

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2022-442-RC2>, 2022
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Comment on acp-2022-442

Anonymous Referee #2

Referee comment on "Atmospheric particle abundance and sea salt aerosol observations in the springtime Arctic: a focus on blowing snow and leads" by Qianjie Chen et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-442-RC2>, 2022

This manuscript provides a valuable observational study that sheds light on the role of blowing snow, from sea-ice or terrestrial surfaces, for primary aerosol production in the Arctic. The authors use a combination of blowing snow observations, and bulk aerosol and single particle composition measured over an approximately 1 month period in April-May 2016. The authors conclude that if blowing snow does produce primary aerosol during observed blowing snow events, then this aerosol material must have first come from deposition of sea-spray aerosol onto snow surfaces, rather than upward migration of brine into the snowpack. This is a valuable result; however, the direct support for this conclusion from observational data is tenuous. While I recognize the significant challenge associated with collecting this dataset, the conclusions drawn may be too strong given weaknesses in the underlying data. I believe the authors can address this by more clearly articulating the potentially significant limitations in their dataset and tempering their conclusions accordingly.

Major comments:

(1) Differences in Mg/Na and Ca/Na ratios are a key result used to draw conclusions about whether blowing snow or direct SSA produced in leads is driving the enhanced aerosol numbers at high wind speeds and during blowing snow events. This data arises from both bulk aerosol composition and single particle analysis, and appropriate discussion is given to the mis-match between bulk and single particle observations. However, these data sets have some significant issues that need to be more clearly addressed in the manuscript. First, the number of individual particles corresponding to the measurements in Table 3 (and Figure 6) should be made very clear. Second, given the large range on Mg/Na and Ca/Na ratios in Table 3 (some as large as the mean), this small number of samples cannot be used to directly conclude that aerosol during observed blowing snow events is more enriched in Calcium and thus any observed aerosol during BLSN events must have arisen from SSA deposition rather than brine migration. Third, in Figure 5, are the differences between Ca/Na ratios in snow-pack between the different BLSN and wind-speed periods statistically significant? The 1-sigma range of measurements are large, while the absolute differences are fairly small for both sub-micron aerosol and tundra snowpacks. For these three reasons, the conclusions drawn are tenuous at best, and should be described as

such in the manuscript.

(2) From previous observations of blowing snow, what is the approximate horizontal extent? Is it possible that this is heterogeneous on the scale of 5-10's of km? From comparison of airport and ceilometer BLSN observations, this does appear to be the case. The implications of this for diagnosing blowing snow in source regions to the author's sampling site should be clearly described.

(3) The manuscript is generally well written; however, the Results & Discussion section corresponds to a very detailed description of the results only, with very little interpretation or guidance given to the reader. In conjunction with this, the Conclusions section is very long, and much of the interpretation that would guide the reader through the at times complex results, resides in the Conclusions. The manuscript will benefit from (1) restructuring the Results & Discussion and Conclusions section to better describe the main outcomes and interpretation of the dataset, and (2) focused discussion of the limitations, potentially within its own section of the Results & Discussion.

Specific comments.

L423-426: This suggests lower concentrations between 1-2 μm (not 1-4 μm), and the conclusion that these particles are "removed" by blowing snow is based on observation of a correlation, rather than direct causation. The calculations described in the following sentences do support this conclusions, but the authors should be clear about the limitations in their ability to assert a mechanism.

Figure 4: Given the variability in many of these observations, rather than showing only the average \pm 1 standard deviation of these measurements, it may be more useful to the reader to show e.g., a box and whisker plot, with all data points (or violin plot) shown behind.

Section 3.3.2: A large amount of space is dedicated here to establishing the importance of Arctic Haze during moderate wind periods. This is well known, may not be supporting the main conclusions of the manuscript. I suggest the authors consider whether this detailed description is needed in the main text.

Figure 6: The total number of sampled particles in each size bin should be overlaid on top of the composition data in both panels of this figure.

Section 3.3.3: Please be more clear about where the absolute number of individual particles collected are presented within this section (presumably this corresponds to data

presented in Figure S3?). This discussion is challenging to follow without a summary of these numbers and clear indication of how many individual particles these percentages are based upon. Further, a large amount of space is given here (and in lines 736-746, Section 3.3.4) to establishing that bulk aerosol composition is not representative of individual aerosol mixing state. Since this is relatively well understood in general, I question whether such a large amount of text is required to describe this. It is certainly important, and can likely be gleaned from a short discussion and reference to the bulk and single particle data figures.

L692-694: It is unclear how this final statement follows from the information covered in this paragraph. Please revise.

Lines 748-752: Are these differences in SO₄/Na ratios statistically significant?

Lines 833-835 (and in general): Has complete lack of Ca enrichment during upward brine migration been shown in previous literature, or do the authors assume that no enrichment occurs in the brine migration process?