Dear Editor (Dear Geraint)!

We improved the manuscript and considered (almost) all recommendations of the reviewers. The changes are indicated in bold in the revised version.

We want to mention that we included a sentence (in the ABSTRACT) concerning the contribution of the Raikoke sulfate aerosol to the 532 nm AOT over the Arctic … and added some text to Raikoke in Sect.3.1 as well.

Furthermore, we added a sentence in the Conclusions (at the end) that a super site for remote sensing and in situ observation (balloon, UAVs, aircraft) in the high Arctic for year-around aerosol and cloud observations in the free troposphere up to the lower stratosphere would be desirable…

Our step-by-step answers in blue!

Reviewer #1:

The authors did a very good job responding to my comments and answering my questions. I like the new Table 1.

Thank You!

Fig. 14: As a very minor recommendation for the authors to consider if they wish: they could mention that the paper's INP concentrations of 0.1 to 0.5 L-1 are in line with previous Arctic in situ observations at similar temperatures of -28.5 oC, especially given their uncertainty ranges in Table 1. The observations that I am aware of range from 0.001 to ~2.5 L-1 between -25 and -28.5 oC (Creamean et al., 2019; Hartmann et al., 2020; Mason et al., 2016; Wex et al., 2019).

Creamean, J. M., Cross, J. N., Pickart, R., McRaven, L., Lin, P., Pacini, A., Hanlon, R., Schmale, D. G., Ceniceros, J., Aydell, T., Colombi, N., Bolger, E., and DeMott, P. J.: Ice Nucleating Particles Carried From Below a Phytoplankton Bloom to the Arctic Atmosphere, 46, 8572–8581, https://doi.org/10.1029/2019GL083039, 2019.

Hartmann, M., Adachi, K., Eppers, O., Haas, C., Herber, A., Holzinger, R., Hünerbein, A., Jäkel, E., Jentzsch, C., Pinxteren, M. van, Wex, H., Willmes, S., and Stratmann, F.: Wintertime Airborne Measurements of Ice Nucleating Particles in the High Arctic: A Hint to a Marine, Biogenic Source for Ice Nucleating Particles, 47, e2020GL087770, https://doi.org/10.1029/2020GL087770, 2020.

Mason, R. H., Si, M., Chou, C., Irish, V. E., Dickie, R., Elizondo, P., Wong, R., Brintnell, M., Elsasser, M., Lassar, W. M., Pierce, K. M., Leaitch, W. R., MacDonald, A. M., Platt, A., Toom-Sauntry, D., Sarda-Estève, R., Schiller, C. L., Suski, K. J., Hill, T. C. J., Abbatt, J. P. D., Huffman, J. A., DeMott, P. J., and Bertram, A. K.: Size-resolved measurements of ice-nucleating particles at six locations in North America and one in Europe, 16, 1637–1651, https://doi.org/10.5194/acp-16-1637-2016, 2016.

Wex, H., Huang, L., Zhang, W., Hung, H., Traversi, R., Becagli, S., Sheesley, R. J., Moffett, C. E., Barrett, T. E., Bossi, R., Skov, H., Hünerbein, A., Lubitz, J., Löffler, M., Linke, O., Hartmann, M., Herenz, P., and Stratmann, F.: Annual variability of ice-nucleating particle concentrations at different Arctic locations, 19, 5293–5311, https://doi.org/10.5194/acp-19-5293-2019, 2019.

We forgot this point last time…. So, now this statement is given in Sect. 3.3 (in the text part) and all four references are included in the reference list.

Technical comments:

P6L23: I am not sure a manuscript in preparation should be cited here?

Is replaced now by the Ohneiser et al., ACPD, 2021 version

P3L31: "to what extent"

Improved

P5L15: "above the ground" (or maybe more accurately, above the surface?)

Improved ..above the surface

P14L27: “The goal was to …”

Improved

Reviewer #2

The authors did a great effort to improve the manuscript, and included several new parts and methods. The manuscript has changed a lot.

Thank You!

Please find below few remaining minor comments to the new additions of the manuscript. It would be optimum for the readers if the authors can revise the text based on these comments.

Nomenclature of variables in the figures: I suggest using the same nomenclature in the figures for the extinction coefficient variable throughout the paper. Specifically, Figure 4,10,13 have “Extinction cf” while Figure 15,17,18 “σ”.

