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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contrasting ice formation in Arctic clouds: surface coupled vs decoupled clouds" *by* Hannes J. Griesche et al.

Anonymous Referee #2

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The manuscript by Griesche et al. describes an approach based on the synergy among the measurements provided by different ground based remote sensing sensors techniques (lidar, radar, radiosondes mainly) to study the ice content of surface-coupled and decoupled clouds in the Arctic using the data from a cruise of the Polarstern vessel in the Summer of 2017.

The authors objective is demonstrate or at least provided concrete support to the hypothesis that INPs from marine biological reservoir controls the ice heterogeneous nucleation of the Arctic clouds. This hypothesis is based on previous studies available in literature. More specifically, the authors aim at demonstrating that the dynamical and microphysical structure of surface-coupled cloud differs from the decoupled clouds with the first type being dominant in terms of frequency of occurrence and because more

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Discussion paper

frequently contain ice than the decoupled clouds (by a factor of 5). This hypothesis appears to be demonstrated from the results reported in the manuscript for the considered dataset. The dataset is limited one month and half in 2017: despite the fact it must be acknowledged the considerable effort spent to collect the presented measurements, a dataset with a longer time coverage covering at least two seasons - discussed also in conjunction with a more detailed meteorological analysis - could provide more robust results. The effect of the limited dataset time coverage may have an effect on the discussed results and this should be considered by the authors while it is faintly mentioned at the end of the paper only.

Below, I provide my general and a few specific comments. I recommend a major revision of the manuscript.

The presented approach lack of many details and the applied criteria often rely on assumptions inferred from the literature: these appear a bit forced or not supported by evidence or sensitivity studies which can quantify the related uncertainties. I list here the number of missing details which do not allow to quantify the uncertainty range in the statistical analysis and do not ensure the full reproducibility of the presented approach.

In the identification of ice clouds (section 2.1 Ice-containing cloud analysis), the description of the procedure applied to classify and characterize individual cloud profiles is purely qualitative, the thresholds applied to the value of the depolarization and backscattering coefficient are not mentioned indicating that the profiles have been evaluated on a subjective basis. There is no mention to the uncertainties and assumptions done in the lidar data processing (use of lidar ratios, calibration of profiles, , quantification of effects like specular reflection, etc. . .) which are quite relevant for the presented statistics. Everything could be referred to a literature paper to clarify the data processing, but, as it stands, I am not able to find one reference for these aspect in the entire section, only one for the multiple scattering affecting the depolarization ratio. The authors uses “the cold side of the temperature inversion which is closest to the cloud-radar-derived cloud top height in the radiosonde data to defined the cloud-top temperature." It is not clear to me how large is the difference in meter between cloud top derived from the radar and the height of the radiosonde in correspondence of the cloud-top temperature. May a large difference be the result of a collocation effect which is negligible or not? In section 2.2, a scaling factor for the parameterization of DeMott et al. (2015) is derived from a single paper in literature, Gong et al. (2020), where filter samples from the Cape Verde Atmospheric Observatory were studied and INP active at temperatures above -10°C were found, which consists likely of biological material. This factor is assumed as a sort of "true" to estimate the INP concentration without any study on the sensitivity of the results to this assumption. The considered assumption may lead to large uncertainties in the retrieved INP profiles. The authors should not forget that the lidar retrieval have already uncertainties and is based on assumptions the effect of which might be amplified by this further assumption in the parameterization of mineral dust.

To conclude this point, I think Section 2.1 must be substantively reshaped.

2.From the text, It seems that the authors did not try a quantification of the effect on their approach of the presence of different types of aerosol beneath the clouds. No information are provided about the aerosol types (from data itself or transport model data), assuming the measurement platform necessarily implies the presence of biological aerosol mainly. Previous studies available in literature showed that the types of aerosol observed in the Arctic may be of very different origin depending on the air mass advection from lower latitudes and on the related natural or antropogenic events (dust, biomass, . . .). Volcanic ares with intense activities throughout the year must be also considered as an important potential source. Why did the authors not used at least the lidar data to type the observed aerosols (for example using lidar color ratio and depolarization) or transport models?

3. Likewise It's unclear why the authors did not use the cloud radar measurements in the identification and filtering of cases with ice crystal precipitation. This is another points which can change the statistics collected in too subjective way, to my opinion,

affecting the final results

4. For the results shown in Figure 4, the reported statistics on the number of profiles considered in the statistics poses a questions on the dependence of the results from dataset time coverage: is the number of coupled ice cloud profiles much higher because these are the most recurrent cases for the investigated period of the year? This aspect must be discussed in clear way, maybe using ancillary datasets.

I think also Section 4 must be improved.

Specific comments Line 6 page 1: replace “in “ with “within”

Line 9 page 1: the factor mentioned here in in the range 2-5, but it is not mentioned in which temperature range assumes these values. It becomes clearer from the following sentence. Please rephrase.

Lines 14-16 page 1: this sentence is not appropriate for the abstract but for the discussion section, please remove.

Line 8 page 6: “to date” must be at the end of the sentence.

Line 8 page 2: “yet” date must be at the end of the sentence.

Line 24 page 3: remove higher at the beginning of the line and change “. . . than do. . .” with “higher than”.

Page 7: Figure caption please put “yet” at the end of the sentence of replace “an” with “a”.

Page 7 line 1: it is not clear to me which algorithm has been used to retrieve the cloud top height from the radar measurements, please specify.

Page 7 line 13: the detection of cloud by the radar up to the tropopause maybe depending on the size of ice crystals and by the concurrent atmospheric attenuation. Please nuance this sentence.

Page 7 line 17: please replace “simplified coupling algorithm“ with “simplified version of the algorithm”.

Page 10 line 8, page 11 line 1-2: do you have any reference to support your arguments?

Page 12 line 1: “is the strongest”or “is stronger”?

Page 12 line 11-13: in this part of the manuscript, there is often the comparison with statistics collected in Leipzig; I am wondering if the authors can say a few words on the usage of data from one site only at the mid-latitudes to make the comparison with a more stable region like the Arctic.

Page 14 lines 14-19: in this paragraph, the reader can find the list of the limitation of the results presented in this study. These are highlighted only at the end of the manuscript as an outlook for future studies while they should be discuss also when the results are presented.

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