Responses to Review #1

The authors would like to thank the reviewer for his valuable comments which helped improving the quality of the manuscript. Our point-by-point responses to the reviewer's comments appear in bold below.

R1.1

Page 7, line 3 I think you are referring to eq. 6 not 7

The sentence has been fixed.

R1.2

Page 9, line 16 to 18: if μi = 0 why is veff =1/3 (and not 1)?

There was an error in the equation on line 18. It has been corrected:

$$\nu_{eff} = \frac{1}{\mu_i + 3} = \frac{1}{3}$$

R1.3

Page 9, calculation of LR via Mie code: please state that this is only a (rough?) approximation as the ice crystals are not spherical.

This is indeed a rough approximation for ice crystals, which are generally far from spherical. Thanks to lidar observations, this non-sphericity is characterized by the depolarization rate, which is generally over 30%, often 50%. In our case, we have values of the order of 10%. This suggests that the crystals are approaching a spherical shape. This is the assumption we have made. We have supplemented the text where we introduce the use of a Mie code:

"In order to assess the vertical profiles of ice crystal effective radius (r_{eff}) and ice water content (IWC), we use a Mie code assuming... **Ice crystals are generally not spherical. Nevertheless, for the clouds sampled in this study, the ICDR is ~10% (Sect. 4.3), far from the values associated with highly non-spherical crystals whose ICDR is between 30 and 50%...**"

R1.4

Page 11, l16 "ice cloud formation highly probable" Please consider rewording. To my knowledge, in INP sparse regions supercooled liquid clouds dominate even at much lower temperatures. However, a recent paper that describes ice formation at temperatures slightly below 0C might be this

https://www.pnas.org/doi/pdf/10.1073/pnas.2021387118

Thank you for your comment. We have added the reference and clarified our comments accordingly:

“With the advection of moist air masses over the site, the formation of ice clouds is therefore highly probable above 0.8 km a.m.s.l. This value is within the range of the bottom height of boundary layer mixed phase clouds in the western Arctic (McFarquhar et al., 2007; Maillard et al., 2021). **At temperatures close to those encountered here, i.e. slightly below 0°C, Luke et al. (2021) have recently shown that secondary ice formation can significantly increase the amount of ice crystals in clouds in the Arctic.”**

R1.5

Fig 3: I agree that we see a highly complex pattern. Hence, is this really a stratus cloud? The cloud at about 2km altitude from UT 18 to 23 probably yes.

This is a good remark also made by reviewer 2. We have made corrections where necessary. There are probably Ci composed of ice crystals that precipitate. This could lead to glaciation of the lower liquid layers, as suggested by reviewer 2.

R1.6

Fig 3: Your VDR has a tendency to show high values above 6km, regardless whether a cloud is obvious in ABR or not. This may be a thin / subvisual cirrus. But what about a typical insecurity of VDR and ABR in about 6km altitude for the night 16 to 17 May?

Indeed, there are certainly thin cirrus clouds above 6 km and this is in line with comment R1.5. ABR is a fairly raw data, associated with a relative error of less than 5% at the altitudes considered for the lidar used and the optical thicknesses encountered. VDR is a ratio of channels, so the relative error is higher. It is only used as an indicator. The absolute error is around 1% (Chazette et al., 2012). It is highly dependent on the optical thickness of the atmosphere. We have added an error calculation on the retrieved optical properties.

R1.7

Page 11, line 25: variability … due to wind shear. In the boundary layer I understand this point. What about the free troposphere? Maybe you could describe your measurement site a bit. E.g. are you surrounded by mountains? What was the synoptic situation / main wind direction etc.

… Ah, I see that you describe this in section 4.2. Still you may add a short description of your site and show this prior to the lidar results?

It was indeed described in sub-section 4.2. We have followed the reviewer’s advice and moved this subsection back to the beginning of Section 4. The wind directions are already given in Fig. 2. We have added a description of the site at the beginning of Subsection 4.2 (now 4.1):

“The lidar measurements were obtained near Hammerfest airport on the island of Kvaløya, which lies in a south-west/north-east trough at an altitude of ~90 m a.m.s.l. The site is bordered by relief peaking at around 360 m a.m.s.l. in the north-west and reaching up to 1045 m a.m.s.l. in the south-east. These reliefs can therefore significantly influence flows over the site by generating wind shears.

R1.8

Page 15 line 30.: relatively low depolarization values for Arctic cirrus have recently been found by Nakoudi. They speculate on a latitude dependence of depolarization. However, as your clouds are low and warm this may not be 1:1 comparable. Still I am not too surprised on your findings.

https://www.mdpi.com/2072-4292/13/22/4555

We have added this reference, which compares well with our results.

R1.9

Table 1: eq 13 is the “backscatter weighted LR”. Eq 12 is a constant LR. If the LR according to eq. 13 is larger than for eq 12 this means (to me, maybe I am wrong) that the thicker parts of the clouds (high backscatter) have a higher LR and a large concentration (high beta) of smaller particles (your Fig 1.) I am wondering whether this does make sense. What do you think?

Yes, if we look only at equation 13. In fact, when we hang a constant LR, constrained by optical thickness, we should find the same thing. Equation 13 uses the N2 Raman channel, which doesn't reach as far as the elastic scattering channel (altitude ranges in brackets in the table). The fact that equation 13 gives a higher LR is therefore linked to a non-negligible contribution from the top of the cloud, which may be associated with lower LRs. However, we must be cautious with the uncertainties on LRs.

R1.10

Fig 5: can you please state briefly how this has been calculated? This is the solution of the Raman channel for alpha, I assume. And beta was taken from Klett? The lidar rations in Fig 5 are larger than in Table 1. Why?

The LRs in Fig. 5 are obtained by calculating the backscattering coefficient via the coupling between the N2 Raman channel and the elastic channel. The extinction coefficient is derived from the N2 Raman channel. The LR in the table is an average value of the LR profile weighted by the extinction profile (Equation 13, now equation 10). High extinction coefficients therefore mostly drive the average LR. This explains the apparent differences noted by the reviewer.

R1.11

Page 19, lines 6-10: While everything (local pollution, Norwegian gas flaring, pollution from Russia and Canadian forest fires can occur), they will probably not manifest in the same night of observations. I would skip the speculation on the forest fires or Russian pollution unless you have a clear hint that you have seen it in the lidar data. Instead, you see an ice cloud at warm conditions. Maybe the growth rate is simply slower?

Just before the cloud period, we noted a significant change in the nature of aerosols in the atmospheric column, with fire aerosols aloft. There are therefore several aerosol natures

passing over the site, as shown in the reference quoted in the article (Chazette et al., 2018). Clouds encompass these different layers.