Author’s response to two anonymous reviews for ACP-2020-1096

We thank the two referees for their time and providing us with their comments and ideas, which improved the quality of the manuscript. We have revised the manuscript considered the points raised by the reviewers and provide here a point-to-point answer to the single remarks (the referee comments are highlighted in blue-italic). The page, line and figure numbering refer to the revised manuscript. Additionally a diff-version of the manuscript is provided for tracking the changes.

General remark

We complemented the discussion of the manuscript by a back-trajectory analysis (page 14, line 17-25, Fig. 8). This analysis was performed by Martin Radenz and hence we invited him as co-author of the manuscript.

Specific reply to Referee #1

Major comments:

- The authors have the 35 GHz ground-based radar data at their disposal, one of the best remote-sensing instruments for the detection of precipitating hydrometeors (and ice particles in particular), even in very small concentrations. Yet, they only use the lidar data to detect precipitation. Commonly occurring cases of weak Arctic precipitation can be missed by lidars in such cases, as a result of the potentially minor contribution of very small ice concentrations to an air volume’s total cross-sectional area of scatterers (e.g., when including nearly spherically-shaped aerosols). This is also evident in Fig. 2, where there is a clear indication of (weak) precipitating fall streaks in the radar data between 18-21 UTC (also suggested by the depolarization plot), even though this period is classified as an ice-free cloudy period. I think that the authors should incorporate the radar data in their analysis, because at the moment, weakly precipitating clouds could significantly change their analysis results (see for example Fig. 3 in Buhl et al., 2013; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50792) in the proceeding figures.

Indeed the cloud radar Mira-35 is much more sensitive to ice detection as the lidar. As mentioned in Bühl et al. (2013) the lidar has a detection threshold in IWC of about 10^{-6} kg m^{-3}. But, as shown in Griesche et al. (2020) frequently low clouds were observed during the analyzed campaign with a cloud base below the lowest detection range of the radar (155m above the instrument). In some cases, even the cloud top was below this height. These clouds would have been falsely classified using the cloud radar only, while these clouds are actually those closest to the surface and therefore most likely coupled to it. Using the near-field capabilities of our polarization lidar Polly^{XT} is thus a prerequisite for the presented coupling study. Another point is that we wanted to do a study that is comparable to previous studies, as the one of Kanitz et al. (2011). Nevertheless, we created the same analysis of the ice-containing clouds for surface-coupled and –decoupled cases (see Fig. 1). As to be expected the amount of ice containing clouds increased, yet ice-containing surface-coupled profiles were both absolutely (i.e. the numbers of profiles) and relatively (i.e. the fraction of ice-containing clouds) more frequent. To conclude this point: we think that the challenges we would be facing including the cloud radar for the ice detection would have deformed the statistics by excluding the very near clouds, and would have made a comparison to other studies difficult. And last but not least, the observed effect (more surface-coupled ice-containing cloud profiles with a IWC down to 10^{-6} kg m^{-3}) would not be affected by radar observations. To discuss this and the following points regarding the methodology, we added the Subsection *4.2 Methodology and instrument effects* in the Discussion Section (the usage of the lidar for the ice detection is discussed on page 15, line 1-6). Additionally the results are presented in Appendix A.

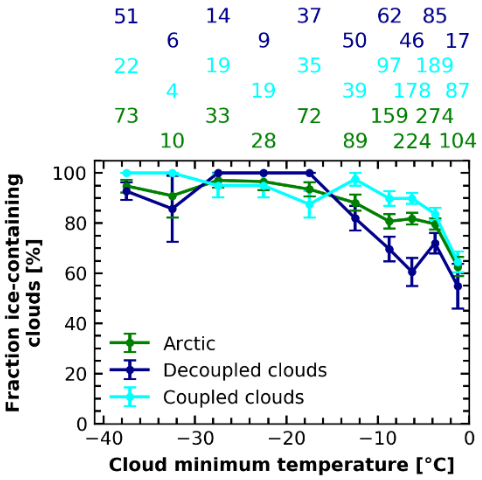

Figure 1: Fraction of ice-containing clouds determined using the cloud radar for ice detection. In green the results of the complete data set is shown, in cyan for the coupled clouds and in dark blue for the decoupled clouds.

