

The paper presents the effect that warm air intrusions in the high Arctic have on the optical and macrophysical properties of cirrus clouds, and falls within the scope of the journal. The study is based on high-resolution and high-accuracy airborne HSRL data with a focus on water vapor measurements. The study is of high importance as for the first time such a significant amount of water vapor vertical measurements in cirrus clouds are used to investigate this subject, and it is also the first study to study this topic in respect to supersaturation in and around cirrus clouds. The manuscript is well-structured and well-written, clearly presenting the authors' findings. I would suggest the publication of this work after consideration from the authors of the following comments (mostly minor ones).

Specific comments

Page 1 line 15 and Page 3 line 77: "RH_i is affected by and can be used as an indication of the nucleation process and the structure of cirrus clouds" and "the distribution of RH_i in the resulting cloud is highly dependent on the nucleation process": Rather than the RH_i is being affected by or is dependent on, one would argue that it affects the nucleation processes that will dominate. Suggestion for a (tense) revision.

Page 2, line 53: "vertical speed omega": consider revising to updrafts

Page 5, line 140: While reading this part I was puzzled why the authors used temperature fields in different levels but wvmr in all atmospheric column (not in levels). Later on, in the results section wvmr in levels is also discussed. So I would suggest here to add (maybe even in parenthesis) the information that later on for the results section wvmr means for different altitude ranges are also discussed/used.

Figure 2, Page 9 line 202, Table 3: "Data from ERA-5", "ERA-5 data", and "ERA-5 reanalysis data".. I would suggest that the authors change these parts to ERA-5 reanalysis dataset or ERA-5 fields.

Page 8, line 140: "Here, we calculate three temperature-dependent thresholds.. which is more efficient as an INP needing lower RH_i": It would be great if the authors could mention here the temperature-dependent thresholds used refer to the RH_i values. Also, it would be nice to mention these thresholds so the reader doesn't necessarily search for Urbanek's et al. work before reading the results.

Page 8, line 181: "Further, we define three RH_i regimes" It seems that these regimes are an output of the previously defined thresholds. If this is the case, please revise the connection between the 2 parts of this paragraph to make it more clear (currently it reads as if this is a new threshold).

Figure 4 vs section 2.3.5, 3.2.2 (e.g. Page 9 line 230), and Table 4: It is difficult to understand the connection between the thresholds presented in the plots/legends and the ones mentioned in section 2.3.5 and page 9 line 230. Specifically, in the text 3 regimes are described, the HOM nucleation regime defined with values over 147%, HET CS between 123% and 147%, and HET MD between 100% and 123%. On the contrary in Figure 4 the thresholds are discussed differently, than the ones mentioned in the main text, with the lower values of HET MD being higher than 100% and this propagates somehow in the other thresholds. Please revise to make clear and consistent the different regions in fig. and main text.

Page 11, line 259: "During AC conditions the cloud-free air is more frequently supersaturated but mostly up to the activation threshold for MD, whereas during WAI conditions the cloud-free air is less frequently supersaturated, but reaches values over the MD activation threshold and up to the CS threshold": This is not visible when comparing the © and (f) plots by eye. I

assume this is discussed base on the statistics of table 4, so maybe you could add the a reference on the table for this part.

Page 15 line 346: “The difference in optical thickness ... hints to differences in the microphysical properties of the two cloud types leading to different extinction coefficients”: Did you find different extinction coefficients from the collocated WALES extinction measurements? if so, a comment on this would be interesting to be included.

Technical corrections/suggestions (bold text & red “,”):

Page 3, line 85: “..high ice supersaturations **values in their simulations**”.

Page 4, line 113: “the measurements come in **the** form of... the whole atmospheric column, from the aircraft to the earth’s surface, when conditions are favorable. Thus, with a single overpass of a cloud, information ..”.

Page 5, line 138: “For the temperature field we used ~~the data on~~ the pressure level”.

Page 8, line 179: “and thus **makes** higher RHi values **are** necessary”.

Page 8, line 196: “until **at** the 250 hPa pressure level **where** the arctic becomes”

Page 8, line 140: “Here, we calculate three temperature-dependent **RHi** thresholds”

Page 9, line 225: “WAI cirrus have **an** almost”.

Figure 4: “under arctic conditions (~~Arctic cirrus AC~~), in-cloud (b) ~~Arctic cirrus AC~~ near-cloud (c) ~~Arctic cirrus AC~~ cloud-free (d) Cirrus clouds measured under WAI conditions (WAI ~~cirrus~~), in-cloud (e) WAI ~~cirrus~~ near-cloud (f) WAI ~~cirrus~~ cloud-free.” Because it is confusing reading “cirrus cloud-free” in the legend.