

Interactive comment on “Polar stratospheric cloud climatology based on CALIPSO spaceborne lidar measurements from 2006–2017” by Michael C. Pitts et al.

Anonymous Referee #3

Received and published: 24 April 2018

General remarks:

This is an excellent overview paper on unique measurements of polar stratospheric clouds (PSCs) and their composition of now more than 11 years by the CALIOP lidar on-board the CALIPSO satellite. The paper discusses new improvements in the classification approach in details, introduces a new retrieval approach of particle surface and volume density information, and gives a detailed introduction in the corresponding climatology of PSC occurrence and composition. The paper is well organised and written, and the scientific objectives fit perfectly to the scope of ACP. I strongly recommend the publication of the manuscript after some minor corrections and improvements.

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Minor comments:

To my mind the manuscript is missing a short section/paragraph on comparisons with PSC measurements of other sensors. Some references on comparisons are given at various places of the paper, but I recommend to summarise the comparisons results at one specific place of the manuscript. This would better highlight the quality and reliability of the PSC detection and classification methods of the CALIOP instrument.

I would recommend to move 3.1.1. and 3.1.2 into an appendix. A reduction on technical details in section 3 would be desirable for none-expert readers.

Page 9, line 5: I am wondering that the MIPAS observations show a NAT belt on 2008-05-29 and 2008/06-01/02 but no indication on May 30. Usually the NAT belt is devolving slowly over a couple of days starting with a small area of NAT/ice activity followed by a downstream formation of a belt-like structure in the next days (e.g. Höpfner et al., 2006). Please clarify, if May 30 is really NAT-free (maybe a typo?) in the MIPAS observations. Is it possible that MIPAS just misses the small NAT area from the day before due to a sampling issue? This potential mismatch may bias your definition of the empirical sub-class of 'enhanced NAT mixtures'.

Section 3.6: The percentages of Figure 10 suggests that mainly enhanced NAT mixtures of Version 1 are classified in Version 2 as ice. Is this correct, or is the effect caused by misclassification of the former Mix-2 and Mix-2enhanced classes. Can you quantify the partitioning between the two V1 classes into the V2 ice class?

Section 3.7: The temperature difference between STS and ice in the $T - T_{ice}$ histogram for the maximum position (ΔT 1-1.5K) looks unexpectedly small to me. I would expect from the equilibrium curves, for example presented in Fig. 5 of Pitts et al. (2013), higher temperatures for STS. Can you please clarify and/or explain in more detail how you defined T_{STS} based on Carslaw et al. (1995).

Page 13, line 8: Is the 'strong' statement regarding the positive tail in the PSC distribu-

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tion (that this is due to warm biased temperatures associated with wave ice events not fully resolved in MERRA-2 fields) based on a detailed analysis or 'only' one plausible explanation. Uncertainties of the threshold lines between Ice and NAT may cause a similar tail in the distribution. Please comment and clarify.

The authors may think about to skip Figure 16, which is partly redundant to Fig. 17. For example, Fig. 17 includes by far more quantitative information than Fig. 16 due to the choice of the vertical and horizontal coordinates.

Section 6: To my mind the SAM II - CALIPSO comparison would profit by some more detailed descriptions and analyses. The information on SAM II measurements are very limited. For a profound comparison of the PSC occurrence frequencies it would be necessary to discuss the detection limits of both instruments (I guess based on extinction or volume density thresholds). The authors should discuss similarities and differences between the two sensors as well.

Technical corrections:

page 14, line 20: 'Hence, it is not ...'

p21, L7: please explain 'DMPs'

p23, L26: For completeness the authors may like/need to add a reference to the SAM II dataset as well.

page 31/32; Fig. 1/2 caption: CALIOP curtain of Fig. 2 looks on my printout and screen greenish and not yellow. Please check.

Figure 3: 'The symbols size are proportional to volume-equivalent radii of NAT and ice'. This fact is hard to see in Figure 3 and may cause the effect, that the Mix1 calculations are hiding all STS results. Is the particle size an important topic for this figure? If not, keeping the figure more simple the interpretation of the figure might be easier for the reader. Is the the particle radius also an issue in Figure 4? If yes, this is not obvious from the caption and the text passages in the corresponding section.

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Figure 4: The authors may explain the grey box in the figure caption (S/N issue) or reference to the details in the corresponding section.

Figure 18: Starting at 5% occ. freq. with the colour bar looks a bit extreme. Especially, if this is leading to the strong statement of section 4.1.2 'with essentially no STS' occurrence in the the deep vortex during September. Please clarify, if this statement is an 'artefact'.

References:

Please abbreviate the First Name of the authors (copernicus style) for the references of Wegner et al., Young et al. and Prata et al. .

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-234>, 2018.

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