- In continuation of the previous comment regarding ice detection using lidar depolarization ratio data, the analysis could have been influenced by specular reflection from plate ice crystals within the -20 – (-8) ◦C temperature range. In these cases where plates precipitate from the cloud base, the determined cloud base might be lower than it actually is (depending on the depolarization threshold), and a cloud can be classified as ice-free since the change in depolarization or the depolarization threshold for ice detection (not clear from the text) is not strong enough. I know that the common tilting of lidars by a few to several degrees off zenith (e.g., 5 degrees as in the PS106 voyage) is commonly believed to address this specular reflection issue, but that is a common misconception, as it does not consider commonly observed higher canting angles of ice particles (see for example Appendix A in Silber et al., 2018; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2017JD027840, Noel et al., 2002; https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2002GL014828).

We agree that the issue of specular reflection is not discussed well enough in the manuscript. However, since this effect is most prominent for rather large dendrites, which form at a temperature of roughly -15°C, we think specular reflection has little influence on our findings where we found the strongest influence at a temperature above -10°C. We extended the Discussion section and elaborated the effect in more detail (see page 15, line 7 and the following).

- I find the theta criterion for the determination of surface coupling state problematic because it doesn't consider the cloud height above the surface, which could result in more lower-level clouds being classified as coupled. As an example, a cloud base at 200 m with theta difference just below 0.5 K (e.g., theta of 260 K at the surface rising linearly to roughly 261 K at cloud base) would be considered coupled even though dtheta/dz = 5 K/km, which is strongly stratified. In their analysis, the authors should take into consideration the height dimension as well as the measurement uncertainty of the RS-41 radiosondes (0.3 C in T, 4% in RH). The current potential for a low-level coupled cloud bias could contribute to the stark coupled vs. decoupled ice occurrence fraction differences at higher temperatures discussed in Fig. 4, given the summertime dataset manifested in the greater occurrence of lower, warmer clouds (see for example the results from Svalbard in Nomokonova et al., 2019; https://acp.copernicus.org/articles/19/4105/2019/).

To test the influence of the theta gradient on the coupling retrieval, we set a gradient-threshold for the coupled state. The threshold was set to the minimum gradient between surface and liquid layer base observed during this study in the case of decoupling, which was dtheta/dz = 2 K/km. In Figure 2, the retrieved results are shown. As expected the number of decoupled profiles increased at the expense of coupled profiles. And with the number of profiles also the frequency of occurrence of surface-decoupled ice-containing profiles increased. Yet, in case of surface coupling the frequency of occurrence of ice clouds increases even stronger. Hence, those profiles, which were classified as coupled with our original retrieval but decoupled when the gradient-threshold is considered, have a higher fraction of liquid-only than ice-containing clouds (increase in frequency of occurrence of surface-coupled ice-containing profiles). Still, the fraction of ice-containing clouds within the reclassified profiles is higher, compared to the originally surface-decoupled classified profiles (hence, also an increase in frequency of occurrence of surface-decoupled ice-containing profiles). In addition (not shown but as predicted by the reviewer) the reclassification only concerns clouds with a liquid cloud base at or below 400m (with the majority at 200m or below). The proximity of those clouds to the surface increases the likelihood of an effect of surface originated aerosols. We conclude that, despite the deficiencies of our original approach to determine the surface coupling state, we stick to this retrieval as this was already used in Gierens et al. (2020). We have discussed these points in the manuscript on page 16, line 6-11 and presented the results in Appendix B.

