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Interactive comment on "Widespread polar stratospheric ice clouds in the 2015/2016 Arctic winter – Implications for ice nucleation" *by* Christiane Voigt et al.

M. Fromm (Referee)

mike.fromm@nrl.navy.mil

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Review of Voigt et al., "Widespread polar stratospheric ice clouds in the 20Fwales15/2016 Arctic winter – Implications for ice nucleation" Reviewer: Mike Fromm

Note: Throughout this review, the author team is referred to as "auth."

This manuscript is a report on satellite- and aircraft-based observations of polar stratospheric clouds (PSCs) in the Arctic winter season of 2015/16, a season during which an apparently unparalleled duration of Type I and II PSCs occurred. Auth use CALIPSO data, which recorded the Arctic conditions continuously from the start of PSC activity through late January. They also exploit WALES aircraft lidar data on a single day (22 January 2016) to look in depth at a synoptic-scale PSC and make inferences about ice-PSC nucleation mechanisms. Given that there are still uncertainties in PSC formation mechanisms, that this particular PSC season was extraordinary, and that there is an abundance of PSC data, this paper is relevant and appropriate for ACP. The team of authors is deeply experienced in all aspects of PSC observations and processes. The data sets they analyze, especially the aircraft lidar data, are suitably abundant and detailed for auth's purposes. The overview of the PSC season, based on the CALIPSO data, is a valuable addition to the published record on the Arctic 2015/16 polar season.

Despite the above-mentioned strengths of this work, my assessment is that the manuscript needs substantial revision in order to be accepted for publication in ACP. My assessment is that auth's main thrustâĂŤthe section on "implications for ice nucleation"âĂŤis unconvincing as presented. Auth base the entire analysis on a single aircraft-based lidar sampling of a synoptic-scale PSC whose lifetime spanned a multi-day period. From that limited PSC sample they launch and analyze trajectories from four locations within the ice-portion of the PSC. These four trajectory histories alone do not reveal a set of conditions that enable them to draw robust conclusions, in my assessment. Given that CALIPSO data are abundant for other parts of this PSC at the time of the aircraft data and at many other points in the cloud's lifetime, it would seem to be incumbent on auth to expand this analysis with CALIPSO data and more trajectories in order to establish convincing results regarding ice nucleation. Otherwise it is incumbent on auth to explain why CALIPSO data are not used, considering their usage of CALIPSO for characterizing seasonal ice PSCs. Major concerns are listed first, then substantial concerns, followed by minor concerns. To complete the view record I include auth's pdf manuscript. Herein auth will find comment bubbles identifying technical/grammatical issues. The manuscript also has comment bubbles for the more substantial concerns, most of which are discussed in the report below.

Major Concerns

The seasonal overview of CALIPSO-based PSC observations and temperature, embodied in Figures 1 and 2, is an important record of the unusually cloudy 2015/16 Arctic season. The results are wholly consistent with many other papers on the subject of Arctic PSC/temperature relations. This analysis is informative in establishing the Arctic PSC evolution in the cold 2015/16 season. Their illustration of the long periods of synoptic-scale PSCs of all types led me to expect auth to use CALIPSO to address their more substantial science question regarding ice nucleation. My expectation derives from the fact that CALIPSO data have been established and extensively use for PSC composition, two principal CALIPSO PSC scientists are co-authors, and that auth's analysis of the season reveals many opportunities to explore ice-PSC nucleation. Hence it would important to know why CALIPSO data are a foundation for this paper but not used to explore the main question auth raise.

Auth demonstrate the overwhelming coverage of the 2015/16 PSC landscape with CALIPSO data but do not use CALIPSO data for the in-depth case study of 22 January 2016. Why is this so? Although the space-based lidar data are acknowledged to be less sensitive vis á vis aircraft lidar, they clearly have been used to introduce the 2D histogram space (established in prior publications by the CALIPSO co-authors) that auth adopt for the WALES lidar analysis a single PSC. Presumably the relative weakness of space-lidar sensitivity is more than compensated for by the many orbits of CALIPSO near 22 January (and all the other dates of CALIPSO coverage in this winter). It is almost self evident that the 22 January HALO flight was executed as a CALIPSO comparison exercise. The daytime orbit of CALIPSO on 22 January that passes between northern Scandinavia and northern Greenland is parallel to and extremely close in space and time to the HALO flight track–especially the northbound leg. This is relevant for a number of reasons, including that auth acknowledge the sensitivity difference between WALES and CALIPSO but do not exploit the opportunity to show the reader exactly how the two instruments resolve the 22 January PSC. Moreover–because the

