We thank the reviewer for their comments. We include here a point by point response in blue.

We draw the reviewer's attention to the fact that, following a similar comment of all reviewers, a figure 1 has been added to the manuscript, featuring information on the operation statistics of DDU lidar. Therefore, all figure numbers have been incremented accordingly as compared to the first version of the manuscript.
Also, to address another comment, the threshold temperatures $T_{NAT}$, $T_{STS}$ and $T_{ICE}$ are now computed based on the closest MLS $H_2O$ and $HNO_3$ concentration measurement. Although this does not change their meaning or their interpretation, most of the figures have gone through some slight changes.

General comment 1: The article is missing a good documentation of the ground-based lidar dataset it is built upon. What is the period of observation covered? How frequent, how long are observation periods? Are specific months (JJAS?) selected, and the rest ignored? Are there annual/seasonal/hourly changes in operation and sampling? How do the sampling coverage and statistics compare to those of CALIOP? This alone could explain differences in ground-based vs spaceborne retrievals.
Following similar comments from other reviewers, the manuscript now includes more detailed information of the operation statistics of DDU lidar. The newly added figure 1 (shown below) presents the number of measurement days per year, from 2007 to 2020, as well as the average monthly distribution of these measurements and their duration.

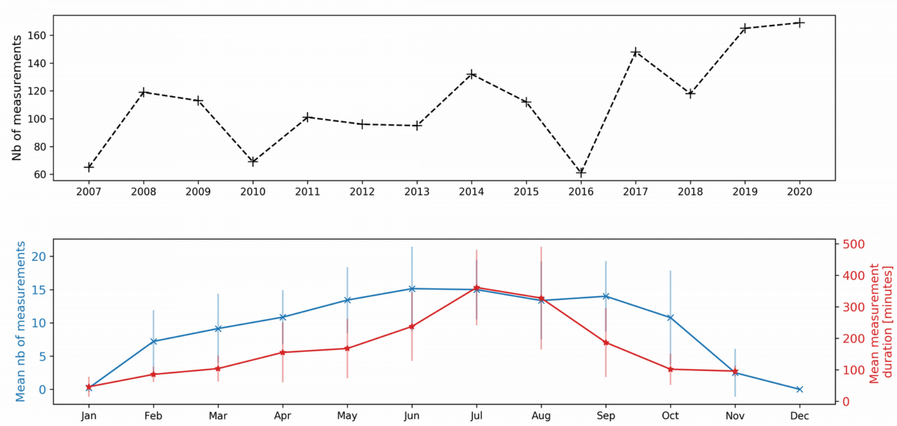


Figure 1: Operation statistics of DDU lidar. (a) Number of measurement days per year, from 2007 and to 2020 and (b) mean duration of measurement sessions per month, in minutes, from 2007 to 2020.

Apart from exceptional PSC detection in October, PSC are not expected outside the months of JJAS. Concerning the types distribution of figure 3 and the trend of figure 9, only the months of June to September (included) were considered. The first reason is to avoid "false" PSC detection to due to aerosol layers: as Tencé et al. (2022) discussed it, aerosol layers injected in the stratosphere by volcanic eruptions or wildfires can present overlapping optical properties with PSCs. The second reason is that this JJAS restriction is also used by Snels et al. (2021) when comparing groundbased and spaceborne lidar measurements, and we mimic this approach to enable the comparison of our results. The restriction to JJAS for the types distribution of Figure 3 was not explicitly mentioned in the manuscript, it is now added line **312**: "To make the comparison valid, we restricted our analysis to the months of June, July, August and September."

The manuscript originally included a mistake we made interpreting in Tesche et al. (2021). To address this issue and following the reviewers' suggestion, we included in this revised version a

comparison of lidar DDU PSC detections to CALIOP PSC measurements around DDU, from 2007 to 2020.
Please refer to the new figure **3d** and associated analysis. CALIOP is now introduced in section **2.2**, along with its PSC detection method in section **3.2**.

