Atmos. Chem. Phys. Discuss., 10, C11643–C11646, 2010
www.atmos-chem-phys-discuss.net/10/C11643/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A sea-state based source function for size- and composition-resolved marine aerosol production" *by* M. S. Long et al.

Anonymous Referee #2

Received and published: 26 December 2010

General.

The authors present a source function for size- and composition-resolved marine aerosol production which is based 1) on the air entrained in the ocean by breaking waves which upon detrainment as bubbles produces sea spray droplets with b) an organic matter fraction which is based on the chlorophyll concentration at the ocean surface as determined from satellite observations. This is a new source function with original ideas on the size-resolved number production flux which is a valuable contribution to the discussion on the quantitative formulation of the production of sea spray particles and their chemical composition. To get a better handle on the sea spray source function is of utmost importance for climate models estimating radiative effects of aerosols on climate in which seas pray aerosol is one of the largest contributions.

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Interactive Discussion
Discussion Paper

I strongly recommend publication of this article in ACP. Below follow some comments for the consideration of the Long et al.

General comments.

The authors use a relationship between the air entrained in the water column and the energy dissipated by the wind-wave field through wave breaking. They provide values for the ratio between the air entrained in the water column and the energy dissipated by the wind-wave field from different studies on different types of breaking waves and based on these they estimate this ratio as [(4 +/- 1) x 10-4 m3 J-1]. However, the uncertainty does not encompass the observations. Hence I would think that the uncertainty in the derived source function would be substantially larger.

In the paper the authors state (p. 22285) that they use size resolved particle number flux measurements from Keene et al. (2007) who measured in a laboratory set-up using artificially generated bubbles with the detrainment rate determined by the aeration flow rate. Long et al. consider two size ranges (0.044-1.0 ïA■m and 1-24 ïA■m diameter at 80% RH). These size ranges are very wide and across each of them the production and removal rates vary widely. Why was this particular separation in two modes chosen?

The authors use mode-2 number fluxes measured with an APS: in view of the importance of these fluxes for the current paper it would be interesting to know how these fluxes were determined from the APS data. Keene et al. do not mention how this was done. Also it would be worth mentioning how mode-1 fluxes were obtained. Furthermore it is important to mention that the measurements were made from artificially produced bubbles in a laboratory situation with fresh sea water. Several methods to artificially generate bubbles were recently discussed by Fuentes et al. (Atmos. Meas. Tech., 3, 141–162): how representative are the bubbles generated by Keene et al. for those generated by breaking waves and do bubble spectra change with wind speed? Last but not least, the new parameterization is based on a single data set obtained from sea water pumped up near Bermuda. In view of previous and somewhat contrasting

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findings by O'Dowd and co-workers, by Modini et al. and by Leck and Bigg, it would be worthwhile discussing how representative the findings on organic matter are.

Detailed comments

22282, 1: "These" particles: specify which particles 22282, 20-25: high temporal resolution insufficient: eddy covariance measurements usually provide fluxes averaged over 20-30 minutes. What temporal resolution are the authors aiming for and why? Does the technique they use provide better temporal resolution? Satellite data are usually only available for the time of overpass which usually is once per day. Or is it the size range that is insufficiently covered by eddy covariance? 22286, 13: I agree that these fluxes are upper limits, but for bubble-mediated production; spume drops were not part of the study and should affect the larger end of the mode-2 fluxes. 22286, 14: the raw data by Keene et al.? 22286, 17: Dp=D80 , why not simply use D80? See also 22290, 4. 22286, eqs 6 and 7 are discontinuous at Dp=1; it would be illustrative to show how well these polynomials fit to the raw data and have an idea on the uncertainties introduced by the polynomial fitting. On p. 22288 the authors refer to Appendix A for a comparison of the 3rd and 4th order polynomial fits to the data, but that comparison is missing. 22287, 9-17: discussion of uncertainty does not include the uncertainty in the entrainment discussed in the general comments. 22288, 6-7: "our parameterization . . . " : I suggest that the authors provide here also a number for the conversion factors they use. 22288, 23: scavenging: Fuentes et al. (Atmos. Meas. Tech., 3, 141–162) conclude that much shorter rise distances are involved, please comment. 22288, 26-27: does this imply that OMsea scavenging is transport-limited? 22289, 3-6: does that imply that the production flux is not affected by OM? I.e. bubble behaviour is not affected? This seems to be in contradiction with work by Blanchard and co-workers. 22289, 19: if OM does not influence hygroscopicity, why is eq 10 formulated? 22290, 20-23: for readers less familiar with the Langmuir formulation, please explain what the saturation constant and K1 are. And give reference. 22291, 1: explain what the Langmuir approach is, refer-

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ence? 22291, 21: “because it does nor correlate . . .” what is “it”? 22292, 3: what is “this preliminary proxy”: Chl-a or DOC? 22292, 8-10: help the reader and provide the constants in addition to referring to their source: what OM/Chl-a relation was used? Which Dp? 22292, 13: should this be ïĄğ1? And on line 15 ïĄğm,1? 22293, 22 – 22294 begin_of_the_skype_highlightingÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂăÂă22 – 22294ÂăÂăÂăÂăÂăÂăend_of_the_skype_highlighting, 2: this repeats what was mentioned above. 22294, 8-9: Long 10 has a different wind speed dependence than the other three which use Monahan & O’Muirchaertaigh. Appendix A: Note here that the source function is the sum as given by eq. 2. See comment 22286, eqs 6 and 7. Figure 3: is there a reason why the graph should go through (0,0), i.e. is there no OM if there is no Chl-a? How can the solid line depict the fit of the mean OMaer data in Table 1 while there are only 2 data points in table 1? Figure 4: the scale in the lower 2 plots does not show any detail in most of the world. Would a log color scale be more appropriate?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22279, 2010.