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Interactive comment

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

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"Widespread persistent polar stratospheric ice clouds in the Arctic" by Voigt et al., ACPD, 2016 doi:10.5194/acp-2016-1082

Reply to Anonymous Referee #1

The authors would like to thank the reviewer for the comprehensive and detailed comments, which helped to improve the mansucript. Below we reply (R) to the comments (C) of referee #1.

Referee #1:

. . . The paper however remains worthy of publication, as it presents an unprecedented case study of large-scale ice PSCs in the Arctic, made during the unusually cold 2015-

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2016 north hemisphere winter. I recommend publication in ACP, once the following points have been clarified.

MAIN COMMENTS

C:. . .To sum up, I think the authors should try to discuss (in the main text or as an appendix) why they think the CALIOP classification algorithm can be safely applied to the airborne lidar observations used in the case study. This could be done either from comparing observations of the same PSC from both instruments (the easiest way) or theoretical considerations.

R: The CALIOP classification scheme as such is based mainly on microphysical/optical simulations (e.g. section 3.1 in Pitts et al. 2009) and not specific to the CALIOP instrument. Of course the results as expressed e.g. in PSC-area depend on the spatial resolution and signal to noise ratio and may (and for sure will in this case) be different for the space borne and the airborne instrument. But a direct comparison between CALIOP and WALES is not within the focus of the paper. This will be presented in a separate study elsewhere.

Concerning the change of the scattering ratio boundary from 0.2 to 0.3: This boundary is chosen in such a way that the ice/STS mixing lines which are apparent in the microphysical simulations (Pitts et al. 2009 Fig. 6 or Pitts et al. 2013 Fig. 10) and the measurements (see Fig. 3) fall into the ice class. The slope of these lines depends on the available HNO3 and H2O as outlined in Pitts et al. 2013 and should be adjusted to the atmospheric situation. For the airborne measurements the value of 1/R_ice = 0.3 has been chosen based on the histogram in Fig. 3.

We now include a more detailed description of the CALIPSO PSC product in Section 3 and of the WALES lidar product in Section 4. In addition we add a new Figure, which compares the different NAT/Mix2 – ice thresholds and discusses the results (Section 5).

C: - My second concern is with the ice formation scenarios described in Sect. 7. The authors consider heterogeneous formation, either on NAT crystals or on STS containing meteoric dust. Both mechanisms appear rather unusual, and there is no clear evidence for either. Did the authors try to apply a PSC microphysical formation model on the air mass histories provided by the backtrajectories, and see if the observed particle types are reproduced? Maybe they have reasons to believe the more usual formation mechanisms found in existing microphysical models are not applicable in the context of such an usual winter. If this is the case, they should explain why.

R: We describe PSC formation schemes in the introduction. Early PSC formation schemes (e.g. Peter, Ann. Rev. Phys. Chem, 1997) suggest homogenous ice formation in liquid supercooled ternary solution aerosol STS as major ice formation pathway. Murphy et al. (Science, 1998), Cziczo et al., (Science, 2001) measured the existence of meteoric dust in the stratosphere. Curtius at al. (ACP, 2005) and Weigel et al. (ACP, 2014) showed that meteoric material is included in a major fraction (70-80 %) of the polar stratospheric aerosol.

Heterogeneous ice PSC formation is mentioned by Toon et al. (JGR, 1998). Regional microphysical modeling by Engel et al. (ACP, 2013) shows that the formation of the synoptic ice PSC observed on 18 January 2010 cannot be explained by homogeneous ice formation at Tice - 3 K solely, but requires a solid heterogeneous ice nucleus such as meteoric material included in STS or NAT and in addition temperature fluctuations. Here, we suggest that the observed ice PSC results from the effects of two distinctive ice nucleation pathways: ice nucleation on meteoric inclusions in STS and on enhanced NAT, both pathways are clearly distinguished by the two branches in the lidar backscatter versus depolarization ratio histograms.

It is the scope of the paper to show the observation of a synoptic scale and persistent Arctic ice PSC, which is very uncommon for the Arctic and to discuss potential formation pathways. More detailed microphysical modelling of the PSC occurrence throughout the Arctic winter 2015/16 will be subject to another study.

SPECIFIC POINTS

1. The paper's title suggests the paper will show evidence for large-scale and persistent ice PSCs. Instead, the paper presents only a single case study of widespread ice PSCs, and no evidence for their persistence (such as a continuous observation throughout a month). The paper presents evidence of persistent conditions (i.e. cold temperatures) that can support the persistence of ice PSCs, but that is not the same thing. CALIOP observations show relatively large areas of ice PSC compared to other years, but these are aggregated over (undocumented) rather large time periods, so it is unclear if the PSCs are actually persistent. The absence of maps derived from CALIOP also makes it hard to appreciate their actual geographic spread. The title should be modified to be more representative of the article's content, maybe by explicitly mentioning that it focuses on a case study.

