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Comment on acp-2022-442

Anonymous Referee #1

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-RC1>, 2022

Review of "Atmospheric particle abundance and sea salt aerosol observations in the springtime Arctic: a focus on blowing snow and leads"

Chen et al. provide an interesting case study of aerosol and sea salt observations taken on the North Slope of Alaska. The results are then used to evaluate parameterizations for predicting blowing snow as well as the sources of SSA (e.g., sublimation of blowing snow or sea ice leads). The manuscript is well-written, although it is too long in certain sections and it might benefit from some cuts to the lengthy discussions of particle composition, providing more of a summary and leaving the details in the tables and figures.

Overall, the manuscript is of high technical quality, but certain key measurements, particularly the SEM-EDX analyses, have only been taken on a small number of samples, which decreases confidence in the representativity of the results. The best solution would be to analyze more samples, but I realize that this might not be feasible given the nature of field work in the Arctic. An alternative would be to modify the discussion and conclusions to better acknowledge these limitations. I support publication in ACP once my comments have been considered.

Specific comments.

Blowing snow observations: Is there anyway to evaluate if the blowing snow observations, which were taken at other sites, reflect the conditions at Utqiagvik? The distance between the airport or the DOE ARM site and the Utqiagvik site is approximately 5 km. Is it possible that blowing snow occurs at different times at the different sites? Could this

explain the large number of periods that blowing snow is falsely predicted? It is very important to provide further details about the topography, vegetation (probably not relevant for an Arctic site) and local meteorological conditions to evaluate the potential differences between the sites in this respect.

SEM-EDX analyses: The conclusion that the SSA must be emitted from bubbling bursting within open leads or from blowing snow sublimation from snow primarily impacted by sea spray aerosol deposition is based on the SEM-EDX analysis of individual particles. While this approach is appropriate, the analysis was only carried out for two days during the field work. In general, the discussion of the calcium and magnesium enrichment in sea spray aerosol arrives at conclusions that are too strong given the limited number of measurements. For example, only 7 measurements of Ca/Na are reported (Table 3), of which 3 samples have a standard deviation that is equal to the average measurement, which makes drawing conclusions difficult. After removing these weaker data points only 4 measurements remain (collected over 2 days), 3 of which exhibit significant enrichment in Ca/Na. Any conclusion regarding Ca enrichment is thus very weak and should be acknowledge as such in the text.

Further to this point, it would be beneficial to the manuscript to discuss in greater detail the methods used in the quantification of the elemental concentrations by SEM-EDX, especially because the ratios of elements are being considered across multiple studies. Does one need to account for differences in sensitivity to different elements? Were standards evaluated to verify the quantification?

Minor corrections and comments.

Lines 156 – 158: Is it possible that there is long-range transported anthropogenic pollution that is not correlated with NO_x?

Lines 300 – 302: Similar to the comment above, an alternative explanation for the apparent discrepancy is that the observations of blowing snow and the measurements of wind speed were taken at different locations.

Line 369: The last sentence here is confusing, since nowhere in the preceding paragraph are the total particle number concentrations between 0.01 – 0.3 μm discussed. I would recommend including a table with this data, including some of the different size fractions discussed in the text.

Lines 396 – 407: It would help to clarify the discussion if the authors provided some example calculations of the diameter of the blowing snow particles under the assumption of multiple SSA particles being produced from the sublimation of one larger blowing snow particle.

Lines 438 – 439: Do the authors have any explanation why there is no observed difference between the different periods for the particles with diameters between 4-5 μm ? According to the information given in the text, these particles should also be scavenged by blowing snow.

Figure 6: It is important it indicate in this figure the number of particles characterized for each size bin (n). The value of n could be indicated along the top axis of the figure. Based on the data shown in Figure S3, it seems like in the largest size bins only a few tens of particle were analyzed, but it is hard for the reader to determine that because one needs to check both the SI and Figure 6, compare size ranges, dates, etc. This is an important point because one should be cautious about drawing strong conclusions from limited measurements, if that is the case here.

Lines 649 – 675: This section is quite long, and I think it can be trimmed to bring forward the important results suitable for a journal like ACP. The detailed comparison in the paragraph on line 649 – 666 is unnecessary as much of the information given is present in Figures 4 and 5. Also, since only 4 supermicron samples were taken I don't think the authors can draw significant conclusions by comparing the samples. The following paragraph should also be cut or reduced. It seems that the main conclusion is that bulk elemental ratios aren't representative of individual particles from different sources, which seems rather trivial.

Lines 690 – 692: This sentence is confusing. What two things are similar? Please rewrite.

Lines 748 – 751: I wonder how statistically significant the differences are between the sulfate/sodium ratio for the snowpack and seawater (0.04 ± 0.01 vs. 0.06). Is this significantly lower?

Specific comments on SI:

Formatting: Equations should be placed on their own lines and not imbedded in the text. (I am making this comment because the SI is not formatted by the journal during publication.) Also, some of the figure captions run over to the next page. If the figures and their captions were limited to one page that would make the SI easier to read.

Table S1: How are the LODs determined?

Figure S4: Is it really possible to separate aged SSA and organic aerosol using SEM-EDX in this study? The two EDX spectra are very similar, and both C and O cannot be measured due to interferences. Also, one would expect for aged SSA and organic aerosol to be present in internally mixed particles to some extent.