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Interactive comment on "Widespread persistent polar stratospheric ice clouds in the Arctic" *by* Christiane Voigt et al.

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Reply to Anonymous Referee #3

The authors would like to thank the reviewer for the comments, which enhanced the quality of the discussion of the lidar data. Below we reply (R) to the comments (C) of referee #3.

Referee #3:

The paper by Voigt et al. describes the occurrence of polar stratospheric ice clouds during the Arctic winter 2015/16 by means of CALIPSO and airborne lidar observations.

The overall situation of this Arctic winter is described by use of CALIPSO observations and ECMWF model fields. Later in the paper one case study from an airborne lidar is presented and used to derive a new threshold for the classification of ice PSCs.

GENERAL COMMENTS

C: - Is the paper supposed to be an overview paper of the PSC situation of the winter 2015/16 or is it to introduce the new threshold? The new threshold needs further justification before it can be considered in future studies. What is the influence of instrumental differences between CALIOP and WALES on the classification? . . ...

R: The general comment is related to the scope of the paper, to the threshold of the backscatter ratio for NAT/STS and ice and to the discussion of the instrumental differences of the two lidar instruments. We will discuss the justification of the threshold and instrumental differences explicitly and in detail in the specific comments section further below.

Regarding the scope of the paper, here we do not want to show an overview of the PSC situation of the Arctic winter 2015/2016. This will be subject to a more detailed follow up study using CALIPSO data, which will also expand on the thresholds for PSC classifications.

In our manuscript, we want to show the unprecedented and persistent ice PSC occurrence in the Arctic winter 2015/16, which is very uncommon for the Arctic and we suggest possible formation pathways based on optical properties derived from lidar histograms and trajectory analysis. The observations are important, as ice PSCs enhance Arctic ozone loss, and thus the winter 2015/16 can be used to test ozone chemistry in current chemistry transport models.

SPECIFIC COMMENTS

C: - The duration and the maximum extension of ice PSC (and NAT + STS mixtures) are shown in Figure 1 and discussed in Section 3. From the text it is not clear what threshold was used for the classification of ice PCS. From the layout of the paper I assume it is still the old one. If so, the calculations need to be repeated with the new threshold. Currently the areal comparison to ECMWF is quite good. I expect that the area derived from CALIPSO observations will be larger with the new threshold. How can you explain the difference? How does the duration, extension and area compare to previous years when the new threshold is used? Again, further statistical analysis of the long CALIPSO time series in needed to justify changes in the choice of threshold values.

R: We rephrase the abstract to more explicitly express our findings.

New text: “Increasing the threshold of the inverse backscatter ratio from 0.2 to 0.3 for ice PSCs as derived from high-resolution lidar measurements at 532 nm wavelength onboard HALO better explains the ice PSC occurrence at temperatures below the frost point on 22 January 2016.”

R: We justify the use of the threshold of the 1/Rice threshold ice versus NAT/Mix2 by comparing different thresholds, 1/Rice = var compared to 1/Rice = 0.3, 1/Rice = 0.2 and 1/ Rice = var as in the CALIPSO classification used here.

New text: “Further we compare different thresholds 1/Rice of ice versus NAT/Mix2. The ice PSC areas for the 1/Rice threshold of 0.3 agree best with the area with temperatures below Tice. This is evident in particular between 22 and 24 km. In addition, in the histogram plot in Fig. 3, the occurrence of the STS-ice branch is unphysically split by a threshold of 0.2 compared to 1/Rice = 0.3, the standard value of Pitts et al. (2009). Further, we present a classification with the threshold used in the CALIPSO lidar classification in Fig.1 for comparison. This threshold adopts a dynamic, altitude dependent variation of the boundary based on MLS measurements of HNO3 and H2O. For this case, the areas classified as ice are again lower than the areas with temperatures below Tice in particular between 22 and 24 km. For our case, the relative difference of ice area for 1/Rice = var compared to 1/Rice = 0.3 is 8.2% and for 1/Rice = 0.2 compared

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to 1/Rice = 0.3 case amounts to 21.8%."

C: - Page 6, line 143: Is the linear particle depolarization ratio from the CALIPSO measurements derived the same way? How reliable is the CALIPSO depolarization value?

R: CALIOP linear particle depolarization ratio is calculated using a consistent approach with that of WALES. The quality of the depolarization is dependent on the signal strength - with the most accurate data for large signals, i.e. ice. But, the quality of the CALIOP particle depolarization has no bearing on the data provided for this study. It is not used explicitly in the analyses - only perpendicular backscatter and R are used.

