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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Biogenic influence on cloud microphysics over the global ocean" *by* A. Lana et al.

**Anonymous Referee #2**

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Overall:

This paper attempts to determine which types of marine biogenic aerosols affect the cloud microphysical properties near the emission source using statistical analysis of satellite data as the main tool. The high negative correlations between liquid cloud effective radii and sulfur/organic secondary aerosol production at mid and high latitude regions lead to the authors to conclude that it is these aerosols, as opposed to primary organic and sea-salt aerosols, that are major drivers of the variability of cloud microphysics. Despite the fact that paper is well written and topic is relevant to ACPD, I recommend major revisions for the paper prior to publication in ACP.

Major Comments:

The main critique that I have of this paper is the use of spatio-temporal correlations from satellite data to determine potential causality in cloud microphysical variability. The four types of aerosols examined (sulfur, SOA, primary organics, and sea-salt) have different formation mechanisms. Applying the same correlation method for all four inherently favors one type of formation mechanisms over the other (especially primary vs. secondary aerosols). I would suggest describing in more detail how the aerosol formation mechanisms differ in terms of time scales, and adjust the correlation method to suit this time scale. See Woodhouse et al. (2008) for a modeling study of the relationship between DMS emissions in a particular location and sulfur aerosols.

There also needs to be some evidence that the emission mechanisms used in the paper are accurate. This is shown nicely in Fig. 3 for $\gamma$DMSflux, but not found for the other three aerosol types. I would suggest showing something similar to Fig. 3 (if possible) for the other aerosol types. If the seasonality of emissions does not reflect that of the surface concentrations (see Meskhidze et al., 2011; Westervelt et al. 2011), additional emission mechanisms may need to be included. Accurate inspection of Fig. 1 shows that positive correlations between cloud effective radii and POA/sea-salt occur along 40S. Actually, most of the emissions of POA (Vignati et al., 2010) and sea-salt (de Leeuw et al., 2011) are supposed to occur between 40S and 60S.

None of the time series Figures (2, 4-6) seem particularly helpful. The useful information from the figures is the correlation value, and these can easily be put into a table. If these figures are not removed, they need units and observations (if available) and can be simplified into seasonal averages.

The strong dependence of the results on the Southern Ocean concerns me. This area has a strong seasonality in meteorological factors such as solar radiation, wind speed, and sea surface temperature among others that may affect cloud microphysical properties unrelated to aerosol interactions. I would suggest discussing and possibly accounting for the influence of meteorology on clouds in this region.



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On the other hand, in the case of the Equatorial and Tropical North Pacific it is argued that positive correlations between $\gamma$DMSflux and effective radii are likely due to high altitude clouds. As correctly noted by Anonymous Referee #1 authors should limit their statistical analysis using clod top pressure and temperature. Moreover, when talking about the Twomey effect it is implicitly assumed that cloud liquid water path is constant. I did not see any discussion of this in the manuscript.

Overall, I think in its current form the paper adds very little to the ongoing debate for the ocean biological influence on cloud microphysics. However, the manuscript can be improved significantly if i) spatio-temporal effect of DMS oxidation are included (i.e., DMS may influence accumulation mode aerosol number 10th of thousands of km downwind) and ii) more detailed satellite data processing is carried out for clouds.

References:

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3655, 2012.