

Based on a set of measurements on blowing snow particles and aerosol particles made during a winter cruise on board the icebreaker *RV Polar stern* within the Weddell Sea sea-ice zone during austral winter (June-August 2013), this work tested and validated model parameterizations of sea-salt aerosol production. This work is very innovative and of great interest for atmospheric chemists, climate researchers and the ice core people community. Whereas at the global scale, sea-salt aerosol emitted from open-ocean represents an important contribution to the atmospheric aerosol load (this is even more true in the southern hemisphere), the understanding of emissions and atmospheric fate of sea-salt aerosol at high latitudes is indeed important for several reasons. First, sea-salt aerosol represents a large source of halogens that if activated plays an important role on the reactivity of the atmosphere over these regions. Second, related to the presence of sea ice, emissions from blowing snow or other sea-ice-related processes (e.g., frost flowers) could represent a significant (possibly dominant) sea-salt aerosol source with respect to the common sea-salt emissions from open-ocean in these regions. That offers the possibility to reconstruct an important proxy of the past climate, namely, the sea ice, through the study of sea-salt ice core records.

Overall the manuscript is well organized and clearly written. The discussion of data is very well conducted, and I enjoyed reading it. I therefore recommend publication of the manuscript, after authors consider the following (minor) points rise below.

Abstract, line 28: Please specify “at a production rate of 10 SSA formed from one snow particle”.

Line: “very similar results” is too vague, please specify: Something like “Although both mechanisms generate very consistent results with respect to observed aerosol number densities.”

Introduction: You can remove line 30: A brief conclusion is presented in section 6.

Section 3.1: line 23: “from the “ appears in bold in my version.

You said “The control run for open ocean sea spray is SI_Base_OO, following the scheme by Jaeglé et al. (2011)”. But this run is denoted OO_Jaeglé in Table 1, right ?.

Section 3.3.4 : Be carefully (remove) with abbreviations NH (northern hemisphere) and SH (southern hemisphere) since you already used SH for high salinity.

Section 3.3.7, line 14 : Please correct «single »

Section 4.2: Here or in figure 5 caption, may be good to indicate references for the observations at the different polar sites.

Conclusion:

I think the sentence “However, the aerosol concentration (Frey et al., in preparation) gradient observed between near surface (~2m above snow surface) and ~29 m will not allow us to conclude robustly where the SSA is produced. “ is an important new information that would appear earlier in the manuscript (the conclusion is not exactly the right place for such a new information).

Whereas I fully agree with your conclusion “Thus, this highlights the need for further in-situ observations and laboratory investigation to fill this gap.”, but it may be nice to be more precise here. For instance, did the study of the size segregated chemical composition of sea-salt aerosol that can cover the range between 0.03 and 20 micron diameters can help ? In addition to extend the information towards the smallest particles, such chemical information (the sodium to sulfate fractionation for example) would permit to investigate the mixing between particles emitted from open-ocean and from marginal ice.

Figure 1: Please introduce also the green line (open ocean) in the caption.

Figure 3: The vertical scale (10^{-4} to 10^3) is the same for the three panels so, removing the numbers in panels b and c, would permit to increase the horizontal scale and to better see the difference in the observations between panels a, b, and c. If not (or in addition), please add a vertical dashed line at one micron on the three panels

End of the review.