Here is the new version of figure 3:

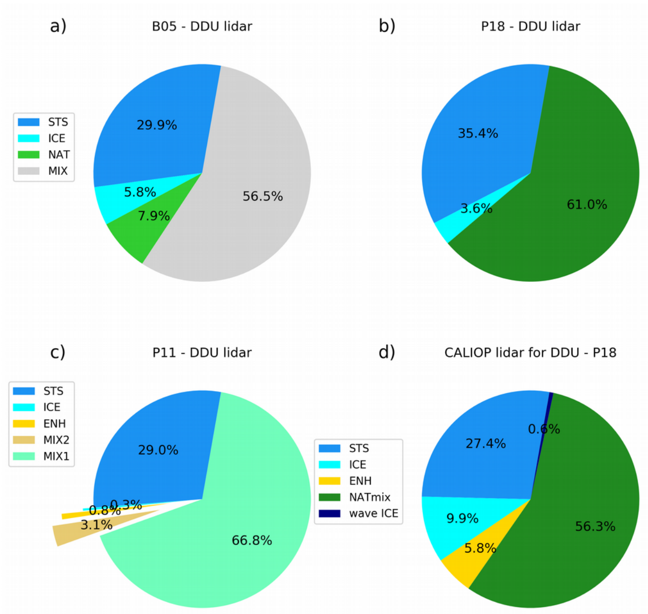


Figure 3: PSC types distribution observed at DDU for the three considered classifications: B05 (a), P18 (b), P11 (c) and observed by CALIOP extracted around DDU using P18 scheme (d).

General comment 2: The text in most figures is small enough to be sometimes illegible. It would be better if most figures were displayed full-width, but the text would probably remain too small. This is particularly true for figures 1, 2, 3, 4, 7 and 8, in which number axes are extremely small. See the image in the supplement that shows some article text (up left) and a bit of figure 1. Please fix this and make text readable in figures.
General comment about the figure size and quality: this point was also raised by another reviewer. The size of the figures is set by the ACP LaTeX template (12 cm for 2-columns figures, and 8 cm for 1-column figures). They are now larger in the revised manuscript.

**Other comments:**

1) l. 17 (and others): here you refer to PSCs as "stacks of layers". My impression is that it is a very lidar-centric view. PSCs are 3-dimensional structures, as such they can be viewed as stacks of layers, but they could also be viewed as columns of vertical slices, arrays of cubes, etc. I'm not sure what this particular way of describing 3-dimensional structures brings to the table. Please clarify: is there something in the nature of PSC formation and dynamics that leads to a structuration of overlapping, horizontally-consistent slabs? (note this is definitely not true for wave PSCs)
It is a very interesting comment and this layered way of describing PSC clouds is actually lidar-centric ; on the edges of the polar vortex, air masses often exhibit numerous filamentary structures mainly fitting by isentropic layers. Maybe the location of the station warps our description to some extent. For clarity purposes and not to shift the discussion on the geometrical discussion, we chose to remove the phrasing.

Edited line **18**: "... often observed as layers featuring different chemical compositions."

2) l. 46: Later... (Larsen, 2000). Please check the chronology of your paragraph here
The later was referring to the early classifications in 1988 and in the 1990s, but it is indeed not clear with the two previous sentences. To fix the chronology, we removed the "Later," line **55**.

3) l. 63: "different set"
Corrected, edited "different sets" line **72**

4) l. 65: "we decided to consider 3 different classifications proposed by Blum, Pitts and an updated version of P11 is also considered" -- please fix phrasing: the updated version of P11 either is one of the three different classifications, OR is also considered, but not both.
Corrected, "is also considered" was removed line **76**

5) l. 65: "Following their conclusions": whose conclusions? Achtert and Tesche 2014 are quite far away, please clarify.
It is indeed referring to Achtert and Tesche (2014), the manuscript was edited to make it explicit: "Following the conclusions of Achtert and Tesche (2014)" line **74**

6) l. 68-79: here only sections 1 to 3 are mentioned. Please include all sections. The lidar instrument is actually presented in Section 2, not section 1 as the text says. Processing and schemes are described in section 3 (not 2), etc.
The whole paragraph was rewritten as recommended by another reviewer. It now lists the sections more clearly – and without mistakes, see line **86-94**.