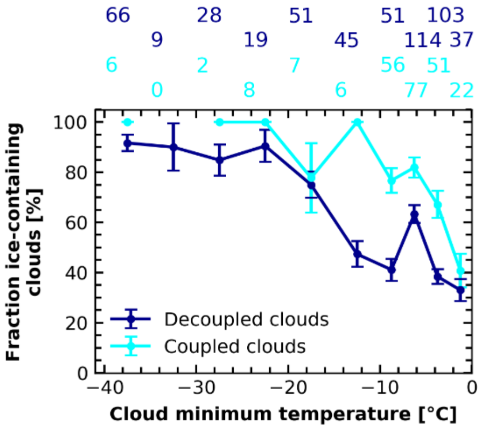

Figure 2: Fraction of ice-containing clouds determined using also a threshold in the potential temperature gradient to identify surface-coupling (cyan surface-coupled clouds and dark blue decoupled clouds).

The measurement uncertainty of the radiosonde can be split into systematic and random errors. Comparing values within one radiosonde profile, a systematic uncertainty is rather negligible, as it is correlated throughout the profile. Random uncertainties on the other hand, can vary in sign and magnitude for each point. The standard deviation between twin soundings up to 100hPa are given by Vaisala for the RS-92 (which was still used during PS106) for zemperature as 0.2°C and for pressure as 0.5 hPa hPa (Jensen et al., 2016). To test how this might influence the coupling retrieval, we used error propagation to determine the uncertainty in the potential temperature profile $\boldsymbol{\Delta\theta}$. The error was used to estimate an upper and lower threshold boundary of the coupling retrieval (0.5K ±$\boldsymbol{\Delta\theta}$) and to repeat the coupling retrieval with these two values. The effect on the fraction of ice-containing clouds for the coupled and decoupled cases was located within the statistical uncertainty of the results (Fig. 3). Hence, we stick to our original analysis, but made the reader of the manuscript aware that the measurement uncertainty of the radiosonde has not been considered (see page 16, line 11-13 and Appendix B)

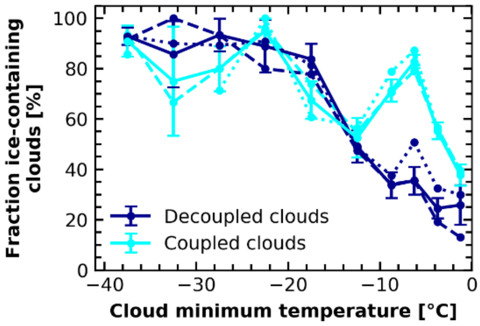

Figure 3: Continuous lines same as Fig. 4 of the manuscript. The dashed lines represent the respective results for the upper end of the error margin (i.e.profiles were classified as decoupled using a threshold for the coupling state of 0.5K +$\boldsymbol{\Delta\theta}$) and the dotted lines represent the lower end of the error margin (i.e. profiles were classified as decoupled using a threshold for the coupling state of 0.5K -$\boldsymbol{\Delta\theta}$).

- Estimation of the INP number concentration: the method in Mamouri and Ansmann (2016) relied on European and Mediterranean data of aerosol mixtures, the values of which can be significantly different from Arctic regions (see for example Kanji et al., 2017, https://journals.ametsoc.org/mono/article/doi/10.1175/AMSMONOGRAPHS-D-16-0006.1/28236). Moreover, in the Arctic alone it has been shown that there is high INP variability and that INP concentrations are not correlated with multiple types of aerosols (e.g., Wex et al., 2019, https://acp.copernicus.org/articles/19/5293/2019/). Given the fact that based on our current knowledge INP occupy only a small fraction of the total aerosol number concentrations (and likely their projected area), there are just too many degrees of freedom in the INPNC retrieval and I do not see how can the authors estimate the INPNC even with the scaling factor they decided to use, and do not see how their conclusions could be dismissed even without the rather short INPNC analysis discussion. I find it very hard to believe that the uncertainties in INPNC (as shown in Figure 6) are smaller than an order of magnitude.

