E. Fuentes et al.

E. Fuentes et al.

elena.fuentes-lopez@manchester.ac.uk

Received and published: 26 August 2010

We thank Prof. Huebert for the very helpful comments. We have addressed the general comments first and the specific comments on a point-by-point basis as indicated below:

General comments:

The reviewer questions the representativity of ocean color data for accurately describing the distribution of phytoplankton biomass in the oceans, and consequently, its applicability to particle fluxes prediction. As the reviewer points out, the satellite data constitute an educated guess of the reality. The intercomparison between different ocean color sensors during algal blooms and its comparison with ground-truth observations

C6845

of Chl-a concentrations and algae species composition are currently being conducted for evaluating the validity of the ocean color products. These comparisons indicate that the ocean color data can be used as a first-order estimate of the real Chl-a and phytoplankton distribution. Whilst these data might not be able to provide accurate information, in our approach we offer a parameterisation in line with the currently available methods for estimating the biological activity in the oceans. In order to reduce the sense of certainty in this part of the paper, the text will be modified to state that the ocean color data can be used as an estimate of the Chl-a distribution rather than for accurately describing the real biota concentration distribution.

Concerning the validity of the inferred Chla-OC relationship, it was literally mentioned in the manuscript that the results presented were “an example of application”. The sensitivity analysis was included not to credit the derived equation, as the reviewer believes, but on the contrary, to highlight the fact that the results are sensitive to the parameters defined in the equation. We agree however, that the reader might interpret that this equation can be used for general application. In order to avoid this and reduce any sense of certainty on this aspect, the example equation will be removed from the text and the relationship between Chla-OC will be presented in Figure 16 as a region which includes the uncertainty limits due the parameters in the mass balance equation (see attached Figure). In addition, the inclusion of the uncertainty limits will allow a better estimation of the minimum Chl-a concentration necessary to observe effects on the particle fluxes and will illustrate the fact that the Chla-OC relationship depends on the specific primary production conditions in algal blooms.

Specific comments:

-Page 14097, line 5: particle number flux? Please specify.

It will be stated in the text that this is the particle number flux.

- Page 14103, line 22: Typo? Don't you mean stained? The typo will be corrected.

- Page 14105, line 17: What is an f/2 nutrient solution? This needs to be readable for aerosol folks.

The f/2 nutrient solution is the sample containing nutrients necessary to grow the algal cultures. A more detailed description of the composition of this sample will be specified in the text.

- Page 14121, lines 14-17: This is confusing. The ocean is full of electrolytes. Are they negating their entire MS by saying that Asher 97 was right?

The reviewer seems to be confused here with the measurements of bubble spectrum and particle size distribution. In the manuscript it is said that Asher 97 is right, i.e. that the presence of surfactants in seawater does not affect the bubble spectrum (this does not refer to the particle size distribution or the bubble bursting mechanism). This allows the use of the bubble coverage area obtained from experiments with artificial seawater devoid of exudate for deriving the particle flux parameterisation as a function of the whitecap coverage in the case with organics. Also, the fact that there is not an observable effect of the surfactants on the bubble spectrum does not imply that there is not an effect on the bubble bursting process itself.

- Page 14122, lines 17-19. Aren't these two sentences in conflict? If not, please explain.

It will be clarified in which cases the standard deviation was or not modified. These lines will be replaced by:

“Only the value of the standard deviation of the second lognormal mode changed to 1.9 for the case with organics. For the rest of the modes no significant changes were observed in the geometric standard deviation with the modification of the organic concentration.”

-Figures: Virtually all of the axis labels and legends are unreadably small. It is not helpful to put a lot of information into the figure itself (as opposed to the caption), if

C6847

readers cannot decipher any of it.

The Figures will be edited to increase font sizes.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14095, 2010.

C6848

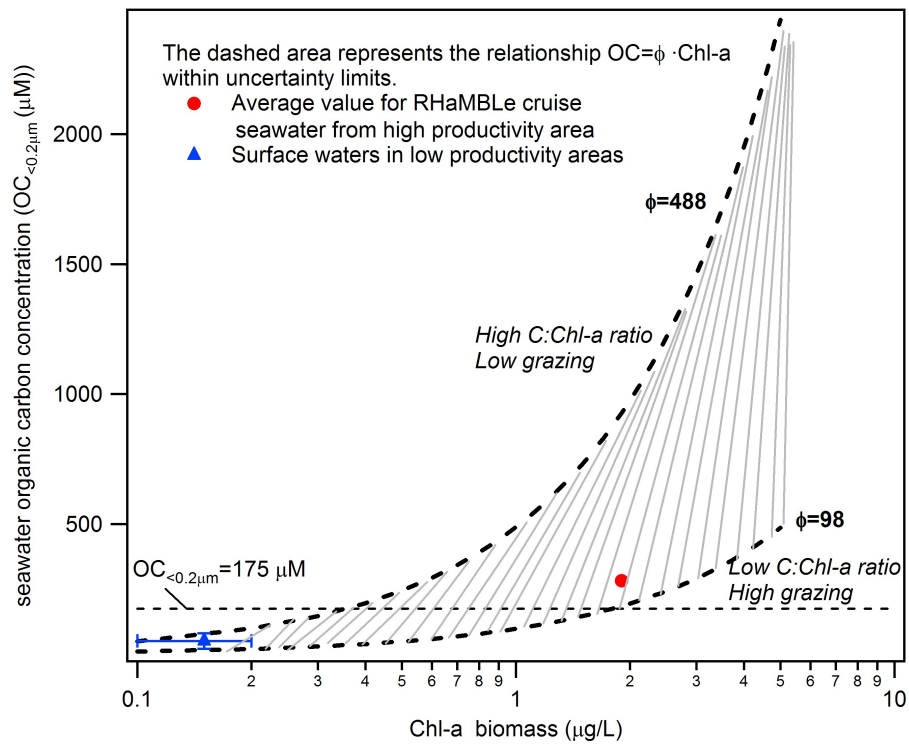


Fig. 1. Relationship between Chl-a and seawater $OC_{<0.2 \mu\text{m}}$ concentration, as obtained from the variation of the parameters defined in equation 7.