7) l. 83: "(NDACC"
Corrected, parenthesis closed line **98**

8) l. 96 and elsewhere: it looks like you've chosen to use "scattering" where I would have expected "backscattering". Is there a reason for this? Could you clarify in the text that this is your meaning?
It is indeed referring to backscattering, the confusion probably comes from the fact the "Scattering Ratio" is often used to refer to the backscattering ratio. I adapted the manuscript to explicitly refer to backscattering, edited line **113**: "backscattering"

9) l. 96: you defined the backscatter/scattering ratio profiles as the ratio of total scattering to molecular scattering. Is any of those two attenuated? Please be explicit.
None of them are actually attenuated, it is the purpose of what is referred to as lidar data processing or inversion, i.e. getting the backscatter coefficient without. Attenuated backscatter ratio is only used as an approximation to check on the clear sky or an aerosol/cloud signature status of a profile. Actually, the word "attenuated" in this case was a misleading so we replaced it. The preprocessing approximation is to compute a backscatter ratio "not corrected for extinction" (new wording), meaning we consider in the lidar equation the particulate extinction coefficient to be 0. Otherwise, the actual lidar inversion procedures ensures correction of the Mie extinction. The manuscript was edited to remove the two references to "attenuated" and to mention this non-corrected for extinction backscattering ratio line **130-31** and **190**: "... non-corrected for extinction backscattering ratio"

10) l. 101: "data is" plural
Corrected, "data are" line **133**

11) l. 110: "Each instruments"
Correct, plural removed line **151**

12) l. 137: "NCEP reanalysis product is the result of a cooperation between NCEP and NCAR": this info is already provided on lines 127-128.
The repetitive sentence was removed line **182**

13) l. 151: I don't think beta_tot_perp has been defined here yet. I'm guessing that each of the three groups [R_T, R_//] etc is used by a different classification scheme. Please make that explicit.
The three groups are indeed referring to the three classification scheme, the text was edited to make it more explicit: “for each classification scheme“ line **197**
beta_tot_perp was indeed not introduced before. However, following the comment of another reviewer, the definition of the variables is now moved to section 2.1, beta_tot_perp is now defined beforehand.

14) l. 165: thanks for the very interesting reference to Behrendt and Nakamura, 2002. I could not find the 0.443% in the text of the article itself, could you expand a bit on how you obtained it? i.e. what temperature or other input parameters you've selected?
The 0,443% parameter is in the table 2 of Behrendt and Nakamura, 2002. We kept the value given for T = 240K, considering with Table 2 and Figure 5 of this paper that the impact of temperature of the molecular depolarization could be neglected for our application.
Since the data is taken directly from the mentioned reference, we do not add any further information in the updated version of our manuscript.

15) l. 169: this was already stated line 151
It was indeed repetitive and the sentence was removed.

16) l. 171: "PSC classification is challenging as described in the introduction but critical": weird phrasing. Please rephrase as e.g., "As described in the introduction, PSC classification is challenging. It is, however, critical..."
Rephrased as suggested by the reviewer “As described in the introduction, PSC classification is challenging. It is, however, critical” line **225**

17) l. 176: "Achtert and Tesche..." the same sentence is already more or less present on page 3
It was indeed stated before in the article, and was therefore rephrased as follows, lines **230-232**: "As mentioned in introduction, the classifications B05 and P11 are considered here following the conclusion of Achtert et al. (2014), to which we had P18, the update of P11 published in 2018."

18) l. 199: MX1, MX2
Corrected, “MIX1 and MIX2” line **250**

19) l. 211: It is unclear to me why you consider P11 in addition to P18. Isn't P18 supposed to supersede the P11 algorithm? Are there reasons why anyone who would like today to study PSCs using CALIOP measurements should go for the P11 algorithm? Version 2 of the CALIPSO PSC product is totally based on P18, so anyone who would like to study PSCs using CALIOP measurements is stuck with P18 anyway (unless she's willing to process the classification herself). Could you clarify what is the point of including P11 in the comparison?
We consider P11 as it is involved in the analysis and conclusions of Achtert et al. (2014), stating B05 and P11 are comparable when applied to the Esrange lidar database with satisfying result. When applying P18 to our groundbased dataset (formerly using P11), we had to adapt the classification so comparing the outcomes of B05, P11 and P18 at DDU sounds consistent to us. It is also a way to validate the adjustment of P18 thresholds to our groundbased setup.
Moreover, as P11 and P18 as well as their intercomparison in Pitts et al. (2018) are based on CALIOP measurements, we consider the additional use of a groundbased dataset interesting.
Finally, the optical properties used in P11 and P18 are different, so it is interesting to us to have

both.

