

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

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Final Response

Included below are our responses (in bold italics) to the comments from the two referees of our ACPD paper. We appreciate all of their suggestions for clarification and improvements to our paper.

Anonymous Referee 1

Scientific questions:

1) In Fig 1, 9 and 10 the negative values in d_{particle} for STS class is attributed by the authors to the low SNR ratio of the perpendicular channel of CALIOP. This is an old

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story that derives from a previous work of the authors (referred 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.

The primary cause of negative and abnormally large values of particle depolarization in the CALIOP PSC data is indeed low SNR. As stated by Hunt et al. (2009), the SNR of CALIOP is much lower than that from typical ground-based lidars because of the large distance between CALIOP and its targets and the modest power-aperture product that is dictated by weight and electrical power limitations on the spacecraft. SNR can be increased by averaging, but at the averaging scales used to produce the CALIOP PSC data the SNR is still a factor of 100-500 smaller than the SNR of reported ground-based lidar PSC observations (e.g. Massoli et al., 2006; Biele et al., 2001). When the true 532-nm perpendicular backscatter coefficient is very small, such as in STS PSCs, negative values are expected in the measured 532-nm perpendicular signal due statistical fluctuations (negative-going noise variations). This is clearly illustrated in the paper on CALIOP noise by Wu et al. (Atmos. Chem. Phys. Discuss., 10, 17263-17305, 2010) and can be seen in the example histogram (attached Figure 1) of 532-nm perpendicular backscatter coefficient values from the CALIPSO Level 1b data files for PSCs detected in the Arctic in December 2009. Negative measured values of 532-nm perpendicular backscatter lead directly to negative calculated values of both volume and aerosol depolarization. In a similar fashion, positive-

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going noise variations will produce abnormally large values of measured 532-nm perpendicular backscatter, which lead to abnormally large values of volume and aerosol depolarization. In addition, the calculation of aerosol depolarization from volume depolarization becomes unstable as the scattering ratio approaches 1.0.

With regard to the possibility of systematic errors, Hunt et al. (2009) show that the measured CALIOP clear air (molecular) depolarization ratio has varied from 0.006 to 0.009 compared to the estimated theoretical value of 0.0036. Thus, the inferred crosstalk between the two CALIOP 532-nm channels has varied between about 0.002 (0.2%) and 0.005 (0.5%) over the lifetime of the CALIPSO mission. As pointed by Cairo et al. (1999), crosstalk will result in an overestimation of volume and aerosol depolarization in regions where non-depolarizing aerosols are present; hence correcting the CALIOP PSC data for crosstalk would make the negative values of depolarization even more negative. We agree with the reviewer's suggestion that CALIPSO PSC optical parameters should be validated using much higher SNR ground-based lidar PSC data before the CALIPSO depolarization data are used in an 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.

As we wrote in Section 2 and which is described in more detail by Hunt et al. (2009), the CALIOP 1064-nm data suffer from generally low SNR in the stratosphere (even lower than the SNR of the 532-nm data), and there are uncertainties at this time in the 1064-nm calibration as well. For this reason, we have not attempted to produce 2-D histograms of aerosol depolarization vs color ratio or aerosol depolarization vs lidar ratio. We do agree that these

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graphs would provide additional information on PSC composition, and we will produce and use them if the quality of the 1064-nm data improves sufficiently in future releases.

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.

We have added an additional sentence on page 24216 to emphasize the importance of this finding and have also added mention of this in the abstract and summary.

2) On page 24211 the authors refer to a 13 percent 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.

We added a new figure (Fig. 3) showing pie charts of the breakdown of PSCs by composition in the data set of 2006-2010 for both the Arctic and Antarctic. To illustrate the variability from season to season, we also list the maximum and minimum percentages in any one season for each of the compositions. Text describing the figure was also added.

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.

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We now explicitly define the color ratio as 1064/532 in 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.

We added a scale for the R values at the top of Figs. 1, 2, 9, and 10.

5) In the text of the manuscript and in many figures it is used indifferently dparticle and daerosol, please choose one.

We now use “aerosol” depolarization exclusively in both the text and figures.

6) In Fig. 11, 12, 14 and 15 please specify for the reader the time lag of the shown subsequent trajectories.

We have more clearly defined the date and time (UTC) of each orbit shown in these figures. Figures 14 and 15 have been revised to include orbits that are separated in time by no more than 7 hours (although they may span two consecutive calendar days).

7) In fig. 13, it is better to zoom the plot to Lat ≥ 60 degrees.

Figure 13 now only shows latitudes greater than 60 N.