R: The paper shows evidence for large-scale and persistent ice PSCs. The massive extension of the ice PSC is demonstrated by the WALES lidar observation on HALO (Fig. 2) and the persistence of ice PSCs is documented by ECMWF areas with temperatures below TICE and CALIPSO climatology of ice areas of the Arctic winter 2015/16 in Fig. 1. The CALIPSO PSC area estimates are produced on a 5-km horizontal x 180-m vertical grid along the orbit tracks. Synoptic ice PSCs were observed by CALIPSO continuously from late December 2015 to late January 2016 and extend over the altitude range of 16 to 26 km - combined with ECMWF temperatures continuously below TICE, there is no reason for ice sublimation. We further want to express that a detailed climatology of the PSC occurrence in winter 2015/16 is not within the scope of this paper and a second manuscript is in preparation by the co-authors. Here we focus on the synoptic ice PSC and its formation mechanisms. We now give more information on the CALIPSO cloud products in the manuscript in Section 3.

New text: "Level 1 CALIOP data in the stratosphere are obtained at 1.0 to 1.67 km horizontal resolution and 60-180 m vertical resolution (depending on altitude). The CALIPSO PSC area estimates are produced on a 5-km horizontal $\times$ 180-m vertical grid boxes along the orbit tracks. The data are aggregated on daily time scales and smoothed over 7-days to eliminate noise. For the PSC classification, the algorithm by Pitts et al. (2013) has been further developed to take into account the impact of denitrification/dehydration on the optical signature of ice and NAT mixture clouds. We now derive a dynamic boundary in the scattering ratio-depolarization optical space based on theoretical optical calculations over the possible range of nitric acid and water abundances. The boundary is then defined as a function of altitude and time based on the observed abundance of nitric acid and water as estimated from nearly coincident Aura MLS measurements."

2. Abstract, l.20: "unprecedented ice PSCs formed" - such ice PSCs might have existed before we started observing them. Please specify "unprecedented in the record" or something like that.

R: We explain the meaning of unprecedented in the Introduction.

3. L23-24: As explained in my main comment #1, there is a non-zero chance that the change of inverse scattering ratio is merely an adaptation of the classification scheme to another instrument, and I see no reason why it should be described as an improvement to the classification method. I also see no reason to mention it in the abstract as one of the paper's result. As far as I can see, the updated classification scheme has only been applied to the case study classification shown in Fig. 2C. It has not been used to update existing climatologies, the article describes no plan to do so, and it is doubtful it would be a good idea to backport this threshold change to the CALIPSO PSC datasets. Since this change has not led to enhanced ice PSC coverage compared to previous analyses, the abstract should not say so. I would suggest to remove this sentence entirely from the abstract.

R: We rephrase this sentence in the abstract to account for the case study. "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." In addition a study is under preparation describing the modifications of the threshold using CALIPSO data and intercomparisons to WALES.

4. L.24: "backscatter ratio -1" if this is meant to describe the inverse scattering ratio, please fix

R: Both nomenclatures, backscatter ratio or scattering ratio are used in literature. As the lidar signal at the laser wavelength is dominated by the light, which is scattered by particles in backward direction, we use here the nomenclature backscatter ratio. We now define the backscatter ratio (or scattering ratio) in the manuscript.

5. L.29: "ice PSCs are a sensitive marker for cold stratospheric winter temperatures" in my impression, given our observation and modelling abilities, cold stratospheric temperatures are currently more easily diagnosed than ice PSCs. So it's rather the other way around. The article provides other, more convincing reasons to observe ice PSCs – for instance that they are responsible for a major fraction of polar ozone loss. I don't understand the need for this rationale.

R: We describe the effect of ice PSCs on ozone loss in the paper. In addition, we add it more dominantly in the abstract. "Persistent synoptic-scale Arctic ice PSCs have not been observed so far. These ice PSCs enhance Arctic ozone loss and in addition are a sensitive marker for cold stratospheric winter temperatures" We think that this is worth mentioning.

6. L.50: "depolarizations" -> "depolarization"

R: Done

7. L.53: "backscatter ratios above 5" – please define how this ratio is calculated. If this is the ratio between the attenuated total backscatter coefficient and the molecular backscatter coefficient, note the Pitts et al. article refer to it as the "lidar scattering ratio", noted R. Using the same convention would be helpful.