We have decided to remove the Section of the INPC estimation together with Figure of the INPC (Fig. 6b in the old manuscript). The reason for this is the great uncertainty of the presented approach. Instead, we extended the discussion on the measured attenuated backscatter coefficient values, including the lack of parametrizations for INP in the Arctic (see page 14, line 1-16).

Minor comments:

- There is no information on the route of the PS106 voyage. I recommend adding a map for reference or at the very least specify the latitude/longitude ranges of that voyage.
A Figure with the PS106 track and dates is added.

- p. 1 l. 2 - suggest defining that OCEANET is a platform in the first instance.
Done.

- p. 1 l. 11-12 "This provides further evidence : : :" – this sentence is not supported by the analysis and is not explicitly discussed in the text. I recommend removing it or revising the analysis and text accordingly.
We complemented the discussion by a back-trajectory analysis (page 14, line 17-25, Fig. 8). This analysis was performed by Martin Radenz and hence we invited him as co-author of the manuscript.

- p. 1 l. 13 – "acting as seeds for ice multiplication" – again, this impact of seeding from below is not discussed and supported by the text (might be suggested only implicitly in the discussion about blowing snow).
We considered this point more explicitly in the discussion.

- p.1 l. 16-18 - suggest reordering these two sentences.
The Section about the INPC estimation has been removed from the manuscript.

- p. 1 l. 23 - "above the one" - suggest rewording
Done.

- p. 3 l. 34 - "as it is the case for the 35-GHz ..." - Even though the ARM KAZR is a valid example, I recommend either removing this part of the sentence or providing a different example, because the KAZR nor the Barrow site are not discussed in this paper.
We have raised this point using the ARM KAZR because the limitation to detect the most-likely surface-coupled clouds below 150 m height concerns also this instrument. We consider this point as important because it hinders one to use the temporally extensive ARM datasets to be utilized for coupling studies similar to the one presented in here.

- p.3 l.21 - "First studies ..." - I do not understand this sentence - suggest rewording
The paragraph has been reframed.

*- p. 3 l.27-29 - is this *the main feature* of Arctic clouds, or simply one of their common features? Also, do the clouds necessarily form in inversions, or do they form the inversions? I think that both options are plausible (see for example Morrison et al., 2012; https://www.nature.com/articles/ngeo1332, Silber et al., 2020; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL087099, Sedlar, 2014; https://journals.ametsoc.org/view/journals/apme/53/12/jamc-d-14-065.1.xml?tab_body=fulltext-display)*
We reworded the paragraph and also considered nonturbulent formation of Arctic clouds.

- p. 6 l.3 - please clarify whether the linear or circular depolarization ratios are used (I suspect the former).
The volume depolarization was used, which is derived from the linear depolarization ratio measurement of the lidar.

- p. 9 l. 31 - it could be the vast majority of clouds (> 80%) but this is certainly not every cloud.
We reworded that sentence stating that the majority of clouds contains ice.

- What are the depolarization thresholds for the determination of liquid and ice? Are there backscatter thresholds as well? These values should be explicitly specified for reproducibility by potential readers.
Similar to a comparable concern raised by reviewer 2 we point to the difficulty in separating ice and liquid clouds by the lidar volume depolarization. There have not been many studies on a quantification of the lidar volume depolarization on ice detection, as volume depolarization ratio is the superposition of molecular and particulate backscatter in the co- and cross-channels. To tackle this obstacle we used manpower to manually analyze the data set and provided a detailed explanation of the methodology.

Therefore we decided to describe the applied method in detail. We analyzed the complete available data set. The only periods that have been excluded from the analysis, are as described those with favorable seeding conditions (i.e., a cloud above the analyzed cloud within 1km). Combined with the fact that all data is freely available, this study should be reproducible by anybody given the description of the methodology in the article.

