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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An assessment of CALIOP polar stratospheric cloud composition classification" *by* M. C. Pitts et al.

Anonymous Referee #1

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In this paper, the authors present results of their PSC classification scheme, slightly updated from the version presented in their 2011 paper, which was itself a slight revision of the original scheme presented in their 2009 paper. In addition, they compare results of this scheme, applied to CALIOP data over both poles between 2006 and 2011, to nitric acid uptake retrieved from gas-phase HNO3 observations from MLS combined with modeled stratospheric temperatures (from GEOS5). They find PSCs, identified to be of a given type based on CALIOP observations, happen in conditions conducive to the formation of said particle type.

This is a well-conducted study, that takes a sensible approach to the hard problem of validating remote PSC identification. It is well written, well organized and easy to understand. Results are presented clearly. I have strong confidence in the authors' ability to correctly analyze CALIOP data and derive reliable results – these results have by now been the subject of scrutiny for many years. But even though I have no problem with the results themselves (with small caveats – see minor comments), I find the conclusions reached by the authors to be too "positive" given the evidence presented. I don't intend to cast any doubt on the authors' approach and analysis, as the classification technique devised by the authors seems to me to have the most well-researched theoretical basis, and to be the most documented of its kind given the amount of available CALIOP data. However, given this context, I feel the paper would be stronger if it tried to frame its results more towards an assessment of the reliability and limitations of lidar-based PSC classification, instead of simply a validation of a single technique. I will recommend the paper for publication once the paper is slightly modified to present a perhaps more nuanced view as discussed below.

Major comments

As introduced above, my main concern with this paper is the conclusions it reaches. The authors write in the abstract that their "classification scheme was found to be excellent in an overall sense", a point that is also made in the conclusion (Sect. 5). In the discussion (Sect. 4), they state that their results "provide compelling evidence that [their] composition classification is robust". Having read the paper, my own impression is that its results cannot lead to such conclusions, but instead provide a useful assessment of unavoidable limitations of lidar-based PSC classification.

First, as far as I know, we still don't have a good vision of PSC formation mechanisms, especially for NAT crystals. Given that, it seems to me that it's actually not possible to certify a given classification mechanism "works" : we don't really know what we should classify PSC as. For instance, the P09 classification scheme considers five PSC categories (ice, mix2-enh, mix2, mix1, STS), but this number and the categories were really chosen based on the dependence of optical properties seen by lidar on the nature of PSC particles. It is not impossible that PSCs of different nature could produce similar optical signatures. In the other direction, the choice to divide "NAT mix" PSC in "mix1", "mix2", and "mix2-enh" seems rather arbitrary – are these classes really representative of distinct PSC categories, affected by different mechanisms, found in different regions or conditions, etc? My point is that while it is possible to evaluate if a classification scheme works as intended, that does not necessarily make it excellent.

My second point is that all the evidence presented here for the evaluation of the classification 1) are circumstancial, and 2) do not necessarily support the classification. The evidence the authors rely the most on are the nitric acid uptake diagrams (Fig. 4-5) and the temperature histograms (Fig. 8-10). Regarding the former, the three figures of nitric uptake in NAT mix PSC (Fig. 5b-d) show a rather large number of points aligned with the STS equilibrium curve (Sect. 3), almost half of the points based on a visual estimation. On page 24653, the authors state that “at first glance, the mixture points... may appear to be misclassified STS”. Later, they state that “However, this points represent... mixtures... [with] low number density NAT particles... [and] much more numerous liquid droplets”. My impression would be that a PSC composed of a low number of NAT particles and "much more numerous" STS droplets really should be classified as STS. The classification might be working perfectly from an “optical signature” point of view, but can we actually conclude anything on the actual PSC composition, if PSC classified as NAT mix can contain either a majority of NAT or STS particles? I came from this figure with a conclusion opposed to that of the authors – that the optical properties of a PSC are insufficient to infer its composition. I expand on this in the minor comments, suggesting ways to turn this into useful results. Second are the temperature distributions of Fig. 9 and 10, which are presented by the authors as "compelling evidence" for the robustness of the classification, a point which they do not justify by any discussion. I agree that each curve seems to peak at the temperatures where we would expect to find the most particles of the expected type, but there is a *lot* of overlap between the temperature ranges covered by all curves. Almost all curves range between Tice and Tice-4K, and peak 1-2K apart (except ice). Given this strong overlap and the proximity of the peaks, for all we know a third of each PSC class could be misclassified and it would produce the same results. Since the NAT mix