In addition, investigations on PSC with CALIOP data do not necessarily have to be done through P18. CALIOP provides the optical lidar properties with backscattering ratio, backscattering coefficient, aerosol depolarization among other variables (i.e. level 2 products), on which the classifications rely in the end. For the convenience of the scientific community and especially to make an easier link to the model community, the CALIOP scientific team also provides a PSC Mask product relying on P18 and we included in this revised version of the manuscript analysis using this level 3 product.

20) l. 237: "features"
Corrected line **285** “feature”

21) l. 242-244: unclear, what are you planning to do with those mixed-phased clouds? Are you going to make them appear as a separate entity, or subsume them in the category of the dominant particle type, or something else?
In the paragraph mentioned by the reviewer, we try to highlight that the "MIX" clouds defined by classifications correspond to different things i.e. physical/chemical reality. In B05, MIX is defined as any cloud not corresponding to the three other types. In P11, "MIX1" and "MIX2" are defined as different kind of NAT mixtures. Finally, in P18 these two types are merged in a "NAT mixtures" category. It is important that the reader keep in mind that "MIX" is not a tag referring to identical things across the different classifications.

For the purpose of our study, when comparing the PSC types distribution resulting from different classification, we merge the categories referring to NAT clouds and mixed phase clouds to put a common ground for comparison.

22) l. 248: "A distribution of PSC types... published in Tesche et al was included": How did you get the numbers from Tesche et al. 2021? As far as I can tell, the article itself did not include numerical values for its retrievals, so did you lift numbers from the figures? If so, it is surprising you can reach precisions like 15.8%.
The exact numerical values were provided on request by Matthias Tesche directly, this should have been made explicit.

23) l. 270-274: From what you write here I understand that ice PSC are under-represented in DDU lidar PSC observations. If that is indeed what you meant, could you please spell it out explicitly? This actually could be checked (relatively) easily -- in each CALIOP profile one could see for a given PSC type the frequency of opaque tropospheric clouds underneath. According to your explanation, opaque tropospheric clouds should be relatively more frequent in presence of ice PSC than in presence of other PSC types. If you think this is outside the scope of the present paper, perhaps mention it as a possible perspective.
It is outside the scope of this paper but we find it is a very interesting and somewhat necessary perspective. As mentioned in the paper, Adhikari et al. (2010) and Achtert et al. (2012) already explored the correlation of ICE PSC occurrences and tropospheric cloudiness, and it could be interesting to relate this study at DDU especially considering that, as Tesche et al. (2021) highlighted it, DDU experiences a higher level of tropospheric cloud cover hindering its spaceborne validation capabilities.
It is somehow mentioned as perspective in the conclusion, but we decided to strenghten this point .
Edited line **543**:

"... Investigating this correlation is an interesting perspective of this work."

Following the comment of another reviewer, and as CALIOP PSC detection around DDU are now included within this paper, we considered ICE PSC occurrences detected by CALIOP above DDU and crosschecked them against the DDU lidar operation on the same days. In most cases, the groundbased lidar was not operated on these days. The manuscript now includes the following comment, lines **335-338:**

"Between 2007 and 2020, CALIOP detected ICE PSC above DDU on 19 different days, out of which 4 correspond to DDU measurements, suggesting a possible important tropospheric cover or bad weather condition hindering operations. However, we do not consider this small sample robust enough to support the analysis."

24) l. 272: "Marginal" According to your discussion, CALIOP results should be closer to the correct number of ice PSC, and they report a frequency of 16% for ice PSC. Is that marginal?
As stated in a previous comment, there was a mistake in our understanding of Tesche et al. (2021), only based on the winters of 2012 and 2015 as far as Antarctica is concerned. 2015 is a specific year where high ICE occurrences were observed. The updated version of figure 3d features a PSC type distribution based on CALIOP measurements above DDU from 2007 to 2020. It presents an ICE proportion of approximately 10%.
While this 10% share seem not marginal, Pitts et al. (2018) publishing a PSC type spatial distribution where, at DDU location, ICE are not expected to be observed frequently.