Nevertheless we made a first attempt to provide a depolarization threshold on the ice detection, which is shown below. We calculated a minimum volume depolarization where the lidar should be able to detect ice. This is based on an ice water content detection threshold of 10^{-6} kg m^{-3} (Bühl et al., 2013) which was converted into lidar extinction using the approach of Hogan et al. (2006). Using a lidar ratio of 30 sr (typical single-scattering lidar ratio of ice crystals, see, e.g., Seifert et al., 2007) we calculated the particle backscatter coefficient. The molecular backscatter coefficient at 532-nm (wavelength of the used depolarization channels) was derived using scattering theory (Hinkley, 1976) for a temperature of -10°C and air pressure of 925 hPa (ca. 700 m height). Assuming a particle depolarization ratio of ice crystals of 0.5, a minimum volume depolarization of 0.03 was found corresponding to the ice detection threshold of 10^{-6} kg m^{-3}.

This threshold was used to reproduce our study with an automatic approach. We defined a volume depolarization signal above 0.03 in four contiguous lidar range gates, but below the liquid-cloud base, as an ice layer. An ice-containing cloud profile was defined, when during half of the profile time an ice layer with volume depolarization ratio > 0.03 was detected. The results are presented in Fig. 4. While we found minor quantitative differences between the manually analyzed data set below -15°C, the main message of this manuscript remains: The occurrence of ice-containing surface-coupled cloud profiles at temperatures above -10°C is much higher compared to surface-decoupled profiles.

We consider an implementation of the automatic ice detection algorithm introduced above as a promising approach for future studies. For the sake of compatibility to our previous studies (Kanitz et al., 2011) we however suggest to follow the original approach presented in the manuscript.

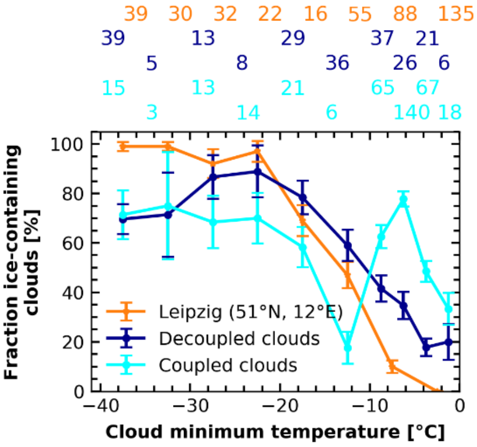

Figure 4: Fraction of ice-containing clouds determined using a volume depolarization threshold of 0.03. In dark blue the results for surface-decoupled clouds are shown and in cyan those for surface-coupled clouds. In orange results for Leipzig, Germany from Kanitz et al. (2011) are presented. The numbers above the plots represent the respective profile behind each data point.

- p. 7 l. 1 - what is the slope or the metric with which cloud top is defined? This should also be specified.
The cloud top has been derived by the highest cloud radar range gate, which was classified as cloud. The minimum detection threshold of the cloud radar was 5 times the signal to noise ratio in the co-channel. This information has been added to the manuscript at page 7, line 14-17.

- p. 7 l. 5 - "coldest temperature" - temperatures can lower but not colder – suggest rewording here and in other locations in the text.
Reworded.

- p. 7 l. 6-9 - This method of using the inversion base temperature as cloud top temperature may explain some of this study's results, as the assumption becomes less valid in cases where clouds protrude into temperature inversions, which often occur concurrently with stronger mixing, not necessarily down to the surface. I think that in the context of this paper the authors might be able to make their point by defining their current "cloud top temperature" as "minimum cloud temperature", which would also be

valid for cloud protruding into an inversion, and would retain the essence of INP activation temperature widely discussed in the text. Also, note that note all polar liquid-bearing clouds are capped by a temperature inversion. See for example Sedlar and Tjernström, 2009; https://link.springer.com/article/10.1007/s10546-009-9407-1, Sedlar et al., 2012; https://journals.ametsoc.org/view/journals/clim/25/7/jclid-11-00186.1.xml?tab_body=fulltext-display, Sotiropoulou et al., 2014; https://acp.copernicus.org/articles/14/12573/2014/, Silber et al., 2020; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL087099

We changed our wording from "cloud-top temperature" to "minimum cloud temperature". Additionally, as suggested by reviewer we have stated more clearly how the temperature was obtained (minimum temperature between liquid layer base and cloud top, see page 8, line 18-19).

