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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Characterization of Polar Stratospheric Clouds with Space-BorneLidar: CALIPSO and the 2006 Antarctic Season" *by* M. C. Pitts et al.

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Pitts et al. (hereafter refered to as "auth") introduce us to the unique and exciting dataset of CALIPSO polar stratospheric cloud sightings. PSCs are a substantial area of long-running interest and importance, and for this reason alone this work deserves attention. Moreover, stratospheric clouds and aerosols in general are of huge relevance, and still inadequately understood. Methods to detect stratospheric clouds, discriminate them from background aerosols, and characterize them in terms of composition, formation processes, and evolution are important for research in polar as well as extra-polar regions. So this work is an important preliminary step in that direction.

This paper is expertly written and organized. For the most part the figures are of high quality, appropriate, and consistent with the aims of the paper. However, I have some concerns of a general nature with the analysis that is reported. I encourage publication when auth have considered these concerns and suggestions.

General concerns:

I understand that the paper has a substantial scope in dealing exclusively with the Antarctic. However, the polar Arctic is a region of great relevance, and there is one full PSC season already recorded by CALIPSO. Auth do not scope in the Arctic. I hope auth consider addressing this by either a referee response or perhaps even inclusion in the paper.

What is the rationale for reducing noise by horizontal averaging? Is there a known horizontal component to the instrument noise? Is the "noise" instrument-related, or geophysical variability. It would be valuable to read a fuller description of this issue, or have a citation to which to refer.

While I understand the utility of a temperature marker for the PSC detection, I am perplexed at the primary use of temperature in the algorithm, in particular using T=198K as a threshold for segmenting background aerosols. This seems like a sort of circular logic, a reversion to the early method of Poole and Pitts (1994) that more recent PSC-detection algorithms (for instance Fromm et al. 2003) sought to avoid. I would ask auth to address this concern and discuss whether this aspect of CALIPSO PSC detection is considered fundamental (i.e. necessary), or perhaps just step in the evolution to the eventual, more sophisticated algorithm that auth envision.

An additional concern is that auth use a vertical domain of 20 km (10-30 km), over which the canonical Tpsc varies on the order of 10K. Again, it strikes me as a reversion to older PSC-detection methods (to employ a single temperature value), which risks aliasing the results in altitude. An additional risk is that in the (approximately) lowest 3km of their domain, significant "contamination" by the upper troposphere is possible.



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Tropopause heights are typically below 10km, but on any given day in the polar region it is likely that anticyclones lift the tropopause above that level. Moreover, it is well established (e.g. Tuck, 1989) that tropospheric flow disturbances (in particular, anticyclones) force stratospheric cooling leading to PSC formation/intensification. It is also well observed that (especially) late-season PSCs form in the very lowermost stratosphere, just above the tropopause. For all these reasons, the choice of a fixed lower altitude limit combined with no accounting for tropospheric bulges (with concomitant increases of condensible gases, cirrus , etc) above 10 km is a weakness of this method. I would ask auth to address this issue, either in discussion form, or through a more altitude-sensitive treatment of Tpsc and tropopause height.

Considering that this is a first, standard-setting paper on CALIPSO stratospheric cloud detection, it seems both natural and important to include some comparison with other validated data giving PSC/aerosol profiles, for instance ground-based lidar. There are to my knowledge multiple aerosol lidars that have been operated in Antarctica (e.g. McMurdo). Why have auth not performed such comparisons, or if they have, not included them in this paper? In my opinion, this paper must contain a discussion (or analysis) on this topic.

Did auth do any vortex discrimination? If so, did they attempt an analysis like Figure 4 to determine if CALIPSO is sensitive to background aerosol differences in and out of the vortex? This might be a useful test, for instance, in order to evaluate the swelling of the population of backscatter ratio at low temperature they present. It also would be generally useful to assess statistically the CALIPSO response to the extra-vortex aerosol as a sort of control data set for the in-vortex studies.

7943, L8. It is not clear to me what auth mean by "forced to live with this behavior." It appears they are describing a real, natural, geophysical feature of stratospheric aerosols. It looks like a slight swelling of aerosol–whether due to a process akin to deliquescence, or something else–a small signal to which CALIPSO is sensitive. This is a good thing! Presumably the tail of the distribution–that which auth are focusing



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on for distinguishing cloud from aerosol–is still resolvable by their method. It is a challenge in all cloud studies to decide on what is meant by a cloud "edge", or cloud just forming, or cloud just before evaporating. The answer will always be elusive. Thus I am not sure what is unique about this shifting-backscatter feature that deserves the special comment quoted above. This may be an example of how a comparison of the inside-vortex and outside vortex BSR statistics could be used to assess the inherent uncertainty in precisely pegging a cloud-no-cloud boundary, and showing CALIPSO's value in characterizing these transition regions. Regardless of how auth decide do deal with this, it seems they could use this information as a quantitative constraint on the uncertainty of CALIPSO cloud detection.

Technical Concerns/comments:

7937,L18. Presumably the 333m horizontal resolution mentioned here is along track. If so, state that and give the across-track resolution.

7940, L4-8. This last sentence is out of order in this paragraph in my opinion. I suggest moving it up near the lead part of the paragraph.

7940, L8. Why 540m for vertical averaging? Why not use a round number like 500m?

7940, L11. "radiation induced" should be hyphenated.

Figure 3. Please consider remaking fig 3 in terms of BS ratio, to make it consistent with the terms of the detection algorithm. This would aid the reader by reducing the data units to assimilate.

Any plans for graduating to daytime CALIPSO? Can these be mentioned in this paper?

Are auth aware of Maturilli et al., ACP, (2005)? This paper may be especially relevant for forthcoming CALIPSO studies of PSC composition and Arctic PSCs.

7945, L20. What is NAT "haze"? The employed quotation marks by auth suggest that this needs to be either cited or described in more detail.



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7946, L20. Redundant statement in one paragraph regarding record ozone mass deficit.

7947, 26. "inner core" seems redundant.

Map figures. Give Greenwich longitude a label.

Fig 10 and 11. The color scales are different, but no color bar is shown for Fig 11.

Fig. 12. Caption needs a little more detail re. contour interval/labeling...they are difficult to read.

Fig 13. Mention SAM II in caption.

Figs w/ time series. Consider using calendar dates on axis labels

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 7933, 2007.