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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "First direct observation of sea salt aerosol production from blowing snow above sea ice" *by* Markus M. Frey et al.

Anonymous Referee #2

Received and published: 13 June 2019

Frey et al present an observational study of sea salt aerosol (SSA) production from blowing snow above sea ice, through measurements during winter 2013 in the Weddell Sea, Antarctica. Since the modeling hypothesis presented by Yang et al (2008, GRL), the mechanism of SSA production from blowing snow has been implemented in numerous modeling studies, unfortunately without observational evidence of the mechanism itself. This work provides a detailed study of the proposed mechanism through measurements of size distributions and inorganic chemical composition of aerosols and blowing snow, and comparisons to modeled parameters of blowing snow SSA production. Given the prevalence of the use of the blowing snow SSA production parametrization, this is a very valuable study. My comments mainly focus on clarification of the manuscript and assessment of statistical significance throughout. Given the significant

length and many figures and tables, the authors are encouraged to consider moving some material to a supplementary information file if appropriate.

One overarching and major comment that needs to be addressed throughout the manuscript is for uncertainties (or standard deviations) to be listed with average values. This is important for assessing data variability, as well as for assessment of statistical significance. Indeed, statistical tests of significance should be applied to inform whether 'trends' and 'differences' are indeed statistically significant, which would greatly strengthen the findings presented in the manuscript. This is important because trends sometimes seem to be overstated in the text when compared to large scatter shown in the figures. Routine statements of statistical significance would significantly strengthen the conclusions throughout.

I highly recommend reorganizing the manuscript to improve readability. Section 3.2 relies significantly on depletion factors. Therefore, I recommend reorganizing to move Sections 3.4.1-3.4.3 to be before Section 3.2. Also, the current Section 3.4.3 would be best after Section 3.3.

Major Comments: Page 1, Line 21 & Page 25, Lines 14-15: These sentences state generally that 'similar processes take place in the Arctic', yet no supporting discussion is provided. Since the current work focuses on the specific conditions of the Antarctic work and no data are provided to evaluate this statement, these sentences should be removed.

Page 1, Lines 2-3 and Page 3, Lines 5-7: The statement "validating a model hypothesis to account for winter time SSA maxima in polar regions not explained otherwise" generalizes beyond the Antarctic, which is not appropriate, and it also not consider other factors, such as lower boundary layer height and lead-based SSA production. This statement should be rephrased to focus on validating wintertime SSA production from blowing snow (which is excellent), as a comprehensive discussion of wintertime SSA maxima causes in both the Arctic and Antarctic is not presented in this work. Further, the work of Huang and Jaegle (2017) did not consider the observed influence of lead-based SSA production in the Arctic (May et al. 2016, JGR). I suggest focusing on the Antarctic, as this is the strength of this work.

Figure 1; Page 3, Lines 30-33; Page 13, Lines 22-23: Please provide a legend for sea ice concentration. It appears that stations S2, S3, and S9 were in areas of reduced sea ice concentration. While there is significant evidence for blowing snow SSA production based on chemical analyses, a discussion of the distance to open leads, in addition to open water (Page 3, Line 32), needs to be included, since there is measurement evidence of wind-dependent lead-based SSA production (e.g., Nilsson et al. 2001, JGR).

Page 7, Lines 3-5: Please clarify whether these time periods of ship exhaust influence were also removed from the aerosol size distribution data, as they should be.

Page 7, Line 9 and Table 3: LODs are normally defined as 3*sigma, rather than 2*sigma. What is the authors justification here? Also, LODs should be reported with one significant figure (too many shown in Table 3, which can be misleading).

Tables 4-5: Data below the LOD should be labeled as such, as exact values below LOQs are not meaningful.

Page 8, Lines 3-5: Instead of reporting depletion factors, I highly encourage the authors to consider reporting "enrichment factors" (e.g. Krvanek et al. 2012, Atmos. Environ.), which are more intuitive to understand in my opinion (i.e. enrichments are >1, depletion corresponds to <1).

Page 8, Lines 8-11: I am quite concerned that data were selectively removed from the datasets presented. I can understand if certain samples are not used for externally identified reasons, but if, for example, sulfate concentration is removed for a given sample, I'm concerned about continuing to use other ions from that sample, as appears to have been done based on the numbers shown in Tables 4 and 5. I worry that

the presented datasets are skewed based on the removal of these datapoints. What fraction of the time did ship emissions impact the dataset? It needs to be clarified what fraction of the data were removed. This data treatment is very important for later statements about the distribution of depletion factors (e.g., statements on Page 10, Lines 7-9).

Page 9, Lines 28-30; Page 10, Lines 1-3: Please reference where these data are presented, or please add them as supplementary information.

Section 3.4.2 and associated text in Conclusions: The authors should be mindful that only aerosol and snow bromine were measured and that no measurements of reactive bromine are presented. Therefore, the strength of the implications for reactive bromine production should be weakened to account for this uncertainty and other factors that contribution to reactive bromine production and abundance.