R: The backscatter ratio here is the ratio of the un-attenuated (extinction corrected) total (un-polarized) backscatter coefficient and the molecular. The extinction correction is done by the high spectral resolution HSRL channel of the airborne lidar using a molecular reference profile calculated from pressure and temperature data from ECMWF analyses (6 h resolution) and short term forecasts (1h steps) to interpolate between the analyses. The CALIOP data is also corrected for particle extinction, assuming composition-dependent lidar ratios. In Pitts et al. 2009 R is defined using the un-attenuated quantities (Eq. 1), the attenuated ones have a prime, although back then only molecular extinction and ozone absorption were corrected, but not the particle attenuation as for the current data set. So the convention used is the same.

8. L.66-74: I think this paragraph would be more appropriate if moved to the relevant section (Sect. 7).

R: We describe the current theories of ice formation in the introduction and suggest the “novel” pathway of ice formation of the ice PSC observed on 22 January in Section 7. We further put our ice nucleation pathway into context with previous suggestions in Section 7. We think that that this approach is appropriate and prefer to explain the basic theory in the introduction.

9. L.73: "the synoptic-scale ice PSC observed on 18 January 2010" observed by who? How? Why is this case relevant? More anecdotally, I understood from the rest of the paper under review that it presented the first observation of a synoptic-scale ice PSC in the Arctic. Is this not the case?

R: One previous record (Engel et al., ACP, 2013) report the existence of a synoptic Arctic ice PSC for one day (18 January 2010), as mentioned in the manuscript. We moved the reference to the synoptic ice PSC observation (Engel et al., ACP, 2013) to the appropriate place.

10. L.74: The paper has a rather unusual organization, with 9 sections and no subsection. While I am all for variations in paper structure, I think adding the usual paragraph

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at the end of Sect. 1 that 1) sums up the objective the authors have set for themselves, and 2) describes the upcoming narrative, would help the reader find her bearings.

R: We want to keep the manuscript is concise and short. We think that this additional paragraph would lead to a repetition of the abstract without giving new content that is why we want to keep the text as is.

11. L.80: please state that the mentioned ECMWF data is not shown here.

Done

12. L.86: phi has not been defined, please state "latitude" instead

Done

13. L.86: "continuously dropped" – "drop" implies an active change towards colder temperatures, please state e.g. "continuously remained below Tice"

Done

14. L.98: What does "IFS" mean? Are there reasons not to use only the IFS data and completely forego the lower-resolution ECMWF data? Please explain.

R: Integrated forecast system. We added this in the text. The ECMWF data at higher resolution (e.g. $0.25^{\circ} \times 0.25^{\circ}$) are not available for the 36 year time frame of the ERA record.

15. L.98-99: "we also show the operational IFS analysis at $0.25^{\circ} \times 0.25^{\circ}$ resolution. The higher resolution IFS analysis shows" -> "It shows:"

Done

16. Fig. 1C and 1E are not discussed, please remove them from Fig. 1.

R: We now discuss Fig.1 C and E in the text.

17. Fig. 1D: CALIOP data are aggregated on what time scales? Please specify.

R: We aggregate the CALIOP data on daily time scales and then run a 7-day smoothing on this to eliminate some of the noise.

18. L. 103: (Fig. 1) -> (Fig. 1B)

Done

19. L. 104: "at 50hPa." Please add "(not shown)"

Done

20. L. 110: Why does the CALIOP data record stop before the end of January 2016? Please explain in the text.

R: The CALIOP science data acquisition was suspended on 28 January 2016 due to a spacecraft anomaly. The problem was subsequently resolved and data acquisition began again on 14 March 2016, we added this information in the manuscript.

21. L117: "a decrease in water vapor" can you provide evidence for this, using e.g. MLS water vapor measurements or stratospheric CTMs?

R: MLS water vapor shows dehydration throughout January/February 2016, we added this information in the manuscript.

22. L.119: please provide a reference for the 8 years of CALIPSO data record (e.g. Pitts et al., 2013).

Done

23. L.140: Did you use the HSR capabilities of the WALES airborne lidar (i.e. molecular/ particular backscatter separation) at all for your analysis? If this is the case, please keep in mind that R532 is usually based on attenuated backscatter coefficient. Using the extinction-corrected backscatter coefficient provided by the HSR channels would have consequences for the classification scheme.