25) l. 301: It would be interesting to apply the various classification schemes on the entirety of the CALIOP observations, and indentify in what geographical regions the results diverge. This is clearly outside the scope of the current paper.
The reviewer points out a very interesting study, and we agree that it is not the scope of our paper focused on DDU lidar observations.

26) The discussion of the comparison suggests to me that outputs of classification should come with some kind of reliability indicator, that would decrease as the measured optical parameters get closer to category boundaries. Such an indicator would improve comparisons and make inconsistencies between retrievals perhaps less significant. Is something similar already present in any product? If you think this is a good idea, you could take the opportunity to suggest it in your paper.
That would be an interesting feature for future classifications. Accounting for the uncertainties on PSC class transitions is actually difficult due to complex nature of optical properties modelling from solid particles, but still could be done to some extent. CALIOP metadata actually provide reliability or confidence indices on the PSC mask product.

From the CALIOP website: "These indices provide information on the statistical confidence in the assigned composition based on a data point's location within the optical space. Indices are reported as the distance (in number of standard deviations) between the point and the relevant boundaries of its composition class, with larger numbers indicating higher confidence in the assigned composition."

While these confidence indices are of great use on the massive raw volume of data available through the CALIOP measurements at the continental scale, it remains difficult to set them to practice on the smaller scale of our groundbased dataset of CALIOP overpasses above DDU.

27) l. 327: "This high variability must be kept in mind": why?
The phrasing recalls that classifications tend to present a very global point of view and represent PSC measurements as single points on a plan ([$R_T$, $R_{//}$], [$R_\perp$, $R_{//}$] or [$R_T$, $\beta_{tot,\perp}$] in the schemes

presented here) whereas fine scale measurements as shown in figure 6a highlight the high temporal variability of PSC fields. When using classification outcomes, one should keep in mind the way these values are obtained and the set of parameters controlling their variability (for instance, time integration of lidar measurements, resolution, smoothing, etc).

28) l. 328: "horizontal smoothing... due to the transport" the transport of what? Please clarify.
"Horizontal smoothing due to the transport" implicitly refers to the averaging caused by the integration time and the stratospheric transport during this time window. Maybe rephrasing it makes it clearer, lines **414-415:** "the trade-off on the integration time between SNR and information loss caused by the averaging of potential varying atmospheric scenes due to the air masses transport"
It refers to the fact that, if the atmospheric scene changes during the integration time window (i.e. presence or absence of a cloud for example), the associated integrated optical properties will be representative of a smoothed version of the changes.

29) l. 333: the type changes throughout the whole day, not just once at 5PM. But your point stands.
It is right, I just wanted to point out a specific type change. The manuscript was edited to rather point to the multiple type changes of the PSC layer around 20 km. Line **419**, "around 5PM" was simply removed.

30) l 334: Related to my previous point about a type reliability indicator, do the optical parameters of this cloud hover near the boundary between two categories in the classification diagram? Would an indicator help identify this situation and flag it as unreliable?
We think the subsequent discussion after line **420** provide hints on the reliability of the type change and addresses the point of the reviewer.
Using a reliability index such as the one provided by CALIOP, the type identified for the PSC sample of this figure would have been flagged as less reliable due to the proximity of its optical properties to the boundaries of the NATmix and ICE types in P18.

31) l. 339-341: Could you specify if, in your opinion, these changes in composition (derived from the changes in optical properties) are consistent with the speed of the deposition and growth processes that would drive the change in composition? In other words, are the changes in composition trustworthy, or are they a demonstration of the limitations of the optical classification approach?
The reliability of the composition changes and associated change in the optical properties underlies the question of PSC state at the time it interacts with lidar beam, this being finally related to thermodynamical equilibrium and kinetics of composition changes. As pointed out earlier in the review, this could be considered a lidar centric issue. This comment is really relevant to us : even if this point actually reflects some limitation in the building of classification schemes using optical properties, the only way to circumvent this is to consider the uncertainties as a whole.
Such an advanced classification would need to combine confidence indices such as the ones provided by CALIOP to accurate uncertainty assessment accounting for both lidar signal and air mass thermal history, as PSC formation and composition strongly relies on this parameter.