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figures 5b-5d (discussed above) suggest such misclassification does happen often, I do not find these histograms as compelling as the authors. Moreover, this is not necessarily something that can be “fixed” by a “better” classification algorithm - temperature overlap might be unavoidable.

Based on these comments, I don’t think the evidence presented in the paper can sustain the level of confidence displayed by the authors in the examples at the top of this section, and I think they should rewrite these sentences to be less conclusive. Unless I am mistaken in my interpretation of Fig. 5b-d, I would suggest to the authors to include a discussion of the points above as well as quantify their effects (see minor comment 24653.20-25 below). Again, I don’t think the classification scheme is bad – actually quite the contrary. I think the paper would be stronger if the authors tried to acknowledge the limitations of the classification and document them. Given the classification scheme is currently state-of-the-art, it would provide a useful reference describing the limits of PSC classification based on optical properties alone.

Minor comments

24645.15: I personally agree with the authors that it is important to better understand the composition of PSCs. Out of curiosity, are the authors aware of any reason to believe that our current understanding of PSC composition somehow impedes our ability to predict the future evolution of polar stratospheric ozone?

24648.06: Could you provide a reference for the number densities and sizes of ice PSC particles presented here?

24649: MLS retrievals of gas phase HNO3 are deemed unreliable, and are flagged as such, when tropospheric clouds are detected. In absence of evidence to the contrary, I would expect the MLS profiles of gas phase HNO3 to be also affected somehow by the presence of PSC, perhaps making them unreliable. Is there any reason/referenced evidence to believe this is not the case?

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24649: The horizontal resolutions of MLS and CALIOP data (400-550km vs 5km) mean that a single MLS profile can be used for up to ∼110 CALIOP profiles. PSC composition can change quite a bit in so many CALIOP profiles. I understand that surrounding MLS profiles were weighted according to the distance to the CALIOP profile, but the inter-profile data variability can't be created out of nothing. I find it quite surprising that this difference in resolution has no impact on the quality of the colocation. It would help if the authors could, for instance, find a way to document the variability in PSC classes across 500 km sections of CALIOP data.

24652.29: Here the authors attribute PSC misclassifications to the presence of "negative noise excursions in the observed perpendicular backscatter signal". Why are negative values considered valid for the classification? I can imagine that these excursions happen at a resolution finer than the one used for the classification, leading to biased low average values. If that's the case, the averaged points are probably below the threshold of a reliable signal-to-noise ratio. In the presented classification scheme, is there any filtering done to remove data with low SNR?

24653.20-25: Regarding the "NAT mix PSC actually contain a majority of STS" problem (major comment). Could you quantify the number of PSCs that follow each branch (STS and NAT equilibrium) in Fig. 5b-5d? Perhaps by counting the number of points on each side of a third curve equidistant of the STS and NAT branches? Even better, could you suggest STS and NAT concentrations (in particle number or mass) for these mixture case? Such values would be useful as they would document how the relative proportion of NAT vs. STS affect the optical properties of a given PSC and its classification in similar schemes.

24657: If I understand correctly, you say the ice arm shifts to the left because there is less HNO3 available for condensation (due to denitrification etc). I don't understand why the availability of HNO3 would limit the growth of ice particles in ice PSC. Are you suggesting that all ice particles require HNO3 to grow in size? Probably not.

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24657: I don't understand from the text how did the cloud population change going from Fig. 12a to Fig. 12b. Is Fig. 12b supposed to be documenting the presence of small NAT crystals? Why are there no more STS clouds in this Figure compared to Fig. 12a ? (i.e. there are no more non-depolarizing particles). I would have expected denitrification to lead to less NAT and more STS, but the figure suggests the opposite. Please enlighten me.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 24643, 2012.