R: In previous CALIOP PSC algorithms, we corrected the backscatter for molecular and ozone attenuation, but not 'self-attenuation' by the clouds. In our new algorithm, we actually retrieve un-attenuated backscatter and calculate the un-attenuated R532 in this case. We think that this has only small impact on our classification. For the airborne WALES data the extinction correction from the HSR channel is used. The maximum optical thickness of this cloud is 0.15, resulting in a two-way transmission of 0.74. Even if not corrected, the transmission effect is not that large and a rough correction with an assumed lidar ratio makes it negligible.

24. L.147: "on all flights inside the vortex": How many flights is that? How was the case study selected?

R: The case study was selected as it was the flight with the largest ice PSC occurrence measured by the WALES lidar. WALES operation started after the repair of the instrument on 22 January 2015. We then had six consecutive flights on 22, 25, 28 January and 2, 26 and 29 February 2016 with PSC occurrence detected by the WALES lidar.

25. L.150: "an extraordinary high data resolution": please quantify this resolution and CALIOP's.

R: The Level 1 CALIOP data in the stratosphere are obtained at 1.0-1.67 km horizontal resolution and 60-180 m vertical resolution (depending on altitude). We have averaged the data to a single 5-km horizontal x 180-m vertical grid for production of our PSC product. The WALES raw data resolution is 15 m vertical and 40 m horizontal. For the analysis present here the data is smoothed to an effective resolution of 4 km horizontal and 30 m vertical. We removed "extraordinary" from the text.

26. L.158-163: The classification change compared to Pitts et al. 2011 is rather weakly justified. The P11 thresholds are based on a very large dataset of PSC observation, changing it to fit a single case study should be better justified.

R: We now justify the use of this threshold (0.3) as the area with ice PSCs better agrees with the area with temperatures below Tice when using this threshold (see new Fig 4).

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 the 0.3 value, we use. We explain the reasoning and present the additional figure in the manuscript. In addition, we present a classification with the threshold used in the CALIPSO lidar classification in Fig. 1 for comparison.

27. L.159: "1/R" – R has not been defined at this point. In the various Pitts et al. papers, R meant the scattering ratio at 532nm, of total-to-molecular backscatter coefficients. Is this what was used here? Please specify. Where does the molecular backscatter coefficient come from here? How does it compare to the CALIOP source? See main comment #1

R: We replied in comment #1. In addition, for CALIPSO, the molecular backscatter coefficient uses density observations from MERRA2.

28. L.167: "confirming the validity of the new classification" Please quantify the agreement between lidar points classified as ice and temperatures from ECMWF below Tice (false positive/missed detections). Could you describe how changing the ice threshold to 0.4 or reverting to 0.2 affect this agreement? Lidar measurements appear very high-resolution as stated before, while ECMWF data is 1x1°, does this affect the comparison in any way?

R: The ECMWF data used for this plot has a resolution of about 9 km x 9 km which is about 10 times higher than stated by the referee and not so far from the resolution of the lidar data. It is not expected that the ECMWF H2O in the stratosphere is accurate enough to compare small scale structures – and the overall agreement is no so bad. When comparing to different 1/R_ice boundaries the 0.3 case has a better match than the 0.2 and the variable threshold case. Further, the relative difference of ice area for 1/R_ice = var compared to 1/R_ice = 0.3 case is 8.2%. The relative difference of ice area for 1/R_ice = 0.2 compared to 1/R_ice = 0.3 case amounts to 21.8%. See also new Figure 4.

29. L.170: "sedimenting NAT particles" – do you have any visibility on the existence of tropospheric clouds below 14km?

R: The whole space between the aircraft flying between 12 km and 14.2 km and the first point in the data set (14 km) was free of clouds or aerosol layers above the background level.

30. L.172: "meteoric inclusions" this is speculative at this point, please remove

R: We discuss the observations of meteoric material in STS in the introduction and in the reply to comment 3.

31. Fig. 4: looking at the map of the Arctic, I can't help but think about the fact that CALIOP only samples the Arctic south of 82°N. Can you offer insights into how this limitation could influence the results, for instance the comparisons shown in Fig. 1D?

R: It is true that CALIOP has a blind spot between 82°-90°N. For the area estimates (see Fig. 1D), we assume that the data at the highest latitudes (∼82°N) are also representative for the blind spot and extrapolate to fill in this gap. This is not very sophisticated method, and a temperature proxy or some other method could be developed to fill in this gap, but this hasn't been a high priority up to now.

32. L.185 and onward: here you assume that PSC particles are transported along the backtrajectory once formed, correct? Is this assumption usual? Too bad there is no attempt to extract CALIOP observations where the CALIPSO orbit intersect the backtrajectories. Showing consecutive PSC maps derived from CALIOP observations (aggregated over a few days, as in Pitts et al., 2011) could also have provided very useful context to the interpretation of backtrajectories.