32) l.354: Here by "lidar" you imply an HSR-capable lidar. Please clarify.
Elastic lidar inversion always requires knowledge of the temperature and pressure to derive molecular scattering. The HSRL technique takes advantage of the spectral distribution of the lidar return signal to discriminate aerosol and molecular signals and thereby measure aerosol extinction and backscatter independently. Our system is not HSRL ready and we derive molecular scattering from external temperature/pressure dataset. Since the DDU lidar is already referenced, we choose not to add any extra information.

33) Figure 5: Here the labels are quite readable, but the decision to make the figure wide and short makes it very hard to identify any structure visually (especially in Figure 5a). Could you please reorganize the figure to change its aspect ratio somehow? Maybe make it a 3-columns/1-row full-width figure?
Addressed in general comment 2 and we are fully aware of this since prepublication, all the figures have been made bigger as the problem was coming from the ACP LaTeX template guidelines. We hope the updated version of the manuscript is more readable now.

34) l. 366-367: "To investigate the effect of temperature variation on PSC..." do you mean "the impact of the choice of temperature dataset on the results of PSC classification"?
We agree on the need to rephrase the relevant lines, a word seems to be missing here making the sentence pretty unclear. Rephrased to “In order to use the most adapted temperature dataset to process our PSC measurements at DDU, we compare several ones in Fig. 8, from reanalysis to satellite observations.” lines **443-445**

35) Figure 8: I'm sorry but I don't understand what is being shown here. As I understand it, the figure shows three numbers : A) the number of days in which the lidar observed a PSC (red triangles), B) the number of days in which the ERA5/NCEP temperature allowed PSC formation (green/red crosses), and C) the number of days in which ERA5/NCEP temperatures were 2K below the TNAT formation threshold, AND no lidar measurements were available (grey arrows). In my view, "the number of days in which the ERA5/NCEP temperature allowed PSC formation" is the same as "the number of days in which ERA5/NCEP temperature were 2K below the TNAT formation threshold". In that case, A+C should be equal to B. This is clearly not the case in the figure, so I must have misunderstood something, but I can't find elements in the text to clarify my misunderstanding. Please help.
We note that the stratospheric denitrification is now taken into account in the $T_{NAT}$ computation, and it tends to decrease $T_{NAT}$ values. As a result, the ΔT criteria adjusted to our lidar measurements is now -1 K and not -2 K as it was the case in the initial version of the manuscript. Figure 9 and the associated discussion have been edited on lines **490** and **492** and in the caption of Figure 9.

The new version of Figure 9 is shown below:

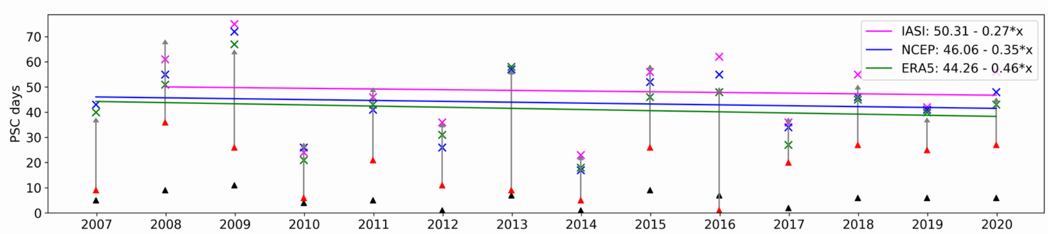


Figure 9: PSC days per year at DDU from 2007 to 2020 featuring PSC detection with the lidar in red triangles. Potential PSC days per year estimated by ERA5, NCEP and IASI based on the lidar measurements are shown in green and red respectively. Green, blue and fuchsia lines represent the corresponding trends. Grey arrows indicate the number of days per year where the T - $T_{NAT}$ < -1 K criterion was satisfied and DDU lidar was not operated.

Figure 9 is indeed showing these three parameters: number of days with a lidar PSC detection at DDU (red triangles), number of days where T-$T_{NAT}$ < -1K is reached with ERA5 and NCEP (green and blue crosses), and the number of days where this criterion is reached with ERA5 and NCEP but no lidar measurements was available.

We provide more information on the methodology used to build this trend as follows:This temperature proxy for PSC is adopted to keep the use of the temperature threshold Tnat to predict

PSC formation. As mentionned in the litterature (Dye et al., 1992 for example), PSCs usually form a few degrees below $T_{NAT}$, so that using $T-T_{NAT} < 0K$ as a criterion leads to an expected PSC overestimation.

