Comment on acp-2021-899
Anonymous Referee #1

Referee comment on "Characterizing the hygroscopicity of growing particles in the Canadian Arctic summer" by Rachel Y.-W. Chang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-899-RC1, 2022

Review: “Characterizing the hygroscopicity of growing particles in the Canadian Arctic summer” by Rachel Y.-W. Chang et al.

This manuscript investigates the CCN activities of artic aerosol, especially the one that originated from NPF and/or with early growth. The measurements were done, during summer 2016, on a vessel, in the Canadian Arctic Archipelago region. CCN is measured using a CCN counter (CCNC Droplet Measurement Technologies, CCN-100). These measurements are part of the NETCARE survey. In this present study, the authors performed a careful study of their data by investigating the potential CCN activities of very specific aerosols. They differentiate two types of aerosols: The one that nucleates and reaches a size below 40 nm, and the one that reaches a size between 40 and 100 nm (both with a Ntot bigger than 500 particles per cc). The main idea behind this study is to investigate the effect of NPF and growth on global CCN activities in the artic. The authors found that although these new particles increase slightly the number of CCN compared to the artic aerosol background, their hygroscopicity is rather low, and conclude that the aerosol growth is mainly due to organic material, probably from the sea. The authors provide updated values for the parameters of the Twomey parameterization (CCN parameterization) for aerosol generated in the Canadian archipelago. The paper complements previous studies performed with the NETCARE survey. The manuscript is well structured, concise, and well written. I, therefore, recommend the manuscript for publication in ACP following appropriate response to the following minor comments.

Minor comments/questions:
P2L44: “whether the particles grow large enough to activate at a SS that is relevant in the ambient atmosphere” what is the range of SS relevant in an artic ambient atmosphere?

P2L54-P3L56: about the possible influence of iodine contributing to the nucleation and growth, there is a new study from He et al. 2021 that could be mentioned maybe like this:

“ There is evidence that iodine (e.g., iodine oxoacids) can also contribute to nucleation, usually on the coast of Greenland (Allan et al., 2015; Dall’Osto et al., 2018; Sipilä et al., 2016; He et al., 2021), as well as more recently in the Central Arctic Ocean (Baccarini et al., 2020).”

Additionally, Baccarini et al, 2020 observed that HIO3 was the main driver of growth to CCN sizes in central Arctic.” However, there is typically insufficient iodine to explain the observed growth in these studies.”. One location out of three-four locations is significant enough to be out of the “typically” word, maybe?

P3-L59-60: There is for example a paper from Sihto et al 2011, looking specifically at CCN activation of these particles secondary organic aerosol from NPF and freshly grown (from the boreal forest). I m not sure how relevant is secondary organic aerosol from the boreal forest for the Canadian Arctic Archipelago comparison. However, the authors found similar results to this present paper.

P4L105-110: I have three comments/questions there.
1) what was the sampling line? the flow is mentioned but not the length (i.e. would it be possible due to a very long sampling line and small sample flow 0.5 L min-1) that small particles are lost on the way to the CCN counter?
2) Most important, what was the sampling temperature? Can the inlet flow slowly warm-up before entering the CCN counter? I m wondering about possible evaporation of the aerosol in the sampling line, especially the organic volatile compounds of the aerosol sampled.
3) at which temperature the CCN counter was operating? How warmer compare to summer ambient Canadian cloud temperature? I always wonder (not only for this specific study), about the possible effect of the warm temperature in the CCN counter on how it can affect the partitioning of the volatile organic compounds of this artic SOA. I understand that it is a standard way to measure CCN properties worldwide and that this data set can be compared with other worldwide data, but maybe this could explain why all these organic aerosols, measured at a warm temperature, show a weak hygroscopicity.

P6-L165: I m wondering about this median activation ratio absolute value. AR is defined as the ratio of the CCN concentration / NTot. While I could understand that CCN measured concentration can be seen as an absolute value (i.e. the smaller particle not reaching the CCN counter would not activate anyway), the Ntot correspond to the number of particles bigger than the detection limit of the specific DMPS (here 10 nm). So I m not sure how someone can use/compare these AR values with previous/future studies. I mean if
instead, the DMPS measured from 15 nm onward for example, and the absolute value of AR would have jumped to 0.6 for example, would the authors have reached a different conclusion?

P8-L167-168, the AR reported from the other studies, are they calculated with similar DMPS cut-off sizes (min and max)? The authors compare AR from Jung et al 2018 study, however, there is no value for Ntot reported in Table 2 for this specific study.

Table 1: I m wondering if in Table 1 an extra value such as Dcrit should be explicitly reported. Dcrit can be calculated back using the reported kappa value (which has been calculated from Dcrit and with the assumption of the chemical composition of the artic aerosol sampled), but it is not as straightforward compared to having it directly reported. Dcrit can be used in certain regional/global modeling.

P9 L196-197: “Based on the median calculated κ values from the entire study, we infer that >80% of the aerosol volume fraction was composed of a non-hygroscopic component, which we interpret as being organic”. I m wondering if the word “non-hygroscopic” is too strong here (and in the manuscript in general). Multiple simulation chambers studies have shown, that SOA hygroscopicity, although not high, has a kappa bigger than 0, which is definitively more than “non-hygroscopic”. For example, Bouzidi et al. 2022 (and reference therein) describe the hygroscopicity of SOA as “slightly hygroscopic” for their less hygroscopic case.

Figure 1: I m not sure if it is possible to color the sea and the land to help to read the map? If possible, this would made the map easier to read.

Typos:

P2 L 23: “ref”. Reference is missing.
References