PSC is synoptic in scale, and much of it is at very high latitudes–CALIPSO has at least half dozen orbits on 22 January on which it made observations of that PSC. Either auth need to address why CALIPSO is not exploited in this case, or their case study needs to be redone. This is critical, since this is the only analysis herein that auth use to present findings of “implications for ice nucleation” according to the manuscript title.

I found the 22 January case study to be unconvincing and confusing, as pointed out below. (1) Auth predicate their analysis on the familiar 1/R vs. depolarization ratio 2D histogram space (Figure 4) for polarization-lidar-derived optical properties of aerosols and hydrometeors popularized by co-authors Mike Pitts and Lamont Poole. Based only on selected, perceived patterns in Figure 4, auth refer to “branches” between non-ice and ice regimes in this 2D space as suggestive of ice-nucleation pathways. While this may be true, it has not been established here or in prior literature that this 2D construct is to be interpreted in this manner. It is perhaps equally likely that the particular patterns (auth’s “branches” as well as other, unnamed definable features) of Figure 4 are just an artifact of static sampling of a broad cloud. (2) Auth state, but do not show, where the points from which they launch back trajectories show up in Figure 4. (3) In the final section of the paper auth discuss NAT nucleation on ice as well as ice nucleation on NAT. If both of these pathways indeed exist, the NAT-to-ice “branch” they identify could represent both directions along that pathway. Yet their assumptions appear to be that the Fig. 4 branches imply only transformations to ice from other compositions. (4) The trajectory analysis, which is rightly presented to build on the 2D histogram space’s patterns, is inconclusive in my view. First, auth selectively convolve trajectory height/T variations with orographic effects. The PSC/meteorology of this case is demonstrably forced by synoptic-scale dynamics. Moreover, the HYSPLIT trajectory model is driven by synoptic-scale dynamics. Any selective attribution of a trajectory’s change in altitude to Greenland orography is not supported by the data they show. Secondly, auth do not present any information, such as a plotted line in Fig. 7, of Tsts. For much of the temperature excursions of the various trajectories, the parcels lie within a temperature regime that supports both NAT and STS. Hence, during these portions of the

Lagrangian history, no conclusion can be drawn other than both NAT and STS are possible. Consequently auth cannot definitively interpret the trajectory as a NAT-to-ice or STS-to-ice transition. Thirdly, the map in Fig. 7 is extremely difficult to examine because of clutter and the fact that there is an inconsistency between the ice-symbol color scheme on the map and the curtain in Fig. 5.

Given that auth employ back trajectories to determine ice-nucleation transitions, they have not availed themselves of at least two very critical papers employing trajectories and satellite data in similar quests. One is Teitelbaum et al. (2003) DOI: 10.1029/2000JD000065, who showed a recurrent link between synoptic-scale Arctic ice PSCs and synoptic-scale dynamics, including a back trajectory analysis. Teitelbaum et al. show that T/height excursions leading to PSC observations are explainable without having to invoke orographic influence (such as Greenland). Second, Santee et al. (2002) DOI: 10.1029/2000JD000227, connect Arctic synoptic-scale PSC observations to satellite-based gaseous and additional PSC observations along trajectories to study PSC composition. In the case of 2015/16, auth could tap a wealth of PSC and gas data with which to make more sense of PSC optical properties and associated trajectories. Given the premise of this paper (the study of “widespread PSCs”), the overwhelming amount of CALIPSO data, and relevant prior works, the very limited analysis here (only 4 ice-PSC data points in a single cloud) is a weakness that can be eliminated naturally by invoking satellite data and a larger set of trajectories.

Abstract and Conclusion section: The "tropical" aspect is not developed at all in this paper. It is only mentioned in summary, speculative fashion at the very end of the "Conclusion" section. Given the critical concerns mentioned above, I contend that the link with tropical cloud nucleation does not belong in this paper.