- p. 7 l. 12-14 - based on the fact that the authors have used the radar for this seeding cloud proximity criterion, I think that they refer here to overlying hydrometeors rather than overlying clouds. If that is correct I recommend revising the text accordingly.

That is correct. We changed the text accordingly.

- p. 9 l. 31 - it could be the vast majority of clouds (> 80%) contain ice but this is certainly not every cloud as currently stated in the text.

We reworded that sentence stating that the majority of clouds contain ice.

- Fig. 3 caption confusion - deltaT should be 5 C below -10 C and vice versa.

Corrected.

- p. 11 l. 13-14 - "The reasons for the increase in ice forming efficiency for low and coupled clouds in the Arctic must be caused..." - while this is likely the case, I think that the authors should tone down this sentence.

We reworded that sentence.

- p. 12 l.10-11 - decoupling does not necessarily mean that there is an underlying inversion, but only that the underlying layer is stable. I suggest revising the text accordingly.

We reworded that sentence.

- p. 12 l. 11-13 - clouds largely act to destabilize the polar atmosphere and not the opposite. Another more likely possibility is that once the marine aerosols are mixed aloft, the atmosphere becomes decoupled as a result of radiative cooling of the surrounding ice surfaces.

We decided to make our discussion more general by stating that we do not have information about the evolution of the cloud before reaching the observation site.

- p. 12 l. 16 - add "as" before "such"

Done.

- p. 12 l. 23 - define beta

Done.

- p. 12 l.13 - a reduction in beta is generally seen throughout the atmospheric profile regardless of the decoupling height (and sometimes increases above the decoupling height such as in the green and blue curves), so I find this argument by the authors to be rather subjective.

The INPs abundance discussion was comprehensively reworded and made more clear. The observed increase of beta above the decoupling height is due to a cloud. This information has been added to the manuscript (page 14, line 5-6).

- Fig. 6 - what do the normalized 0 and 2 values represent?

0 is the surface and 2 is 2 times the decoupling height. The axes labels have been improved.

- p. 13 l.1 - Temperature units are missing.

Paragraph has been deleted. See answer to major remark of INPC estimation.

Specific reply to Referee #2

Major comments:

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.

The reviewer rises the legitimate concern about the limitation of the dataset. Indeed we would like to analyze a longer time series, but this is what we had available for our study. We incorporated this point into the discussion of the manuscript (page 14, line 26-29).

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.

We did not provide quantitative thresholds about how we separated ice and liquid clouds because of the difficulty in doing so. There have not been many studies on a quantification of the lidar volume depolarization on ice detection, as volume depolarization ratio is the superposition of molecular and particulate backscatter in the co- and cross-channels. To tackle this obstacle we used manpower to manually analyze the data set.

Therefore, we decided to describe the applied method in detail. We analyzed the complete available data set. The only periods that have been excluded from the analysis, are as described those with favorable seeding conditions (i.e., a cloud above the analyzed cloud within 1km). Combined with the fact that all data is freely available, this study should be reproducible by anybody given the description of the methodology in the article.

