Review of: Model study on the dependence of primary marine aerosol emission on the sea surface temperature

The study presents an evaluation of the multi scale model system COSMO-MUSCAT using measurements from two EMEP intensive campaigns in June 2006 and January 2007. The major emphasis of the paper is on the comparison of the sea surface temperature (SST) dependence on primary marine aerosol emission. Long et al. (2011) was chosen as basic emission function for primary marine aerosol (PMA). SST-correction for PMA emission flux was achieved using Jaeglé et al. (2011), Sofiev et al. (2011), and Zábori et al. (2012) corrections. It is found that the SST correction helps bring the coarse mode PMA into better agreement with the observations. Primary organic aerosol of marine origin was also modeled using Long et al. (2011) parameterization. As the model was shown to underestimate the observed organic carbon concentration at Cape Verde Atmospheric Observatory, it is suggested that the formation of secondary organic material needs to be included in the model to improve the agreement with the measurements.

The paper is poorly written and often very confusing. There is excessive information without clear focus throughout the study. Experimental measurements are not well described and the reasoning for the modeling approach is not clear. I also did not see clear formulation of the scientific objectives. I recommend rejection of the paper in its current form.

Major Comments:

There is no clear justification given why Long et al. (2011) parameterization was chosen as the basic emission function. Of course, authors are free to choose any parameterization they want for the study, but the reason for the selection should be clearly explained. How did other parameterizations of PMA compare to the experimental measurements? In addition to Cape Verde data, authors should use additional measurements reported in the literature. Decisions based on the data collected at a single station for the limited amount of time with one specific measurement instrument may lead to erroneous conclusions.

The motivation for selection of the observation stations is not clear. Fig. 1 below shows geographical locations of measurement stations used for the model evaluation. Why were stations so far inland selected for the evaluation of PMA *emissions* schemes? How well was the transport and deposition captured by the model? How was the data selected? What are the accuracies of the measurements? Was there any back trajectory analysis done? Overall, there is so little information given that it is impossible to even evaluate the relevance of the data for the model evaluation.

It is not clear why Long et al. (2011) parameterization was selected for modeling marine organic matter (OM). Authors quote a paper by Gantt et al. (2012) that shows that Long et al. (2011) parameterization did not capture seasonality of marine organic aerosol at Mace Head and Amsterdam Island. Also, when using Long et al. (2011)

parameterization, model-predicted surface concentrations showed zero or negative correlation with monthly- and weekly-averaged observational data (Gantt et al., 2012). Recent paper by Rinaldi et al. (2013) gives further insight into organic matter enrichment in submicron primary marine aerosol. It should be noticed that Long et al. (2011) parameterization yields very high enrichment of organics in sub-micron fraction. For example, for [Chl-*a*] = 0.3 and 1.0 Eq. (12) in the current paper yields $R_{V,1}$ between 1400 to 0.3 and 2200 to 0.33, respectively for 0.01< Dp <1 μm. Under such high organic enrichment (see Fig. 11 in the current paper) it is hard to argue "the influence of OM on water uptake can be neglected." The assumptions used in the current paper on hygroscopic growth of sub-micron PMA can lead to considerable uncertainties.

I recommend the discussion regarding the effect of temperature on the bubble spectra to be removed. Firstly it is quite inaccurate, secondly much detailed discussion can be found elsewhere. The process itself going from ocean bubbles to sea spray size distribution is not as simple as it is proposed here. The radius-depth trajectory (Fig. 35) in Lewis and Schwartz (2004) is for still water at 4 km depth and may not describe accurately bubble population in real environment. See e.g., Fig. 4 in Wu (1981) for water T=14 and 3 °C.

Most of the figures are also confusing. What does MMS on Fig. 2 stand for? What is the point of Fig. 3? How was Figure 4 generated? Manuscript suggests that the "remaining gaps were filled by linear interpolation." Fig. 2 below shows MODIS Terra and Aqua at 9 km resolution merged data that I was able to download. This figure shows large areas of the open ocean (above 45N) covered by clouds. Current paper does not explain how was [Chl-*a*] ~ 1 μg/L prescribed to this region. Concerning Figures 15-18, what are the purple and orange boxes depicting? Quantiles? Usually the bar between them represents the mean or median, what is it here if the other colored dashed lines are medians?

Minor Comments:

Abstract, please give the model's name. As it is now, it says "an atmospheric transport model". The abstract should not be vague.