C: Are the values and accuracy of the linear particle depolarization from WALES comparable to CALIPSO?

R: Especially at low backscatter ratios R the CALIOP particle depolarization data is of course much noisier which smears out the fine structure of the histogram. In the ice regime where the estimates of particle depolarization are most accurate, the depolarizations from CALIOP and WALES are comparable.

C: - Page 7, line 160: The new threshold seems to be unjustified. The only justification given is the form of the histogram. Why did you pick 0.3 and not 0.4 or 0.25? More discussion is needed.

R: We now added a figure (new Fig. 4) to the manuscript comparing the different thresholds. We discuss the results in the text, see comment above.

C: - Page 7, line 162: The form of the histogram is quite different to previous published observation from the CALIPSO PSC team. How do you explain the difference? Is it just the case for the shown case study? What does it look like for the other WALES observations?

R: As mentioned above, the particle depolarization data of CALIOP is much nosier than the WALES measurements and hence cannot reveal all the fine structure. But in general the form of the joint distribution is not far from what is expected from the microphysical simulations presented e.g. in Pitts et. (2013). There are no further coincident measurements of WALES and CALIOP. Thus different meteorological situations may cause differences in PSC histograms in other studies.

C: - Page 7, line 167: The comparison of the PSC classification and temperature generally agree. But one case is not enough to convince the reader of anything! It could be just coincidence. More observations/better statistics are needed to proof the quality of the comparison. Why do you not use the other observation of ice PSCs from WALES measurements?

R: The PSC classification is derived from microphysical/optical calculations and applied to CALIPSO data. WALES uses the classification from Pitts et al., 2013, with a modified ice versus NAT/Mix2 threshold. We think that in general the classification is appropriate and consistent with the temperature thresholds for NAT and ice. In addition, we now show the effects of different thresholds. And the WALES measurements consist of about 1000 statistically independent profiles, so the good agreement between ECMWF temperature and the region attributed to ice from the measurement is extremely unlikely to be just statistical coincidence. The flight on 22 January 2016 was planned as a CALIPSO match. A detailed description and intercomparison of the cloud products from CALIPSO and WALES of this flight is planned for publication elsewhere.

C: - Page 7, line 170: What is the lidar ratio of NAT? How do you know the NAT lidar ratio? Please provide references. I am only aware of the paper by Reichardt et al. (2004) who state that the lidar ratios of solid and liquid PSC particles are similar.

R: Sorry, here it was erroneously written lidar ratio, but color ratio was meant. We changed that in the text and explain the colour ratio.

New text: "At the bottom of the PSC, a layer of sedimenting NAT particles was observed, with larger sizes as derived from the combination of a high depolarization ratio with a quite low color ratio (ratio of the backscatter coefficient for the 532 nm channel and the 1064 nm channel) of 1.5."

C: - Page 8, line 188: Your typical NAT trajectory was never well below T-ice. Does NAT not form well below T-ice (3 to 4K below Tice)? It can however exist at warmer temperatures (up to T-nat). Was the NAT formed in the last 12 h within the ice layer or before -240 hours?

R: NAT formed heterogeneously, probably on meteoritic dust, at temperatures below TNAT and above Tice, as explained in the manuscript. The nucleation rates for this scenario are given e.g. in Voigt et al., ACP, 2005, Grooß et al., ACP, 2005, Hoyle et al., ACP, 2013.

C: - Page 9, line 201 and page 10, second paragraph: How was the NAT formed if the temperature was never below T-ice?

R: See reply above.

C: - Page 9, line 202: Can you please provide the reader with CALIPSO observations? I am sure there are observations along the back trajectories that coincide in time. These observations can be used to support your discussion. Further, it would be good to use a box model along the back trajectories to understand the formation of ice and NAT particles better. One example on how to use back trajectories to investigate the formation and alteration of a PSC observed by CALIPSO and a second lidar can be found in Achtert et al. (2011).

R: Thanks for the suggestion. The comparison of the WALES and CALIPSO match and the climatology of the PSC distribution in the Arctic winter 2015/16 is planned for publication elsewhere. This study could also include model simulations.

C: - Figure 4A: Please add markers for every 12h (or 24h).

R: We now added markers every 48 h to the trajectories, a higher resolution (24 or 12 h) makes the plot too busy. Thanks.

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