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**Interactive
comment**

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

Anonymous Referee #1

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In this article, the authors first document the evolution of stratospheric temperature (from reanalyses) and concurrent ice PSC area observed by CALIOP in the Arctic between December 2015 and February 2016. After pointing out the unusual and synchronized prominence of extremely cold temperatures and ice PSCs, they present a synoptic-scale ice PSC case study observed from airborne lidar. They classify different regions of the clouds as ice, NAT or STS. They initiate backtrajectories within these regions and propose formation scenarios for these various compositions.

The paper is short but efficient. The introduction is very convincing and well-referenced. The observations and approaches are relevant. The paper is written clearly, although it sometimes feels like two different papers mashed together (an article about the PSC Arctic 2015/2016 winter season in sections 1, 2, 8 and 9, with a case study in the middle). The figures are informative and concise. The methodology

appears sound (with one exception – see first major point below) and easy to follow. On a general level, I was disappointed to find the article spends only very little time describing the PSC distribution over the entire Arctic during such an unusual winter season. CALIOP observations, especially, are underused: no maps, little statistics – I can only hope it means a more complete paper is in preparation. Instead, most of the paper (sections 3 to 7) is devoted to a discussion of particle formation pathways during a single case study. In the end, only little new information has been learned regarding PSCs. I do not see this as a major problem, but as such, the paper fails to deliver on its title's promises. 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 2015-2016 north hemisphere winter. I recommend publication in ACP, once the following points have been clarified.

MAJOR POINTS

I have two main concerns with the paper. The first is that the authors use a PSC classification scheme designed for the CALIOP lidar, that went through several revisions in many years, and apply it directly to measurements from a different lidar system. Lidar signal processing and schemes are generally very instrument-dependent, and transposing a classification algorithm between instruments is generally not trivial. I appreciate that the authors' interpretation of the joint histogram in Figure 3 suggests rather strongly that the classification scheme can be applied like the authors do, although it looks very different to the one obtained by analyzing several years of CALIPSO data in Pitts et al. 2013 (a point I would like to see the authors address). The authors should at least try to justify why the technical differences between the instruments do not affect the results of the classification. For instance, the classification relies on the inverse scattering ratio $1/R532$. $R532$ is affected by extinction and multiple scattering. These phenomena will affect satellite and aircraft lidar measurements very differently (given the change in distance to the instrument, the different fields of view, SNR, detectors, etc.), which has consequences for the classification scheme. Computation

C2

of R532 also requires knowledge of the molecular backscatter profile, which is not directly observed by elastic lidars and derived from reanalyses in CALIOP PSC products. It is unclear where the molecular backscatter profile comes from in the present paper. Given the weak backscatter levels of PSCs, even small differences in its definition could have important consequences for R532. The authors' decision to change the ice PSC R532 threshold from 0.2 to 0.3 could very well be the consequence of instrumental or calibration differences between CALIOP and the WALES lidar, but the authors do not approach it that way. 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.

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.

Other than that, I would be glad if the authors could clarify the points below.

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.

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.

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.

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

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.

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

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.

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

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?

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 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.

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

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

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

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.

15. L.98-99: "we also show the operational IFS analysis at $0.25° \times 0.25°$ resolution (...). The higher resolution IFS analysis shows..." -> "It shows..."

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

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

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

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

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

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

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

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.

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

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

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.

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

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 1°x1°, does this affect the comparison in any way?

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

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

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?

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.

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)

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).

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?

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?

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.

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

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)?

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.

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.

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

figure 2B and figure 3)

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

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