We replaced sigma by Extinction cf. in Figs. 15a, 17a, and 18a.

Lidar ratio symbol (page 9, line 34): Is there a reason why for the extinction-to-backscatter ratio the symbol L is chosen in this paper, instead of the symbol S or the abbreviation LR which are used in the literature? Consider to revise or include a small comment on the paper for this selection.

We did not use S for lidar ratio because S stands for super saturation in cloud research papers. It is like T for temperature. We should not violate that. I think we do not need to state that explicitely.

Page 18, line 31: “ Lifting phases of gravity waves can be as long as 20 minutes (1200 s) as our Doppler lidar and radar observations conducted in several field campaigns during the last 10 years indicate”. If available, consider including some references for this.

We found the nice paper of Kalesse and Kollias (2017) on vertical wind statistics over Oklahoma (13 years of ARM radar observations in cirrus), and they support our assumption (600s). They found that gravity waves have typical durations of 19 +/- 7 min over Oklahoma in winter! Gravity waves are different in the tropics and for the rest (midlatitudes and high latitudes). Then, the updraft period of the full gravity wave is about 10 min = 600s. We do not give all these details, we just provide the reference in Sect. 3.4.

By the way, we provide the following statement in the beginning of Sec 3.4 now: According to a study of Barahona et al. (2017), Arctic ice clouds tend to form almost exclusively by heterogeneous ice nucleation with a contribution of only 10% by homogeneous freezing. We give this sentence, because it is so much in line with our study and findings.

Concerning the uncertainties introduced and discussed: As the uncertainties are different for different mixing states, the authors should consider adding in Table 1 a comment with the information on the mixing state these uncertainties are representative of. See specific description of the inconsistencies below.

We mention this point in Sect. 2.2 now. A short statement in the Table would be confusing … because some explanation would be necessary, and that would lead to a large caption text.

Page 7, line 31: In the referenced paper of Haarig et al. 2019, Table 1, the uncertainty of the n_50 is a Factor of 2 and for n_250 is 30%, while in the Table 1 of this work the uncertainty provided for n_50 is 50%, and for n_250 is <=25%. Additionally, from the POLIPHON method one can easily calculate that the type-separated extinction coefficient uncertainties, on a layer where mixtures of different aerosol types prevail, can exceed 30% for the non-dominant aerosol particles. For example, for an aerosol layer with particle depolarization ratio of 10±1% (dust mixtures), using the POLIPHON method with δ_dust = 30±3% και δ_nondust = 5±2% the weight calculated is 0.236±0.052%. For a layer with βp_total=1±0.1Mm^(-1)sr^(-1) (uncertainty defined in this work), the pure dust backscatter uncertainty is propagated as: βp_dust= 0.236±0.057 Mm^(-1)sr^(-1) and for the non dust: βp_nondust= 0.764±0.092 Mm^(-1)sr^(-1). With LR_dust = 45±11 Sr, LR_smoke = 85±21 Sr (<25% uncertainty as defined in this paper): ap_dust= 10.6±3.6 Mm^(-1) => 34% error on the dust extinction coefficient, and ap_smoke = 64.9±17.9 Mm^(-1) => 28% error on the smoke extinction coefficient. POLIPHON calculation from this point for the dust mass concentration gives: M_d = 17.7±6.3 => 36% uncertainty. This is an example of a case where the uncertainties provided in Table 1 are lower than the error propagated uncertainties from the measurements and method. That is why a comment on Table 1 on the mixing state representative for these uncertainties would be very useful for the reader.

The effect of the mixing state on the retrievals is partially mentioned in page 7, line 35 for the CCNC retrievals: “Comparisons with airborne in situ measurements showed that CCNC can be obtained with an uncertainty of about 30% (inversion of multiwavelength data) to 50% (conversion of the 532 nm extinction coefficient) when the aerosol type (and thus the typical aerosol size distribution) is known, and about a factor of 2 if the aerosol type is not well known or mixtures of different aerosol types prevail..”. But not for the rest retrievals (page 7 line 28): “Regarding the aerosol microphysical properties, the comparisons showed that particle number concentrations, surface area, volume and mass concentrations can be obtained with an uncertainty of 25-50% (see Table 1) ..”.

So, we improved this in Sect. 2.2, by adding some sentences on this.