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Interactive comment

Interactive comment on "The MIPAS/Envisat climatology (2002–2012) of polar stratospheric cloud (PSC) volume density profiles" *by* Michael Höpfner et al.

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Many thanks to Michael Fromm for his thorough review helping to improve the manuscript substantially.

Our answers are given below. The original referee comment is repeated in **bold**, changes in the manuscript text are printed in *italic*.

Note: My report includes the pdf of the manuscript annotated with comment bubbles identifying technical issues.

All comments and suggestions identified in the pdf have been addressed/incorporated

Printer-friendly version

Discussion paper

in the revised manuscript.

H18 extend the foundational work of Spang et al. (2018) by exploiting the entire MIPAS archive to retrieve PSC volume density (VD). H18 describe the prior MIPAS works on PSC detection and typing, propose to derive VD, a useful quantity for polar processing and chemistry applications, and then show an overview of PSC VD data as a "climatology." H18 is well organized. The section on the retrieval development is clear and thorough. Their retrieval method will be beneficial to the AMT audience.

To the extent that this work's new contribution is in the VD approach, algorithm, and verification with respect to independent PSC data, this manuscript is well targeted to AMT. The work goes on to present some summary PSC VD patterns in the 11-year MIPAS era. Even though the climatology aspect of this work is considerable, and perhaps a better fit for another journal such as ACP, the balance H18 struck between algorithm and applied science permits me to conclude that this is an acceptable candidate for AMT.

That being said, H18 need to motivate the effort they put in to deriving PSC VD. The paper's introduction does a nice job of framing the state of MIPAS PSC developments but does not offer a science reason for the creation of a PSC VD data set. If H18 revise the Introduction to make a compelling case for the value of PSC VD over and above PSC occurrence and composition (already completed by Spang et al. (2018). That would provide important motivation to justify the analysis H18 present. Besides that, there are a number of minor and technical issues the authors will need to address before this work can be considered ready for publication in AMT.

We agree that the introduction does not contain much information about the motivation for a specific VD retrieval (in fact, the information is spread in the later sections). Below, under the specific comments, we explain how the new manuscript will be revised accordingly.

Below I list these concerns. In addition, the manuscript has been annotated with comment bubbles identifying specific, technical, and/or grammatical items needing attention. It is provided as part of this report.

Introduction. H18 briefly mention the apparent weakness of prior approaches with limb sounding of IR radiance "...without consideration of the fact that each raypath of the observation intersects multiple altitude levels, leading to an intertwined retrieval problem..." but do not explicitly state how their approach overcomes this weakness. Perhaps this is well articulated in latter portions of the paper, but I was not able to find it. The Introduction needs a statement as to how this is dealt with for the benefit of the science quotient of the new PSC VD data set.

Yes, the adopted solution to this problem did not become clear here. What we mean is that, due to the limb geometry, all the previous methods related to occurrence and composition used to investigate MIPAS limb data up to now are mostly valid for the highest PSC-layers and become more and more uncertain at lower altitudes. That is, because in those methods each limb-view is analyzed separately. In case of the presented VD retrieval, we use the information from all limb-views of a whole limb-scan together (like in trace-gas retrievals). Thus, also the lower layers of a PSC should be described better by the VD profiles.

The text will be appended accordingly:

In the present work we tackle this problem by adopting a complete altitude-resolved inversion of all limb views simultaneously. This means that, like in the case of standard trace gas retrievals, a global fit approach is used to derive altitude profiles of PSC volume densities (e.g. Höpfner et al., 2006b).

Introduction: Presumably there is added value to the science community to have

PSC data expressed in terms of VD. But the reader is not given the argument for this or a literature background on this topic. It would be essential for h18 to make that argument in order to motivate this work.

For a better motivation, we have extended the introduction by:

Beyond the PSC existence and composition, which is already available from MIPAS (Spang et al., 2018), volume density is an independent quantitative parameter which can be used for validation and analysis of atmospheric model results. For example, by comparison with MIPAS data on volume density, Khosrawi et al. (2018) could show that their global model simulates PSC existence well but underestimates strongly the PSC mass which might influence vertical redistribution of HNO_3*.*

Figure 1. Three areas of clarification are needed. 1. The plots have up to 3 different lines, solid black, presumably VD; solid orange, presumably median radius; and black dotted line. The caption describes the dotted line as a "first mode" (presumably in size units, which are scaled in orange). I don't see any solid lines of any color that indicate "the second mode." Either more lines are being described than shown, or the descriptions themselves need to be revised. 2. How is one to interpret the ice PSC plots where the VD=0 and median radius is >0. Doesn't VD=0 indicate no PSC? Or does it just indicate no ice? Some explanation would help. 3. The 19920127 STS plotshows a very large median radius. Why is that classified as STS?

