Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-413

Anonymous Referee #2

Referee comment on "Measurement report: High Arctic aerosol hygroscopicity at sub- and supersaturated conditions during spring and summer" by Andreas Massling et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-413-RC2, 2022

Massling et al. present in their manuscript observational results of aerosol hygroscopicity and aerosol cloud activation from two field campaigns carried out in spring and summer 2016 at Villum Research Station in Northern Greenland. These are valuable observations from a part of the world where observations are generally scarce but are needed to better understand aerosol-cloud and aerosol-radiation effects on Arctic climate. As such, it is important that this data and findings will be published and be accessible to the scientific community.

The paper is well written, although sometimes very lengthy and the authors are encouraged to shorten and constrain their manuscript where possible. A few but important technical details are missing and need to be added to the revised version. In addition, further clarifications (described in detail below) should be made before the manuscript can be accepted for final publication.

Detailed comments (in chronological order):

- Abstract: The last three sentences are very general statements and partially more like an outlook. Suggest to delete them.
- Introduction (2nd paragraph): The authors might not be aware of it, but the effect of water-uptake on particle light scattering has been actually directly measured/studied in the Arctic by Zieger et al. (2010). The scattering enhancement is indeed significantly larger compared to other continental or maritime sites (see e.g. Burgos et al, 2019) due to the special interplay between size and hygroscopicity in the Arctic (Zieger et al., 2010).
- Introduction (3rd paragraph): The regional characteristics of the Arctic (and the corresponding aerosol properties) are actually quite diverse (see e.g. discussion in Schmale at e., 2021). Certain parts of the Arctic such as the Siberian Arctic are exposed to high levels of anthropogenic/industrial activities, while the high/central

Arctic e.g. shows often different seasonality in aerosols properties than the lower Arctic. This is also important for the different drivers of new particle formation (which will be addressed later in the manuscript) as e.g. discussed in detail by Schmale and Baccarini (2021). I therefore recommend that the authors should more carefully define what they actually mean with "the Arctic" for their study and also mention the regional diversity.

- Introduction (4th paragraph): The work by Jung et al. (2018), which is cited later in the manuscript, should be mentioned here as well, since it represents a long-term study of CCNC measurements in the Arctic with the same instrumentation used here.
- One important missing part are the details on the particle sampling. Please add a of the actual set-up sketch to the revised version. Please also add information of the used tubing (type, inner diameter, length, etc.), the inlet type (with or w/o size cut? height above ground, manufacturer, etc.). Where were the meteorological parameters measured (e.g. is the temperature shown in Fig 2 and 3 measured directly at the inlet)?
- Concerning the SMPS: Was a pre-impactor used? Was the SMPS data corrected for losses? If yes, how and which assumption about the particle density was used? Were the size distributions validated to a total CPC?
- Concerning the HTDMA: It is important to also state information of the RH of the dry DMA / selected diameter (please also add this data column to the data).
- Line 208: As already mentioned above: Have you compared or used the total CPC to better judge which instrument was mal-functioning?
- Line 232: Is the RH accuracy given in absolute or relative terms?
- Section 3.2: For a better interpretation of the size distribution data, the authors could consider to present the particle size distributions as a contour plot (e.g. hourly and normalized; it could maybe be integrated within Figure 5 and 6). This would be more consistent with the other time series and could maybe help to facilitate the interpretation of the individual features seen in Fig. 5 and 6.
- I am still surprised about the extremely low uncertainties that were retrieved for the CCN concentrations in Table 1 and 2 from the curve fitting. Are they really meaningful?
- Trajectory analysis: The authors mention that they have performed a trajectory analysis (e.g. pager 15, 3rd paragraph and in Sect. 4), but the results are not shown. It would indeed strengthen some on the statements and claims made later on, if this analysis would be added to the revised manuscript. Even simply calculating the time over ice, ocean and land would maybe give some more insights to the respective aerosol sources.
- What is the reasoning of the HTDMA to measure the growth factors at two RH close-by at 85% and 90%? Would it maybe be easier for the interpretation of the results to convert (with kappa-Köhler) all GF-values to 90%? It is not 100% clear to me on what is gained by showing both timeseries of GF at 85% and 90% in Figures 7 and 8.
- Lines 506-509: There are some recent findings that Aitken-mode particles could also be of primary origin (e.g. Xu et al., 2022 or Lawler et al., 2021), it might not be only secondary particle formation.
- Page 23, 3rd paragraph: The authors could also reference and mention the work by Mauritsen et al. (2011) about the tenuous cloud regime in the Arctic and the susceptibility of Arctic clouds to changes in CCN concentration.
- Conclusions: As mentioned above, the results of the trajectory analysis are not really shown. Suggest to remove this part or add the results to the revised manuscript or SI.
- Figure 5 and 6: Are the total particle concentrations measured by a CPC or derived from integrating the SMPS size distributions?
- Data availability: It is great that the authors have already provided their data. I would recommend to also include the RH-data for the HTDMA (e.g. for the dry diameter, ambient, and measured at the inlet). It would also be good to clarify in the read-me if any of the data was corrected to STP (or not).
- SI (page 1): Add "the" before the "CCN counter". Is the shown calibration a composite of all the four performed CCNC calibrations?

Minor comments:

- Line 23: Add “the” before “initial”
- Line 565: take -> taking; remove one of the “only”s
- Line 605: Add “%” behind 0.63
- Line 397: Suggest to remove the “substantial” or clarify what you mean with this.

References:

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