The groundbased dataset provides the number of days where the lidar was operated as well as the number of days where PSC were detected at DDU.

Independently, we calculated the number of days where the threshold $T-T_{NAT}$ < Delta was reached on the lidar operating days, spanning the Delta range -10K to 0K. We then used this delta as criterion to match the number of PSC days detected by the lidar to the number of predicted PSC days. In other words, the delta variable is used as control variable between two criteria: the one of our lidar observations and the one, using $T_{NAT}$, of the model renalyses. But the trend is in essence built from PSC detection using lidar measurements.

To answer the reviewer's question, the sum of red triangles and grey arrows does not necessarily add up to the green / blue crosses count. First, the method is designed so that, when only computed for the dates where the lidar was operated, the green / blue crosses are as close as possible to the red triangles. But the distance between both datasets (crosses and triangles) was of course optimized for the 14 years, not individually for each year. Crosses and triangles would have been exactly equals should we have computed a specific Delta value for each year, which would render the trend meaningless.
From a more qualitative point of view, it is also expected that triangles + arrows do not equal the cross values. Sometimes, $T-T_{NAT} < -1K$ is reached but no PSC is formed: the temperature is not a sufficient condition for PSC formation. On the other hand, some days we detect a PSC but ERA5 or NCEP state that $T-T_{NAT}>-1K$: this is for example the case if a PSC is formed following a subscale cooling temperature not resolved within the models.

Finally, please keep in mind that this method, i.e. the adjustement of a criterion to derive a number of PSC days per year, is in essence a statistical approach and not a theoretical formation criterion calculation. It is expected that the year to year variability makes the criterion over or underestimated because it is set as the best match considering the 14-year dataset as a whole.

Please note that this figure is edited according to other reviewers' comments. It now includes the same trend computation based on IASI temperature measurements (marked as fuchsia crosses and line), as well as the number of PSC days detected by CALIOP above DDU as black triangles.

36) l. 408: the negative trend that is found here mostly depends on the reliability of ERA5 and NCEP stratospheric temperatures, and on the presence of a overall stratospheric temperature trend in those datasets, correct? Could you make it clearer why your results are not just confirming the presence of a warming trend in ERA5/NCEP stratospheric temperatures? i.e. what is the lidar bringing here?
In light of the details on the previous comments, we hope the role and added value of the lidar measurements in the trend is made clear by now. To address the second point, the trend shown here is of course connected to temperature trends, but not equivalent. There is a difference between a trend of mean temperatures, and a trend of overpassing a given threshold. Figure 9 is the latter one. We could have a stratospheric warming trend that does not necessarily impact the extreme values, and vice versa. As for PSC formation, the occurrences of extreme temperatures under a given threshold remain the critical factor, highlighted in Figure 9. So we consider that the trend of Figure 9 is not just confirming a warming trend, it is connected to it but it brings a somewhat different

information.

37) l. 445-448:I understand from your conclusions that 1) applying the three classifications schemes to ground-based lidar observations leads to results that agree quite well, and 2) applying the same classification scheme (P18) to ground-based and spaceborne lidar leads to results that agree well too. From this, I understand that the choice of classification scheme has after all little importance on the results. Do you share that opinion? If not, could you amend your conclusions to include arguments for the opposite viewpoint?
The overall agreement on using different classifications considering one of them is built from an arctic dataset (B05) and the other from a spaceborne dataset (P11, P18), i.e. built on a different scale and relying on different optical variables, should be seen as a successful characterization of complex optical patterns using different observational geometries, carrying different uncertainties. We somewhat share the opinion stated in the reviewer's comment, but it is worth considering the scale of the dataset on which the classification is applied. On this decadal scale, an overall agreement is found. For process studies, and especially considering aerosol plumes, particle characterization may be more tricky as optical properties can be closer to the boundaries of any given scheme, leading to, on the smaller scale, inaccurate characterization.

The choice of the classification may also depend on the instrumental setup. The different classifications rely on different variables and some of these variables are directly accessible while some have to be undirectly computed, this leading to larger uncertainties. We aim at providing the community with a DDU dataset carrying its own features and highlights, with a dedicated set of optical properties. Considering our low number of ICE PSC detection, the three classifications overall show a very good mutual agreement.

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