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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 #1

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The paper presents a sea-state based source function parameterisation for primary marine aerosol production. It is a really valuable contribution to the community effort put in deriving a reliable marine aerosol source function taking into account biogenic organic matter which received an already significant attention since O'Dowd et al. (2004) paper. Contrary to many efforts, authors went on to parameterise sea spray production starting with an air entrainment which is driving aerosol production through bubble bursting and though it is similarly dependent on U10 as most parameterisations it is an elegant approach. I really like that. The paper is certainly recommended for publication subject to minor corrections to improve the already excellent paper.

Minor comments:

Authors should consider an important recent modelling paper by Vignati et al. (2010)

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which was published early this year and, therefore, may have been missed. I would recommend considering its main results in the context, because it is based on Gong (2003) and O'Dowd et al. (2008) source functions, both of which are extensively discussed and compared in this paper. The very latest paper of Fuentes et al. (2010) which came out after the authors have already submitted their paper would also be extremely useful to take into consideration. However, in fairness, I leave that at author's discretion.

Surface tension is an important parameter when considering production of film drops and it is well known that marine organic matter exhibits significant surface tension lowering. Even if not possible to account for this effect it would very useful to at least discuss it where appropriate.

Page 22284. It would be useful to report variability of the U10 power coefficient as part of the uncertainty analysis. In fact, variability of the power coefficient would be the main source of the uncertainty and not of the linear parameters, I suppose.

Page 2285 The divergence of APS and impactor based data is more likely due to different inlet efficiencies and not gravitational settling and impaction once particles get into APS. Impactors, which run at higher flow rate and have larger inlets, are prone to smaller losses than online instrument inlets which are poorly characterised by manufacturers. To me size-resolved number production fluxes based on Long10 look more realistic and should compare better with ambient size distributions than previous source functions. Author's effort in producing full relevant size spectrum source function is highly acknowledged.

Figure 3 Authors are right when stating that it is difficult to evaluate source functions in the face of scarcity of measurement data. Indeed, some emerging studies suggest that the existing source functions for marine OM can be both too weak (yet unpublished) or too strong (Fuentes et al. 2010, ACP). However, Figure 3 suggests that sea spray particles reach saturation in OM at $\sim$0.3 ug/l chlorophyll concentration. That really

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brings a question how reliable remote sensed chlorophyll data really are or whether chlorophyll is a good proxy for predicting biogenic marine OM. Considering the average chlorophyll concentration of ∼0.23 ug/l would suggest that sea spray should be globally enriched in OM to almost saturation level, which is clearly not the case (Fuentes et al., 2010, ACP; Modini et al., 2010, ACP). I would suggest that the authors may consider emphasising the uncertainty of their source function (outside the evaluated uncertainty) as it is heavily constrained by two datasets (Keene 2007 and Facchini 2008). However, it could well be that chlorophyll concentration is not the best proxy globally. In that case prediction by chlorophyll could be overestimated in subtropical regions, where there is little chlorophyll, but a lot of water soluble OM. Taking that into account would make OM vs chlorophyll relationship shallower. Also Facchini et al. (2008) paper suggests much higher enrichment of insoluble OM versus soluble OM. I guess there is a room for improvement in Long10 parameterisation accommodating physico-chemical features of DOC/POC as more experimental evidence becomes available.

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