Nevertheless, we made a first attempt to provide a depolarization threshold on the ice detection, which is shown below. We calculated a minimum volume depolarization where the lidar should be able to detect ice. This is based on an ice water content detection threshold of 10^{-6} kg m^{-3} (Bühl et al., 2013) which was converted into lidar extinction using the approach of Hogan et al. (2006). Using a lidar ratio of 30 sr (typical single-scattering lidar ratio of ice crystals, see, e.g., Seifert et al., 2007) we calculated the particle backscatter coefficient. The molecular backscatter coefficient at 532-nm (wavelength of the used depolarization channels) was derived using scattering theory (Hinkley, 1976) for a temperature of -10°C and air pressure of 925 hPa (ca. 700 m height). Assuming a particle depolarization ratio of ice crystals of 0.5, a minimum volume depolarization of 0.03 was found corresponding to the ice detection threshold of 10^{-6} kg m^{-3}.

This threshold was used to reproduce our study with an automatic approach. We defined a volume depolarization signal above 0.03 in four contiguous lidar range gates, but below the liquid-cloud base, as an ice layer. An ice-containing cloud profile was defined, when during half of the profile time an ice layer with volume depolarization ratio > 0.03 was detected. The results are presented in Fig. 1. While we found minor quantitative differences between the manually analyzed data set below -15°C, the main message of this manuscript remains: The occurrence of ice-containing surface-coupled cloud profiles at temperatures above -10°C is much higher compared to surface-decoupled profiles.

We consider an implementation of the automatic ice detection algorithm introduced above as a promising approach for future studies. For the sake of compatibility to our previous studies (Kanitz et al., 2011) we however suggest to follow the original approach presented in the manuscript.

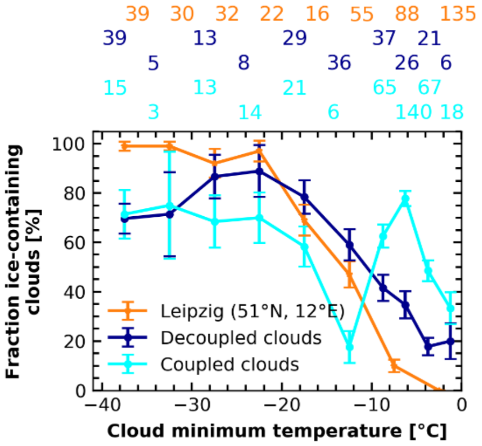

Figure 1: Fraction of ice-containing clouds determined using a volume depolarization threshold of 0.03. In dark blue the results for surface-decoupled clouds are shown and in cyan those for surface-coupled clouds. In orange results for Leipzig, Germany from Kanitz et al. (2011) are presented. The numbers above the plots represent the respective profile behind each data point.

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.

Similar to a comparable concern raised by reviewer 1 we agree that the issue of specular reflection is not discussed well enough in the manuscript. However, since this effect is most prominent for rather large dendrites, who form at a temperature of roughly -15°C, we think specular reflection has little influence on our findings where we found the strongest influence at a temperature above -10°C. We extended the Discussion section and elaborated the effect (see page 15, line 7 and the following**).**

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?

As suggested by reviewer1 we changed the terminology from "cloud-top temperature" to "minimum cloud temperature". To derive the minimum cloud temperature we searched for the lowest temperature between liquid layer base and cloud top. This information has been added to the manuscript on page 8, line 18-19.

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.*

A comparable concern was raised by reviewer 1. We decided to remove the Section on the retrieval of the INPC together with the Figure of the INPC (Fig. 6b in the old manuscript). The low data basis makes sophisticated parametrization impossible. We tried to provide an estimate of a possible INP load but have to admit that the uncertainty of the presented approach is too large. Instead, we extended the discussion on the measured attenuated backscatter coefficient values, including the lack of parametrizations for INP in the Arctic (see page 14, line 1-16).

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.

