

Interactive comment on “Global distribution of sea salt aerosols: new constraints from in situ and remote sensing observations” by L. Jaeglé et al.

Anonymous Referee #2

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General Comments:

The authors present a new formulation for the sea-spray source function that takes into account that water temperature affects the production of sea-spray particles. The formulation is based on the comparison of sea-spray aerosol concentrations obtained from five cruises in different parts of the world covering the major oceans and sea-spray aerosol concentrations computed with the GEOS-Chem model using the Gong (2003) formulation for the sea-spray source function. The question arises whether this is the most appropriate source function since it is extrapolated from the Monahan et al. (1986) source function to smaller particles, using experimental data from O'Dowd et al. (1997) but for sizes smaller than those for which data are available. Also the large size limit of the Gong (2003) source function is larger than that given by Monahan et al.

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More recent source functions exist, e.g., from laboratory experiments by Mårtensson et al. (2003) and surf zone experiments by Clarke et al. (2006). These latter source functions are in reasonable agreement and comparison of these with Gong (2003) show clearly that Gong falls off too fast at the small particle end. This is important for the smallest particles considered in the work by Jaeglé et al. of $r_{\text{dry}} = 0.01\text{--}0.5\text{ }\mu\text{m}$ (25696, 9). The authors should explain their choice for Gong et al. and evaluate the problem at both the small and large particle end.

The comparison of model results and experimental data shows that the model concentrations are much too high. The authors first investigate whether the wind speed dependence used in the model fluxes would be too high and propose an alternative formulation for the sea-spray source function with both a much smaller wind-speed dependence and different coefficients (MODEL-U2, eq 3). However, although this new formulation brings observation and model closer together this correction is not sufficient. The evaluation of MODEL-U2 is only briefly discussed in section 4.4 and the focus of the rest of the paper is on the sea surface temperature (SST) dependence. I suggest that the conclusion on the less good performance of MODEL-U2 is included in Section 7 or 8.

The ratio of experimental and model concentrations appears to be clearly dependent on SST and a third order polynomial fit of this ratio to SST is included in the sea-spray source function formulation (MODEL-SST, eq.4). The use of MODEL-SST brings the model results much closer to the observations. This is not surprising since the same observations were used in the development of the SST dependence and its evaluation. I suggest that the authors mention this in the manuscript and that the real evaluation of the MODEL-SSA is in the comparison with the UniMiami ground-based network data, the MODIS AOD and the AERONET results.

Detailed comments.

Title: hyphenate sea-salt

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25701, 3-13: dry vs wet deposition: It is a bit surprising that the removal of data influenced by precipitation does not influence the comparison. This would imply that precipitation is very well modeled. How is wet deposition included in the model?

25701, 14-eq (3): the observations are local, the model considers an, in comparison, very large grid of 2x2.5 degrees. Are variations across the grid accounted for and how?

Figure 4 shows ratio and r , I suggest to also provide the regression lines (intercept, slope) to help understanding the further analysis, in particular the derivation of eq. 2. C_{obs}/C_{model} would be simply given by the slope (is that what's meant with ratio in the Figure legend?) since Fig 4 suggests a straight line, and E_{obs} would be simply a multiplicative factor times E_{model} . However, if that is true, I don't understand how the wind speed dependence would change. Apparently the C_{obs}/C_{model} ratio has a non-linear dependence on wind speed, but that's not clear from the text nor from the Figures. I assume that eq (2) is evaluated for each individual data point to get to Figure 5. Could you please clarify the issues raised here in the text?

25704, 15: Sellegri et al. (2006) also addressed water temperature dependence

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