Page 21, Lines 9-10: Depletion factors examine the degree of depletion, but they do not provide information on the mass present. Therefore, the data here cannot assess contribution to the fraction of net bromine release, as currently presented, especially without reactive bromine measurements.

Page 19, Lines 22-25: This analysis is only valid if you assume there is no precipitation of NaCl.2H2O. Please verify that based on temperature, and perhaps take out the very low temperature points.

Page 19, Lines 127-28: Does this also mean that the aerosols collected were a mixture of sea salt emitted from the ocean and sublimation of blowing snow?

Page 22, Lines 32-33: A conversion factor is used to calculate [SSA] based on Na+ and using seawater composition, but this seems to undermine and not take into account the sulfate-depletion observed.

Page 21, Lines 30-31 and elsewhere: Is this U10m and the associated data in Fig 16 an average, or threshold? It isn't clear how the data were binned. Please clarify calm and stormy conditions. Does calm represents U10m<5 m/s? How about stormy?

Page 22, Lines 16-19: It seems "not all water is lost" could represent a large uncertainty of blowing snow sublimation. This is important for reactions that depend on the surface area of aerosols. It could be highlighted in the abstract or conclusion. Also, please justify how to get 10ˆ(-3) um. Using snow salinity of 0.06 psu from Table 5, median snow particle of 100 um from Table 6, yields d(dry) of 1 um.

Page 23, Lines 1-3: Please show this comparison and data in a supplementary file.

Page 25, Lines 5-10: This is not a new finding and has been presented in other work. Therefore, either these sentences should be removed here or other work should be referenced to further support these findings.

Data Availability: Since the current work is expect to be very valuable for informing future modeling work and other studies, I highly encourage the authors to put these data in a public archive.

Figure 7: Please add a legend to give meaning to the colors presented. Also, it is stated throughout the manuscript that the surface snow is typically significantly sulfate depleted (justifying the sea ice source for sulfate-depleted aerosol), but here the surface is more often near 0. Please clarify.

The highly relevant work of Giordano et al. (2018, ACP) “The importance of blowing snow to halogen-containing aerosol in coastal Antarctica: influence of source region versus wind speed” should be considered in this manuscript.

Minor/Technical Comments: Throughout the manuscript, watch for ‘paragraphs’ that are only 1-2 sentences, as this disrupts the flow and limits discussion. Consider reorganization to prevent this.

Page 1, Line 9: Please state the size of the sulphate-depleted aerosol.

Page 1, Line 13: Based on the data presented later, ‘enriched’ is likely a typo and

should be ‘depleted’ here with respect to aerosol at 29 m.

Page 2, Line 20: Provide a reference to a SSA review here.

Page 4, Lines 27-28: I think it is dividing kappa instead of multiplying. Please check. Also, please provide the value for the von Karman constant in parentheses.

Page 5, Lines 12-14: Please provide a greater description of the inlet. Also, please clarify whether the data presented where corrected for these particle loss estimates (‘we adopt’ is confusing phrasing).

Page 5, Lines 22 and 27: Please clarify the size range of aerosol collected.

Page 9, Line 8: I assume the authors are discussion temperature in degrees Celsius, but this needs to be stated.

Page 9, Line 14: Where is the timing of the snowfall presented/shown?

Page 9, Line 22-23: Please provide a reference that connects the friction velocity with the boundary layer conditions. Also, reference where these data are shown, or add to a SI.

Page 10, Line 16: Please clarify “two 7-10 day long periods”. I’d suggest wording such as “two periods, one lasting 7 days and another 10 days”, or similar.

Page 11, Lines 3-4: Please provide concentrations in parentheses for context.

Page 13, Lines 16-17: The direct comparison of N0.4-12 to dp<2 um here is confusing since these are different size ranges.

Page 14, Line 15: Please define SWE (snow water equivalent?) and the ‘saltation layer’ (what height?).

Page 15, Line 3: What does “(0.001)” correspond to here? Please clarify.

Page 15, Lines 6 and 11: Please clarify that Ut and u*t are calculated, not observed.

Page 15, Line 15: Please show how u*t values were calculated.

Page 15, Line 32: Please define what you mean by ‘minor’ here. Please quantify.

Page 17, Line 13: Please delete “have” typo.

Page 17, Line 3: Didn’t mean dp increase?

Page 19, Line 32: Do you mean 0.1204 here?

Page 20, Lines 8-10: The wording “well established” should be removed, as the Yang et al papers are models based on a hypothesis rather than measurement based and this associated uncertainty should be noted.

Page 20, Line 27: Data in Table 5 are presented in ug g-1. Please fix or clarify.

Page 21, Line 11: Change “due a” to “due to a”.

Page 21, Line 14: No data are presented examining the acidity of the surface snow-pack.

Page 23, Line 22: Delete extra “the”.

Page 23, Lines 29-30: Remove “always” and replace with “often” to more appropriately reflect the data shown.

Page 25, Line 27: “LL & MM”?

Figure 16: The variations in these distributions (e.g. standard deviations) should be shown.

Figure 17: This figure is difficult to understand currently.

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