Atmos. Chem. Phys. Discuss., 10, C1226–C1228, 2010
www.atmos-chem-phys-discuss.net/10/C1226/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantification of DMS aerosol-cloud-climate interactions using ECHAM5-HAMMOZ model in current climate scenario" *by* M. A. Thomas et al.

Anonymous Referee #2

Received and published: 4 April 2010

Using an aerosol-chemistry-climate general circulation model, this manuscript quantifies the contribution of ocean DMS emissions to seasonal and global changes in cloud microphysical and radiative properties. The paper is well written and appropriate for ACP. The title clearly reflects the contents of the paper and the scientific methods used are well described. However, there are some issues discussed below that I think should be addressed prior to publication.

General comments:

1. While the major discussion in the paper is about the production of SO4 through DMS oxidation, the role of MSA should be clearly discussed. Research shows that

methane sulphonate (MS-) (in addition to oxidation products of ocean emitted isoprene, monoterpenes and amines) is a major contributor to secondary organic aerosols associated with marine ecosystem productivity.

2. Current research also demonstrates that ocean derived primary organics contribute up to one third of submicron sea spray aerosol mass and have considerable effect on marine CCN number. Since both primary and secondary organics of marine origin are associated with ocean ecosystem productivity, analogous to DMS, potential role of organics should be discussed. Authors should perhaps use some of the recently developed sea-to-air marine organic aerosol flux parameterizations (e.g., Vignati et al., 2010; Gantt et al., 2009) to assess what fraction of the remotely sensed data (e.g., effective radii, CDNC) can be attributed to marine aerosol sources other than sulfates.

3. Clearly sea salt is one of the important contributors to AOD (CCN) over the oceans. More details should be given regarding the sea salt source function used in the model and how does that function compare to other parameterizations. How the conclusions drawn in the paper will change for different sea salt (sea spray) parameterizations.

4. Paper shows that aerosols, formed over the oceans through DMS emissions, can have significant effect on cloud microphysical and radiative properties. Is there a potential feedback of such interaction? Can modulation of cloud properties affect local metrology (e.g., water vapor, atmospheric temperature profile, sea surface temperature, subsidence, etc)? Can such interactions also influence large scale wind fields over the Southern Ocean?

Specific comments:

1. There should be clear distinction between CDNC and "number of activated aerosol particles." Is this in-cloud or grid-averaged CDNC?

2. Comparison of Fig. 2 & 3 shows, that while there is very little emissions of DMS from the southern ocean in SON-2000, there is considerable mass mixing ratio of H2SO4. I

believe this is due to the in-cloud production of sulfate. This raises a question should all the clouds 30 to 75S be discussed in a similar way? What is the potential effect of cloud base height on CDNC or droplet radii?

3. Acronym: SeaWiFS (not “SeaWIFS”)

4. Figure 6 is incorrect. It shows radiative forcing (i.e., Fig. 7) instead of effective radii.

5. The main objective of the paper is the effect of DMS over the southern ocean; nevertheless, it should be discusses why comparable DMS emission values over the northern ocean have no considerable effect on overlying clouds.

6. I find it puzzling to see no considerable seasonal effect on latitudinally averaged baseline time series on Figs. 8 & 9. Was there any seasonal variation in wind speed over the Southern Ocean?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3087, 2010.