1) We agree that the explanation in the Figure caption should be improved, as well as the visibility of the lines in the plots – they all contain four lines. To improve the latter, we have strongly increase the line thickness. The caption now reads:

Example profiles from the in-situ balloon database on PSCs used as input for the radiative transfer model. The database contains parameters of bi-modal log-normal distributions derived from the particle counter measurements. Here the median radius (top orange axis) and the total particle volume density (VD, bottom black axis) of each *mode are shown. Dotted lines indicate the first mode with smaller particles and solid lines the second mode. The title indicates the date of the balloon observation and the predominant composition of the PSCs (MIX is a mixture of similar volume densities of STS and NAT).*

2) In case of the ICE-PSCs at higher altitudes, the volume densities of the second mode are in the order of 0.1 $\mu m^3/cm^3$ while the radius is about 1-2 μm. Thus, there have been particles of that size, but very few, such that the resulting volume is small.

3) It shows a large median radius of the 2nd mode (up to 4.5 μm), however with a low volume density (about 0.3 $\mu m^3/cm^3$) compared to the first mode (up to 4 $\mu m^3/cm^3$). So there might be a few large particles (e.g. NAT), but the volume is dominated by the small particles.

P15, L5-6. H18 make the point that the MIPAS data shown in Figure 7 are unique because measures much closer to the pole than any other satellite PSC data set. This point is well taken, but the onset date they show is not notably different than Antarctic onset dates recorded by instruments farther from the pole (e.g. CALIPSO, SAM II, POAM II, III). The advantage of MIPAS is that it provides this uniquely nearpole coverage throughout the season, in both hemispheres (as Spang et al. 2018 point out). Perhaps H18 might consider enhancing/refining the discussion here?

In this paragraph, we intended to present an interesting example of the new MIPAS PSC dataset. In the southern hemisphere, we believe that the first period of PSC evolution is extremely relevant due to the nucleation of NAT, which is still an unresolved problem (e.g. Hoyle et al., 2013), and which leads to the subsequent denitrification of the stratosphere. To make this clearer we have extended the text as:

“As an example for the coverage of single profiles, Fig. 7 shows the retrieved PSC volume densities at 20 km altitude in mid-May 2010. One can clearly observe the onset of PSCs evolution right in the center close to the South Pole. *The appearance and* *mass of PSCs over Antarctica in May can deliver valuable information on the nucleation process of NAT particles, which are relevant for denitrification (e.g. Lambert et al., 2016). For example, in Lambert et al. (2016, Tab. 2) the reported on-set date as derived from CALIOP is 22-May while according to MIPAS observations (Fig. 7) first PSCs appear 5–6 days earlier.* During several of the years observed by MIPAS, the first PSCs are detected in this region during very similar times. These observations are unique since no other instrument has observed PSCs during their formation so far south *(Spang et al., 2018)*."

Abstract: Related to the point made above, H18 make a statement in the abstract about "this climatology captures this onset . . ." However, isn't it the more general MIPAS PSC data set and climatology (previously reported) that gets the credit for this rather than the specific VD climatology? If in fact the newly developed PSC VD data set shines a unique light on this, the paper needs to make clear how the VD data set expands the constraints to higher latitudes and different times than the MIPAS PSC-detection affords.

We agree that the detection of the PSC on-set alone has already been captured by the previous MIPAS datasets. Thus, we have changed the related sentence in the abstract to:

Among other interesting features, this climatology helps to study quantitatively the on-set of PSC formation very near to the South Pole and the large variability of the PSC volume densities between different Arctic stratospheric winters.

P15, L11 (and Figure 8). "single enhanced values are visible" The panels of Figure 8 are very small and there are white bars competing with the VD color scale, making the features called out difficult to discern. Perhaps H18 could elaborate on what they mean by the quote. Perhaps also they could provide a single, expanded panel with the feature of note pointed out. Finally, it is not evident to me that PSC features above 27 km must be artificial. PSCs have been observed

higher than 27 km regularly (CALIPSO curtains show this to be true). Hence I'd ask H18 to consider enhancing the visualization of this feature, describing it more clearly, and discussing whether it might also be evidence of real PSCs in addition to the "side-lobe" feature they identify.

We have expanded the related explanation in the text accordingly. In addition, we have added two figures (see Figures A5 and A6 in the supplement to this comment) in the appendix with MIPAS-CALIOP co-incident comparisons showing the effect of retrieval instabilities compared to high-cloud observations.

In the plots of the southern hemisphere (bottom two rows of Fig. 8) at altitudes of 28 km and 26 km, bands of enhanced values are visible during mid-winter. These often appear as side-lobes in the retrieved profile when optically thick ice clouds are present, as can be observed in co-incident observations of CALIOP and MIPAS (Fig. A5). In comparison, high-altitude PSCs are mostly not confined to a single retrieval level, visible in Fig. A6. The instabilities could be suppressed by increasing the regularization strength, however, at the expense of a deterioration of the vertical resolution. We have, thus, decided not to change the constraint, but to point at these potential outliers.

P13, L19-20. "Here, both MIPAS retrievals and the CALIOP dataset often indicate much smaller values" Much smaller than what? Please clarify.

We have changed the text to better clarify this point:

Here, both, MIPAS retrievals and the CALIOP dataset often indicate much smaller values of volume density compared to the calculations under the assumption of thermodynamic equilibrium.

Please also note the supplement to this comment: https://www.atmos-meas-tech-discuss.net/amt-2018-163/amt-2018-163-RC2-supplement.pdf

As mentioned above, all comments in the supplement have been addressed in the new version of the manuscript.

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