Section 5.2: Auth predicate much of their paper on the two-dimensional histogram of lidar-based PSC optical properties developed/refined by CALIPSO scientists and co-authors Mike Pitts and Lamont Poole. The works of Pitts et al. that laid the foundation for composition regions were based on modeling and a subjective, statisticsbased choice of composition boundaries. To my understanding, there was no aspect of phase-transition built into this construct. The histogram is exclusively a view of static patterns of optical properties that reveals types and mixtures, not phase-transition pathways. Auth interpret patterns in the histogram space that they call “branches” crossing boundaries. My understanding is that the histogram construct is insufficient for gleaning such transitions, especially if it is based simply on the histogram. However, at the point in the paper at which the 2D histogram phase space is introduced, auth name two “branches” across phase boundaries. Moreover, they experiment with altering one of the established phase boundaries to bring the branches into greater accord with auth’s assumed phase-transition pathways. This appears to me to be an unjustifiable stretch. It is also my assessment that even the follow-on trajectory analysis, which might allow one to add meaning to the histogram, is inconclusive (as stated above). Hence I consider the argumentation made in 5.2 to be unsupported.

Section 5.3: Auth experiment with Pitts et al.’s criteria for a NAT Mix2/Ice boundary. On what basis is this determination made? It’s not apparent that any vertical line through a sloping feature such as this can be considered better than any other. Auth need to make a better argument as to what looks optimal about their choice of 1/R=0.3, that slices through a sloping feature extending from 0.6 (blues) to $\sim$0.1 (reds). Auth close this section with a statement about how one of their variants on the foundational 1/R=0.2 threshold might be advantageous for application to the 2015/16 season overall. This seems to be unsupportable, based on the concern mentioned here.

Substantial Concerns

Section 5.2: Auth give a brief discussion of 1064/532 nm color ratio. If the color ratio data are to be discussed, and speculation made regarding sedimentation, a figure is called for. Moreover, a vertical variation of color ratio within a static, single cloud sample does not provide enough information to infer a process such as sedimentation any more than other explanations (such as varying particle mixtures, microphysics, etc.) The color ratio analysis should be presented in more exacting detail, or dropped.

Section 6.1: It's not clear how these sensitivity tests are relevant to the process of sedimentation. Auth mention several sensitivity runs but don't show any results. Given the importance auth ascribe to these tests, they merit a figure or table to help the reader. Please suitably flesh out this discussion, or drop it.

Section 6.1 and 6.2: Auth explicitly invoke Greenland and its orographic role in PSC formation and phase change within the WALES lidar's sampling of a synoptic-scale PSC. While the orographic influence of Greenland has been convincingly documented in prior papers, all the evidence here (PSC observations and air mass trajectories based on reanalysis data) point to synoptic-scale drivers for the cloud and parcel temperature/height excursions. Indeed the HYSPLIT trajectory model would not be suitable for replicating orographically generated waves. It can be readily discerned from the trajectory data that auth show, and an examination of fields such as tropopause height, and even total column ozone, that the driver of cold-pool shown in Figure 4 is not local orography. In short, the orography premise has not been substantiated. What has been shown is totally consistent with synoptic-scale dynamical forcing.

Section 6.2, Page 11, Line 14: "Summarized, the trajectory analysis supports our hypotheses of ice nucleation in STS with meteoric inclusions . . ." I do not see how the trajectories of 1-3 support this hypothesis. They all evolve from T>Tnat into a realm with T<Tnat but > Tsts, then T< Tnat and Tsts, then T<Tice. Hence I do not see how auth can conclude that STS is the precursor, much less STS with an indeterminant meteoric inclusion. If I am wrong, please comment. If not, please alter this discussion suitably.

Section 7, Page 12, Line 5: Here auth discuss the sensitivity differences between CALIPSO and WALES. This was not explored herein, and should have been if this point is to me part of the conclusions. Moreover, involving CALIPSO in the case study is natural and essential. My suggestion is to redo the case with an integrated CALIPSO/WALES analysis.

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Discussion paper

Minor Concerns

Section 6.1, Page 10, Line 11-12: This statement is inconsistent with the mapped trajectories...parcel 5 does pass over Greenland ∼day 8.

Section 6.1, Page 10, Line 12: "Therefore" implies that something stated in the prior sentence(s) is the determinant for why "temperatures stay above Tnat" It's not clear what that link is or that one even exists. Please clarify.

