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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of modelled sulfate radiative forcing to DMS concentration and air-sea flux formulation" *by* J.-E. Tesdal et al.

Anonymous Referee #2

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'Sensitivity of modelled sulfate radiative forcing to DMS concentration and air-sea flux formulation' by Tesdal et al.

This paper presents sensitivity runs using a GCM with an explicit aerosol scheme in order to quantify the impacts of using different seawater DMS climatologies and sea-air flux parameterizations. This study adds to the numerous studies in literature which have had similar aims at understanding the effect of DMS and impacts of changes in the DMS flux.

Unfortunately, in its current form, the manuscript does not add significantly to the literature and hence I cannot recommend its publication. Most of the work presented here

has been done before and simple sensitivity studies do not add much to our current knowledge on the effect of DMS, especially when there is no comparison done with observations.

1) The suitability of the model to study the impact of DMS on aerosols, cloud properties and radiation changes has not been demonstrated. Have the authors compared the model results to any observations? Several climatologies of sulphate are available, and locations with atmospheric DMS and SO2/H2SO4 observations can be also compared to show that the model is indeed close to the observations or highlight the major shortcomings of the model.

2) What is the reason behind including sensitivity experiments based on seawater climatologies K99, K00 and L10? L10 is an updated version of the K00 and K00 climatology. It seems unnecessary to include all three considering the L10 climatology is an upgrade and covers all the data that went into the K99 and K00 climatologies. This would save a lot of discussion, which could be focused on other important features of the results. It is important to note that they are not different climatologies, but rather upgrades and hence only the latest should be used.

3) Also, is there a specific reason the authors include the AN01 seawater DMS climatology? This climatology, in particular, does not compare well with observations of seawater DMS, which shows that estimating DMS using global fields of chlorophyll, nutrients and light is not an accurate method considering the complexity in the oceanic DMS cycle. Additionally, a similar exercise has been by the authors in another paper (Tesdal et al., Environ. Chem., 2015).

4) Lana et al., 2011 did a comparison of the total flux of DMS using different climatologies, similar to that presented in table 3. A recent publication, Mahajan et al., JGR, 2015 (which is not cited) has done similar comparisons to those presented in this paper, but in further detail including spatial features, quantifying the impact of a change in the DMS climatology. Woodhouse et al, ACP, 2013 have studied the effect of changing

the DMS flux on regional scales. This raises the question as to what is new in this study. The differences by changing the sea-air parameterisations are not surprising, while changes due to different climatologies have been studied before using different models.

5) Why do the authors focus only on global and annual means? Considering the large spatial and seasonal differences between the climatologies, one would expect large regional difference (e.g. Mahajan et al., JGR, 2015) and these features would be smoothed out by taking global and annual means. This could be a reason for the linear dependence of the results, when in reality regions with different background aerosol loading, or relatively lower anthropogenic SO2 emission would show highly nonlinear dependence considering the multistep chemistry-physics interactions to go from sea-water DMS to cloud properties.

6) Does the model include ternary nucleation of H2SO4 and MSA with organics, which have recently been estimated to play a major role in the conversion of DMS to aerosols? (Dawson, PNAS et al., 2012; Riccobono et al., Science, 2014).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 23931, 2015.