As also pointed out by reviewer 1 the cloud radar Mira-35 is much more sensitive to ice detection as the lidar (the lidar has a detection threshold in IWC of about 10^{-6} kg m^{-3}, see Bühl et al. (2013)). Similar to our answer to reviewer 1, we like to point to the frequently observed low clouds during PS106 (see Griesche et al. (2020)) with a cloud base below the lowest detection range of the radar (155m above the instrument). In some cases, even the cloud top was below this height. These clouds would have been falsely classified using the cloud radar only, while these clouds are actually those closest to the surface and therefore most-likely coupled to it. Using the near-field capabilities of our polarization lidar Polly^{XT} is thus a prerequisite for the presented coupling study. Another point is that we wanted to do a study that is comparable to previous studies, as the one of Kanitz et al (2011). Nevertheless, we created the same analysis of the ice-containing clouds for surface-coupled and –decoupled cases (see Fig. 2). As to be expected the amount of ice containing clouds increased, yet ice-containing surface-coupled profiles were both absolutely (i.e. the numbers of profiles) and relatively (i.e. the fraction of ice-containing clouds) more frequent. To conclude this point: we think that the challenges we would be facing including the cloud radar for the ice detection would have deformed the statistics by excluding the very near clouds, and would have made a comparison to other studies difficult. And last but not least, the observed effect (more surface-coupled ice-containing cloud profiles with a IWC down to 10^{-6} kg m^{-3}) would not be affected by radar observations. We discussed this issue on page 15, line 1-6. Additionally the results are presented in Appendix A.

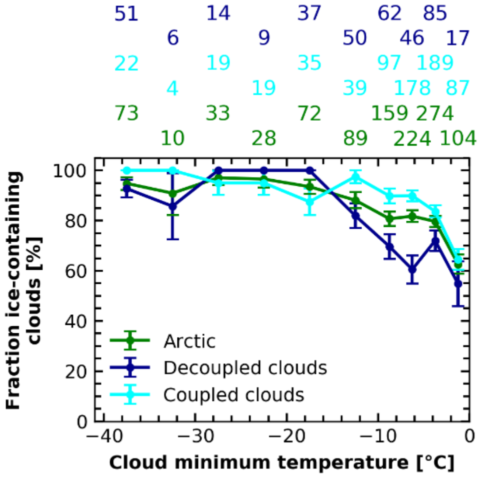

Figure 2: Fraction of ice-containing clouds determined using the cloud radar for ice detection. In green the results of the complete data set is shown, in cyan for the coupled clouds and in dark blue for the decoupled clouds.

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.

Previous studies have shown that the occurrence of low level and thus likely surface-coupled clouds in the Arctic have been highest in the northern hemisphere summer. Nevertheless these are also the clouds which are one of the greatest challenges for models (Morrison et al., 2012, https://www.nature.com/articles/ngeo1332). Hence, we strongly support the request to use longer time series, to study this observed effect in more detail.

Specific comments

Line 6 page 1: replace "in " with "within"
Done.

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.
We specified the temperature range.

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

Line 8 page 6: "to date" must be at the end of the sentence.
Done.

Line 8 page 2: "yet" date must be at the end of the sentence.
Done.

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

Page 7: Figure caption please put "yet" at the end of the sentence of replace "an" with "a".
'An' replaced by '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.
The cloud top has been derived by the highest cloud radar range gate, which was classified as cloud. The minimum detection threshold of the cloud radar was 5 times the signal to noise ratio in the co-channel. This information has been added to the manuscript (page 7, line 15-17).

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.
The sentence has been reworded.

Page 7 line 17: please replace "simplified coupling algorithm" with "simplified version of the algorithm".
Done.

Page 10 line 8, page 11 line 1-2: do you have any reference to support your arguments?
Instead of adding a reference for this statement, we have weakened our statement.

Page 12 line 1: "is the strongest"or "is stronger"?
"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.
The reason for the comparison with the mid-latitude site of Leipzig is justified by the fact that we believe the free-tropospheric aerosol in the Arctic is dominated by continental long-range sources as is also the case for the free troposphere over Leipzig. This possibility has been discussed in more detail in the manuscript (page 14, line 17-25).

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.
The discussion has been completed by the respective points.

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