Anonymous Referee 2

Scientific comments:

1) In both the introduction and the conclusions much is made of the CALIPSO measurement dataset providing context for the finer-scale measurements made during the RECONCILE campaign. However, there is no mention of the RECONCILE measure-

ments in the main body of the text. While this isn't an overview paper for the RECONCILE campaign, it would be good to have a few details in the main body of text of how the RECONCILE and CALIPSO measurements fit together.

We have added several new sentences in the Introduction and Section 5 to better describe the complementary nature of the RECONCILE and CALIPSO measurements.

2) I agree with referee 1 that some more detail on the authors' conclusions about the detection of NAT particles prior to observations of mountain wave ice PSCs would be useful.

We have added an additional sentence on page 24216 to emphasize the importance of this finding and have also added mention of this in the abstract and summary.

Technical comments:

1) Page 24211, line 24: Please expand the SNR acronym.

Done.

2) Page 24226, line 13: For Powell et al, JAOT, 2009, the doi should not contain a '-'.

Corrected.

3) Fig 1, 2, 9, and 10: I agree with referee 1 that upper limits to the color bars should be added to help the reader.

We added a scale for the R values at the top of Figs. 1, 2, 9, and 10.

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4) Fig 2: The bin size of 0.054 x 0.02 given in the caption differs from that given in the text (page 24211, line 29) - should this be 0.054 or 0.05?

This should be 0.05 and has been corrected in the figure caption.

5) Fig 10: The δ characters in the y-axis labels are missing.

The δ characters in the labels are not missing in our version of the manuscript - so we're not sure what happened in Referee 2's version. We will make sure they are not missing in the final version.

6) Fig 11, 12, 14, and 15: I presume that the titles of each subplot include time, as well as date, information. However this is not very understandable, and in some cases (Fig 14 and 15) makes it appear as if you're plotting data from orbits which are separated in time by 24 hours! Please replace these titles with clearer information on the timing of these orbits.

We thank the reviewer for calling this to our attention. We have more clearly defined the date and time (UTC) of each orbit shown in these figures. Figures 14 and 15 have been revised to include orbits that are separated in time by no more than 7 hours (although they span two consecutive calendar days).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24205, 2010.

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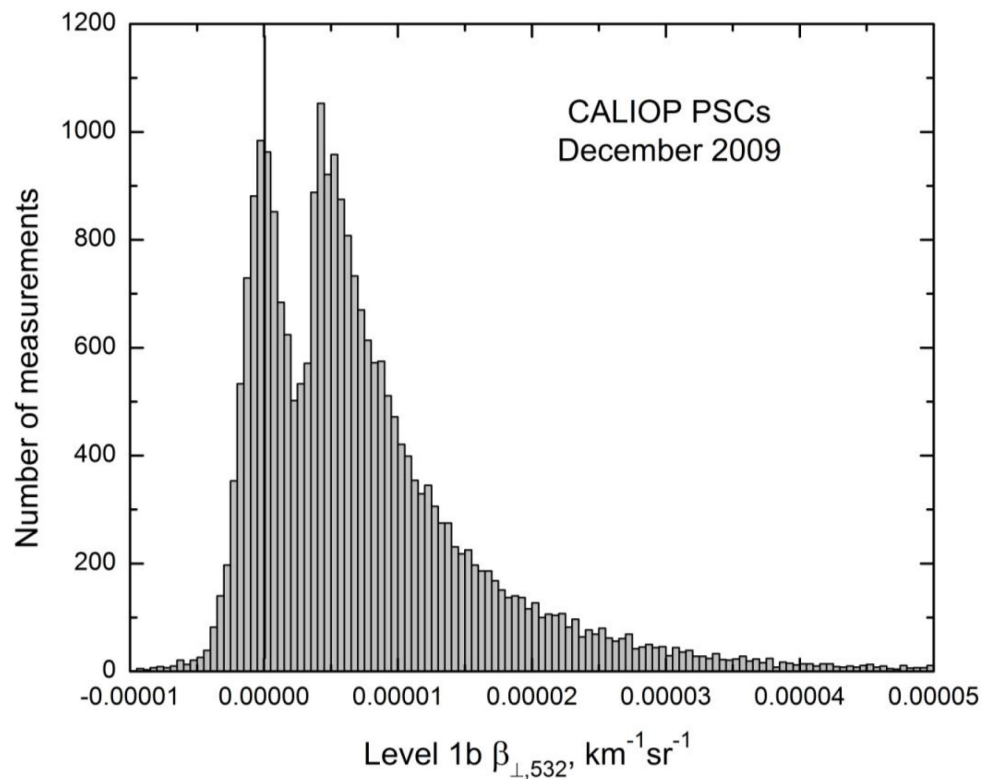
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Fig. 1. Histogram showing distribution of perpendicular backscatter values measured by CALIPSO in PSCs detected in December 2009.

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