Pg. 379, line 3 – "traces gases" should be "trace gases"
Pg. 380, line 10 – "an" should be "a"
Pg. 382, line 8 – "Although many components and chemical species could be found, a large fraction is still unknown" please reword
Pg. 391, line 1 – "It is used as driver..."
Pg. 392, line 2 – "growth" needs to be "grow"

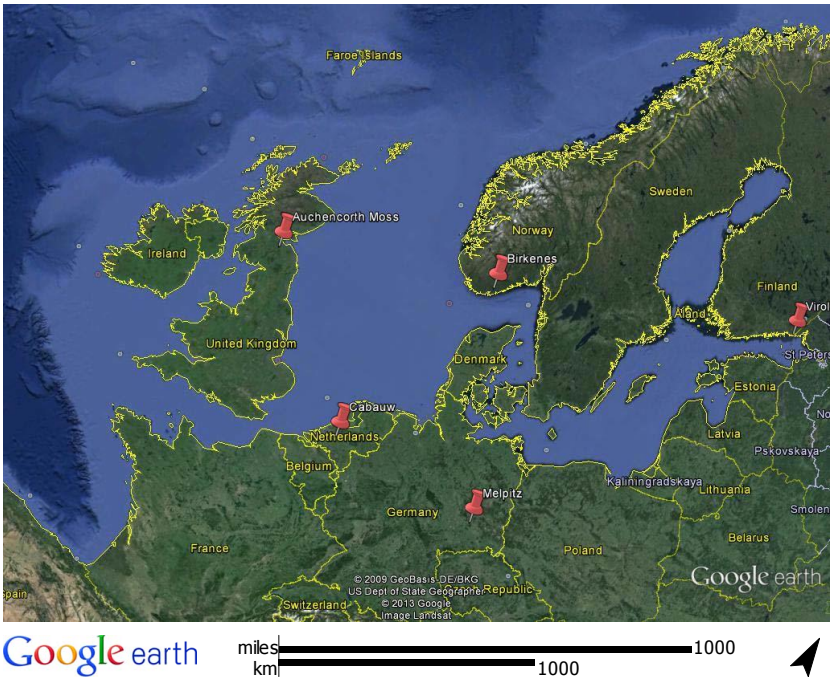

Figure 1. Geographic locations of the stations used for model evaluation.

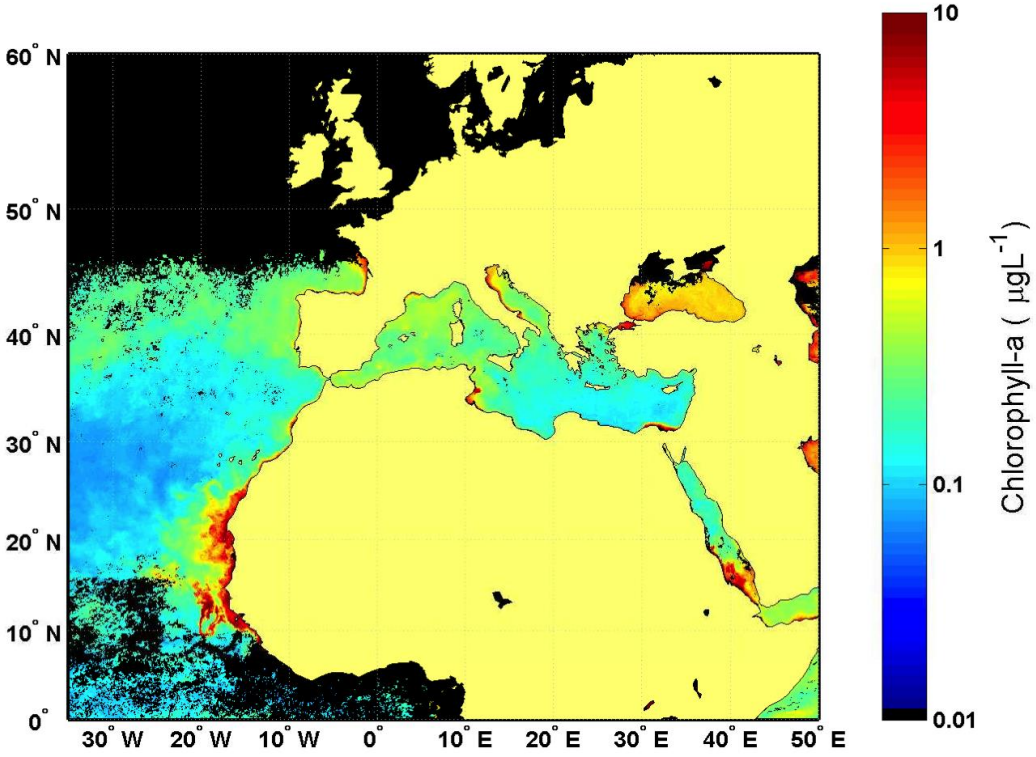

Figure 2. Monthly averaged surface [Chl-a] MODIS Terra and Aqua at 9km resolution, merged data for December 2007. Black color denotes areas with no data.

References:

Rinaldi, M., et al. (2013), Is chlorophyll-a the best surrogate for organic matter enrichment in submicron primary marine aerosol?, *J. Geophys. Res. Atmos.*, 118, 4964–4973, doi:10.1002/jgrd.50417.

Wu, J. (1981), Bubble populations and spectra in near-surface ocean: Summary and review of field measurements, *J. Geophys. Res. Oceans*, 86, 457–463, DOI: 10.1029/JC086iC01p00457