R: We assume that the PSC particles are transported along the trajectories and we give sedimentation speeds for the particles. We also tried to interpolate the trajectories on CALIPSO tracks, however the coverage is poor, therefore we refrain from a more detailed analysis. We think that this approach can be used in the climatological PSC description of the winter 2015/16 to be published elsewhere.

33. L.201: "the observations." -> "the observations (Fig. 4C)". Please consider switching Fig 4B and 4C if you agree with this change (first the 10-days, then the zoomed version)

R: We prefer to keep B and C and show first the 36 hours prior to the observations and then the 10 days trajectories.

34. L.202: "CALIPSO observations consistently indicate the presence of NAT clouds from Dec 2015 to Jan 2016" Fig. 1E could be changed to NAT-only and referenced here. Otherwise please specify (not shown).

R: The STS area is smaller than the NAT area, and the NAT area includes the STS area as NAT /STS mixtures could be present under non-equilibrium conditions. We now reference Fig. 1E here, thanks.

35. L.208-210: The ECMWF reanalyses might lack the temporal and spatial resolution required to represent temperature perturbations induced by Greenland's orography. Do you have reasons to believe this is not the case here? Have you tried interpolating the backtrajectories on the higher-resolution IFS temperature field, to see if it does not reveal larger temperature fluctuations that would allow for homogeneous ice nucleation?
R: For the trajectory calculations, we use the ECMWF at the higher $0.25^\circ \times 0.25^\circ$ resolution, as stated in the text. We discuss the effect of resolution on trajectories in the manuscript. The impact of orography is found in the temperature histories of the trajectories. For homogeneous nucleation, we expect ice with significantly higher concentrations (more to the upper right end of Fig.3), which was not observed. Hence we think that the resolution is sufficient.

36. L212: I would appreciate if you could explain why meteoric material is required for the heterogeneous nucleation of ice to happen on STS droplets in this particular case?

R: Meteoric material has been observed in 70 to 80% of polar stratospheric aerosol.

Solid surfaces reduce the nucleation threshold and hence change nucleation rates. Engel et al., (ACP, 2013) show the effects of different nucleation schemes on PSC properties for the winter 2009/10.

37. L222: Could you run a PSC microphysical model on the backtrajectories? This could offer insights on which formation mechanisms can be triggered given the temperature/ concentration conditions. It would help make this section feel less speculative. The proposed formation scenarios are possible, but feel rather circumstantial.

R: Unfortunately we do not have a microphysical PSC model. Thus, our observations motivate a second study on the PSC formation scenarios in the extreme Arctic winter 2015/16.

38. L.227: "we find here" -> "we propose here"

Done

39. Sections 8 and 9: both these sections attempt to put the previous results in a broader context, but succeed only in a very superficial way. Sect. 8, especially, brings little to the article as it weakly ties up the little information provided by Fig. 1D with some points about the role played by ice PSCs in denitrification. I would suggest to merge both sections under a single heading, e.g. "conclusions". Could you also summarize in this new section the new insights provided by the case study (sections 3 to 7)?

R: We merged Sections 8 and 9 into Section 8. We extended the discussion and set our observation into context with MLS satellite observations by Manney and Lawrence (ACP, 2016). We think that the insights from the WALES observations are shown in Section 7 and need no repetition.

40. Section 9: As already commented in the abstract, I find the authors' proposal to use ice PSCs as a proxy for cold stratospheric temperatures unconvincing.

R: We replied to comment 5.

41. Fig. 2A: If I understand correctly, this figure shows R, i.e. the inverse scattering ratio. The figure title "Backscatter Ratio" is confusing, as it brings to mind the backscatter coefficient (the usual lidar measurement, in km-1.sr-1). Please change the figure title and mention R explicitly to avoid confusion.

R: R is not the inverse scattering ratio, but the scattering ratio, also known as ‘backscatter ratio’ in the pertinent lidar literature since the early 80’s. We now define R in the manuscript.

42. Fig. 2A: Is there a reason why the color bar is upside down? (same question for figure 2B and figure 3)

R: We think this is not a major concern and would like to keep it as is.

43. Fig. 2B: Depolarization ratios here are in percents, while in Fig. 3 they go from 0 to 0.6. Please use consistent units.

R: Depolarizations ratios are unit less and % is not a unit, but a scaling factor, so we see no in-consistency.

44. Fig. 2: What are the little down-pointing triangles indicating on the latitude and longitude axes near the bottom?

R: These are turn around points of the aircraft, between two triangles the lat/lon-scales linearly run in one direction – increasing or decreasing.

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