

Interactive comment on “The 2009–2010 Arctic polar stratospheric cloud season: a CALIPSO perspective” by M. C. Pitts et al.

Anonymous Referee #1

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General comments: The paper presents new data of CALIPSO Lidar over the Arctic in 2006–2010, and in detail in 2009–2010. The manuscript is very well structured, the data shown are unique and very interesting, the conclusions are supported by a large data set of lidar observations taken during the polar night. The abstract is quite clear and the general policy on the issue of SI units is fulfilled. So I strongly support the publication on ACP. Some questions on the assumptions used in the automatic algorithm for PSC detection along with some suggestions about the structure of the figures and others few minor points are reported.

Scientific questions: 1) In Fig 1, 9 and 10 the negative values in dparticle for STS class is attributed by the authors to the low SNR ratio of the perpendicular channel of CALIOP. This is an old story that derives from a previous work of the authors (referred

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in the present one as P09) on the CALIPSO data of PSCs. It still seems to me that the problem is systematic and likely referable to the assumptions in the dvolume calculations (i.e. crosstalk between channels, assumption on dmolecular value...). Anyway such strange negative and abnormally large values of particle depolarization don't invalidate at all the work, being the PSC classification related to a relative more than to an absolute use of particle depolarization. However I venture to suggest to better understand the reason of the problem (i.e. comparing the data with ground based lidars with higher SNR), if in future the authors aim to use the CALIPSO depolarization in a more absolute sense. 2) Did the authors investigate the possibility to produce a composite 2-D histogram of the data set (like fig. 9) in dparticle vs color ratio, and in dparticle vs lidar ratio, if the SNR of both the green and the infrared channel allows that? In case I guess such graphs should show interesting features with regard to PSC class separation.

Suggestions: 1) On page 24216, the conclusions of the authors "The detection of NAT particles in December, prior to any observed mountain wave ice PSCs, supports ..." is too important in a scenario of NAT PSC formation still far away to be clear, and the authors could spend some more words on the question. Moreover such conclusion cannot be missed in the abstract and in the summary. 2) On page 24211 the authors refer to a 13% presence of Mix2 PSCs in the entire data set. It should help the reader having a graph (i.e. histogram, pie chart ..) showing the percentage of each class in the data set of 2006-2010. 3) Being no accordance among the Lidar community for using as color ratio the 532/1064 or the 1064/532, in Fig. 2 I suggest to add the words (1064/532) to the Y axis title. 4) In Fig.1, 2, 9 and 10, add a scale of R values on the top X axis (like in similar figures in P09), that will help the reader. 5) In the text of the manuscript and in many figures it is used indifferently dparticle and daerosol, please choose one. 6) In Fig. 11, 12, 14 and 15 please specify for the reader the time lag of the shown subsequent trajectories. 7) In fig. 13, it is better to zoom the plot to Lat >= 60°.

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