Section 6.1, Page 10, Line 13: The PSC temperature of 5 and 6 are both within the envelope supporting both STS and NAT. However, their lidar-based compositions are clearly different. How do temperature histories help us understand why one is NAT and one is STS?

Section 6.2, Page 10, Line 24: Because auth are relating the points in Fig 5 and 7 to the histogram space, it would be essential to show the six symbols in their respective locations within Fig. 4.

Section 6.2, Page 10, Line 25: The black line in the figure is extremely difficult to see. Moreover, there are more than one black enclosures in the figure. Can the line be plotted more boldly? Should auth point out the multiple locations of the black enclosures and discuss them?

Section 6.2, Page 10, Line 27: The diamond color convention is inconsistent between Figs 5 and 7. Hence the text is confusing. Please correct the figures and make the discussion consistent.

Section 6.2, Page 10, Line 29: Again, what s the significance of Greenland? These synoptic-scale variations in height/temperature are a marker of stratospheric synoptic-scale dynamics. All the evidence that I have assembled (e.g. tropoause-height analyses, total ozone maps) indicate that the cold pool here is enhanced due to a tropospheric anticyclone forcing a bulge in stratospheric isentropes. The fact that it is near Greenland is probably inconsequential. If auth agree, please clarify the discussion. If not, please explain Greenland's influence.

Section 6.2, Page 10, Line 30: The temperature history of 4 in the time frame immediately preceding observation (i.e. within the preceding 5 days) supports a previous composition of STS as well (T<Tsts and Tnat). Hence this definitive conclusion here is not supported.

Section 6.2, Page 10, Line 34: Presumably auth are referring to Figure 1 here. If so, they should state that. But even so, Fig. 1 only shows a combination of NAT and STS, so there is no indication within this paper that the history of NAT and STS in January allows them to make this conclusion. If auth agree, please clarify the discussion.

Section 6.2, Page 11, Line 9: There is no evidence presented that orography is implicated in the dynamical signals in the T/z data. Orography by itself does not play a direct role in the lagrangian reference frame here.

Section 7, Page 11, Line 28-29: What does this discussion of large-particle sedimentation have to do with this paper's analysis or main point?

Section 7, Page 12, Line 11: This claim about a specific "branch" in the 2D histogram space is not supported herein or by other papers. I suggest removal of this statement.

Section 7, Page 12, Line 14: "NAT nucleation on ice…" That process was not discussed or examined here. However, by mentioning it, auth acknowledge the inherent weakness of the "branch" interpretation of Figure 4. I.e. the various "branches" in Figure 4 could signify phase transitions in opposite directions. This makes clear that the analysis presented herein is incomplete if the aim is to constrain "ice nucleation" as embodied in the paper's title.

Section 7, Page 12, Line 21: Here auth briefly speculate on the implications for tropical ice clouds. More recent, and arguably more relevant papers are not cited here. Chepfer, H., and V. Noel (2009), A tropical "NAT-like" belt observed from space, Geophys. Res. Lett., 36, L03813, doi:10.1029/2008GL036289. Also a comment on the above paper: Poole, L. R., M. C. Pitts, and L. W. Thomason (2009), Comment on “A tropical ‘NAT-like’ belt observed from space” by H. Chepfer and V. Noel, Geophys. Res. Lett., 36, L20803, doi:10.1029/2009GL038506. The conclusion I discern is that some of the co-authors of this paper have reservations about how to transfer PSC lessons based on the optical 2D histogram space to other realms. Please consider removing the tropical thread of this paper, if it cannot be more fully established.

References, Page 18, Line 4: “Poole, L. R., and M. C. Pitts, pers. comm.” Please give an update on this. It does not show up in AMT-D as of Jan 13 2018.

Figure 4: The dotted line needs to be much bolder. Figure 6 caption: How are MLS measurements used to justify the sloping threshold? I cannot find an explanation, and it is not self evident.

Figure 6 caption: What is “ISF”?

Figure 7: Why is a Tsts line not plotted. I think this is a natural and essential item to include here.

Additional technical concerns are shown as comment bubbles in the manuscript PDF.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-1044/acp-2017-1044-RC1-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1044, 2017.